Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-1	11	0	0	0	0	IPCC WG1's mandate is to provide a policy-relevant assessment of the worldwide scientific literature on climate change. The AR5 will consist of 14 chapters. If each of the chapters has an average length as the SOD of chapter 11 (currently 129 pages), the report will end up with nearly 2000 pages. I would say in that case the IPCC report will have failed at its mission to provide a summary of the scientific findings in a form that can be relevant to anybody outside the scientific community itself. It is hard to imagine who (politicians, stake holder, the interested public?) will ever going to seriously read a 2000 page assessment report. I think we should aim for an overall length of Chapter 11 not exceeding 40 pages which corresponds to a 2/3 cut of the currentl length. There is a lot of detail given in the text and plots that I don't feel is necessary for an assessment report at all. Instead, more references to the relevant literature could be made, see more comments below. [Antje Weisheimer, United Kingdom]	The text has been cut by approximately 15%. The ES has been cut substantially.
11-2	11	0	0	0	0	Although there has been some improvement on this aspect compared to previous drafts, I still feel the split between the near-term predictions and projections is a bit confusing to the reader and not well enough explained or motivated. [Antje Weisheimer, United Kingdom]	We have spent a great deal of time considering such issues. The fact of the matter is that Ch 11 is dealing with a wide range of complicated issues. We have broken the text up into what we see as a logical order, we have a Box to explain the terminology. Further simplifications have been made - please see latest Box.
11-3	11	0	0	0	0	Fig 11.1a: It is not clear whether this is supposed to be a schematic demonstrating general ideas or whether this is supposed to show actual results from CMIP-5 runs (if so, which runs are used?). The quality of the plot is poor with the thin lines hardly to be seen. The distinction between a "simulation" and a "forecast" is very strange because a model forecast is also a simulation with a model. [Antje Weisheimer, United Kingdom]	It is a schematic although it is a result from a particular model and, since it is meant to be a schematic the particular model is not emphasized so as not to divert attention to model details etc. The figure is meant to illustrate several concepts which seem to be difficult to otherwise convey namely: forced and internally generated components, uncertainty in the evolution of simulations, projections and forecasts (and the difference between them) as evidenced by the different "lines". The figure has been modified and is, I hope, now clearer. The comment suggests adding a schematic figure and this is now added.
11-4	11	0	0	0	0	Fig 11.1b: Why has the forced probability density function such a large spread? And why is the spread constant over time? Do we really think that the two distributions converge to one identical distribution? How is model error reflected in this schematic? [Antje Weisheimer, United Kingdom]	The spread of the forced probability density reflects the natural variability around the forced component that we attempt to illustrate in Fig 11.1a. This schematic is meant to illustrate the idealized case and, since the spread does not appear to change dramatically over a decade or two we avoid either increasing or decreasing it in the diagram. We do know the two distributions become at least indistinguishable as in results reported for the NCAR model predictability experiments.
11-5	11	0	0	0	0	Fig 11.2: What data/variable/model is this plot based on? What is "local" correlation skill score? I don't think this figure is very suitable for Box 11.1 because it shows lines which refer to initialised and uninitialised runs which have not really been explained before. Also the overall message of what this figure is supposed to say is not all that clearly explained in the text. [Antje Weisheimer, United Kingdom]	The reference is now given and the caption expanded to point out that the "uninitialized" results refer to the "simulations" and the "initialized" to the forecasts thus, we believe, connecting the results to the several ideas the Box attempts to convey.
11-6	11	0	0	0	0	Fig 11.3: This figure is little informative, confusing and unclear. The top panel has too many lines that are not well enough explained. The axis of the bottom panel plot is unclear, the whole figure would need much more explanation (but I'm not sure it would be worth it). The text refers only very briefly to the figure. Please either simplify or delete. [Antje Weisheimer, United Kingdom]	This figure no longer appears.
11-7	11	0	0	0	0	Fig 5: Although I can see the motivation and aspiration of this plot, I don't know whether it is all that useful after	We believe this figure is very important. It has been

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						all. The spread among the models is so big (> 2degree) that it dominates the overall impression. [Antje Weisheimer, United Kingdom]	redrawn to respond to several comments. The purpose of this figure is to illustrate both model drift and systematic error. These are important features of climate predictions, and at the same time show how anomalies are obtained. Besides, it is one of the few figures in the chapter that shows the relative size of the anomalies to be predicted with respect to the systematic error of the different models.
11-8	11	0	0	0	0	Fig 11.10a: too much information not well explained and not enough structured. What are we supposed to see in that figures? The maps are very noisy for a simple message. [Antje Weisheimer, United Kingdom]	This figure has been enormously simplified and now shows only information about the relative size of the spread with respect to the error.
11-9	11	0	0	0	0	Fig. 11.10b: Where is the observed anomaly plotted? [Antje Weisheimer, United Kingdom]	Figure 11.10b is now gone.
11-10	11	0	0	0	0	Fig 11.10c: I can't see the unstippled regions [Antje Weisheimer, United Kingdom]	Figure 11.10c is now gone.
11-11	11	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Done
11-12	11	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Done.
11-13	11	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Done.
11-14	11	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Done
11-15	11	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Done
11-16	11	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Done
11-17	11	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Done
11-18	11	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter	Yes we have coordinated closely with Ch 11 and Annex II

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						assessment. [Thomas Stocker/ WGI TSU, Switzerland]	
11-19	11	0				Overall the chapter 11 conveys a large amount of information and draft 2 represents an improvement with respect to draft 1. However the chapter still lacks of homogeneity. The section related to the climate predictions is (still) very short compared to the one with climate projections (and this despite the noticeable length of the chapter). In particular there is an unbalance between the use of deterministic scores and probabilistic ones. It could be added (for example) a figure with the BSS averaged over a representative region. This figure could complement the Attributes Diagrams shown in figure 8. [Susanna Corti, Italy]	Thank you. Chapter has now been reviewed with homogeneity in mind. Chapter is now improved on this front.
11-20	11	0				I want also to insists on the importance of information given in BOX 11.2 and relative figure. This COX should be placed before the discussion of near-term projections. [Susanna Corti, Italy]	Reveiwer seems to be referring to Box 11.1. This Box is first cited in Introduction (11.1), i.e. before section on projections (11.3).
11-21	11	0				Overall a good chapter, with lots of information on tmescales which are probably more relevant for policy than those typically considered in previous IPCC reports. The prediction section is new and assesses the literature well. [Nathan Gillett, Canada]	Thank you. No response necessary
11-22	11	0				Several places in the chapter projections are derived from ranges of model simulations. In the discussion in a few places, the authors allude to the fact that the model range might not be representative of the observations e.g. on pg 27, ln 14-17 the authors say that the 5-95% range is only a crude uncertainty range for the observations, and that it takes no account of model quality, and that there is no guarantee that the observations must lie within this range. On page 52, ln 47, the CMIP5 spread is described as an 'ad-hoc measure of uncertainty', and the text again reiterates that the real world might follow a path outside of this range. But nowhere in the chapter do the authors discuss under what conditions the real world would be expected to lie within the 5-95% range of the CMIP5 models 90% of the time. If the authors first explain under what assumptions this is true, then they can more clearly discuss which of these assumptions do not hold in certain cases, and therefore explain why they assess that the true uncertainty range is different. In fact the multi-model ensemble spread is interpretable in this way under the 'indistinguishable' paradigm, in which climate models and the real world are drawn from the same distribution, and in which uncertainties converage to a value related to the width of the distribution of model simulations as the number of models increases (Annan and Hargreaves, 2010). Annan and Hargreaves (2010) find that this is generally a good assumption for projections made using CMIP3 models. An alternate hypothesis is the truth + error model in which each climate model corresponds to the real world plus a random error term. Under this paradigm there are no systematic errors in climate models, and uncertainties will tend to zero as the number of models averaged increases. The IPCC Good Practise Guidance Paper on Assessing and Combining Multi Model Projections provides a good resource here and should be cited and discussed. The first recommendation of this report is '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 beingused or assumed.' Annan, J. D. and J. C. Hargreaves (2010), Reliability of the CMIP3 ensemble, Geophys. Res. Lett., 37, L02703, doi:10.1029/2009GL041994. Knutti, R., G. Abramowitz, M. Collins, V. Eyring, P.J. Gleckler, B. Hewitson, and L. Mearns, 2010: Good Practice Guidance Paper on Assessing and Combining Multi Model Climate Projections. In: Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Assessing and Combining Multi Model Climate Projections [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, and P.M. Midgley (eds.)]. IPCC Working Group I Technical Support Unit, University of Bern, Bern, Switzerland. [Nathan Gillett, Canada]	Noted. Whilst we don't disagree with the broad points made by the reviewer, the idea that climate models and the real world might be drawn from some notional common distribution is a rather philosophical point which we don't believe it is helpful to discuss in the chapter. The key point, on which we agree, and which we make clear, is that the raw model range provides only a crude measure of uncertainty and therefore it is necessary to take into account other sources of evidence when making overall assessments (e.g. 11.3.6.3). We have discussed our response to this comment with a Review Editor (Francis Zwiers).
11-23	11	0				When referring to climate variations due only to processes internal to the climate system, the authors should use the phrase 'internal variability' (see glossary definition of 'Climate variability'). 'Natural variability' is not the same, since it includes variability forced by natural forcings including volcanic aerosol and solar irradiance variations. 'Natural internal variability' is not incorrect, but I think it's confusing, since it could be mis-interpreted as 'natural variability'. 'Natural variability' and 'natural internal variability' are used many times in the chapter to refer to internal variability. [Nathan Gillett, Canada]	we agree that internal variability is natrual variability that is due only to processes internal to the climate system is called 'internal variability' We know that natural variability is a broader group. However, sometimes it is clearer to use "natural internal variabiltiy". It depends on the context.
11-24	11	0				Chapter 11 in particular requires a detailed proof read throughout. There are numerous errors - grammatical, spelling, wrong choice of words etc. - which can be detailed if required. [Government of Australia]	done

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11-25	11	0				This chapter on near-term climate change is very useful for both testing the knowledge integrated in the different models and for environmental decision planning. Comments on models processes are those already made for chapter 9. [European Union]	Discussion of processes here pertains to sources of preditcabillity. So while processes have been discussed previously, ch 11 needs to clarify those relaevant predictability.
11-26	11	0				In many places of the whole chapter 11 the term "forecast" is used in connection with climate prediction. Normally it not is not an usual term in the context of climate change, its especially used in context of weatherforecast. The use of the term "forecast" in the context of climate projections in chapter 11 is new and should be explained at a dominat place and furthermore it should be taken up in the Annex III Glossary. [Government of Germany]	The term "forecast" is used interchangeably with "prediction". Please note that Ch 11 does not use either term when referring to "projections". The difference between forecast or prediction on the one hand, and projection on the other, is very carefully explained. See e.g. Box 11.1.
11-27	11	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	sent to leads authors for SPM
11-28	11	0				The flavor of decadal part vs. the near term projection past is very different. While the former reads like a science discourse, the latter is much more focused on describing changes in climate features as they pertain to the evolution of near time climate. Please consider ways in which this may be taken into account in order to improve the text. [Government of United States of America]	Text has been improved in this regard. Please note, however, that contrasts arise because we have to provide more technical infromation to adequately explain predictions and quality assessment as this is a new area for IPCC reports. This material needs to be presented - even though it contrasts with the rest of Ch 11 - because it has not been presented previously wnd will be confusing to readers if its not.
11-29	11	0				There is an absence of information on near-term climate change projections and predictability of regional drought which will have large impacts on both water and food supply. Given the emphasis in the chapter to near-term projections and predictability on evaporation, heavy precipitation events, specific humidity, and poleward expansion of Hadley Cell circulation, will there be impacts on future drought severity, duration, or frequency. The IPCC AR5 WG1 intended to inform policy makers should provide some guidance on near-term regional drought risk. Are there any climate science insights on the impacts of near-term warming on future drought severity, duration, or frequency? [Government of United States of America]	In response to this comment, we have (i) added additional panels to the former Figs 11.16-18 (SOD numbers), where we now also present E-P. In addition, we discuss droughts in an additional paragraph. The difficulty here is that the overwhelming majority of studies did merely address the far-term, and few only considered the near-term. We don't think that there is sufficient evidence for a substantial near-term assessment of droughts.
11-30	11	0				In a general sense we found that Chapter 11 was ambitiously trying to include a lot of important but fairly difficult and disparate material, and it likely needs more work than some of the other chapters in the report. [Government of United States of America]	Noted. Very substantial additional work was completed. See e.g. responses to the other comments. Over 1300 received and addressed.
11-31	11	0				The Decadal Prediction section could be better organized. The authors do a nice job in bringing in results from experiments earlier than CMIP5, but often the text just seems to be a skimming of the literature, with not as clear an organization as for the Near Term Projection section. [Government of United States of America]	Section has been completely reorganized and streamlined.
11-32	11	0				One disappointing aspect is the lack of mention of future predictions, even if for just a few years in the future. Only some predictions from a group of experiments outside of CMIP5 are mentioned, and then only in terms of a comparison of initialized with non-initialized predictions. In several places, the authors allude to statistical predictions without providing any details, including how well they do. They are not included in any of the figures. For many years, statistical predictions were more skilful than dynamical predictions on seasonal timescales. It would be surprising if that were the case for long time scales, except perhaps for the forced response of global temperature. If they don't do as well, it would be worthwhile stating so, and why. [Government of United States of America]	Future predictions are shown in Fig. 11.11 and contribute to the assessment in Fig. 11.27.
11-33	11	0				It is difficult to get a sense of the state of the science from reading this chapter. [Government of United States of America]	The numerous improvements we have made (see responses to other comments) make this much clearer than the SOD.

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11-34	11	0				figures 11.1 and 11.10 Reference period is not 1986-2005. Why is the reference period not consistent with the rest of the report. An explanation as to why not would be useful. [Government of United States of America]	Fig 11.1 now uses standard ref period.
11-35	11	0				There is an absence of scientific guidance on near-term climate change projections and predictability of regional flooding and of regional coastal inundation. Can the IPCC AR5 chapter on near-term climate change, projections and predictability, provide any insight into either an increase or decrease in the risk of regional flooding and coastal inundation in the next few decades? [Government of United States of America]	Such issues go beyond remit of WG1.
11-36	11	0				Authors has started very well with this chapter by first describing the differences between projection and prediction which is always a numbver of people do confuse. [Government of United Republic of Tanzania]	Thank you. No further comment required.
11-37	11	0				Figures 11.7, 11.9, 11.10a need explanatory text on the figure itself [Ed Hawkins, United Kingdom]	These figures have been simplified and redrawn. They have titles now and a fully descriptive figure caption.
11-38	11	0				Figure 11.8 has no source – Corti et al? [Ed Hawkins, United Kingdom]	All figures now have a source. It is Corti et al in the case of this figure.
11-39	11	0				Figure 11.33 – was not the latest version submitted to the Chapter team. Needs updating. [Ed Hawkins, United Kingdom]	latest version now included
11-40	11	0				References to Hawkins & Sutton are often incorrect. There are two different papers both cited as 2009, but only one is in the reference list. Needs to be 2009a and 2009b. [Ed Hawkins, United Kingdom]	corrected
11-41	11	0				I was asked to review Section 11.3 (Near-Term Projections) and Box 11.1 (Climate Prediction, Projection and Predictability), which I have done. I would have reviewed the rest of the chapter but I'm afraid that I ran out of time before the site closed. [Fyfe John, Canada]	Thank you for the reviews you were able to provide. No further response required.
11-42	11	0				I would suggest providing references for all figures used in the chapter? [Daniela Matei, Germany]	When references are available these are cited in text
11-43	11	0				I generally recommend to cite the sources of all figures. [Holger Pohlmann, Germany]	When references are available these are cited in text
11-44	11	0				The decadal prediction experiments cover a period for much of which the ocean observing system was inadequate. Indeed, even global mean upper ocean heat content has important uncertainties. The recent development of ARGO has transformed the situation. The relationship between the skill that can be obtained with past data and the skill that can be obtained today is unclear. This chapter should make it clear that the assessment available is very largely of skill obtainable in the pre-ARGO era. The main place to do this is in Section 11.2.3, but I feel that some text should also be inserted in the Executive summary. I make some suggestions below; other wording would be possible too. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Noted. Disscussion of the vargaries in prediction skill associated with observing systems now briefly discussed.
11-45	11	1	1	1	1	This chapter refers many times throughout the whole chapter to a non-existing section 11.4. This is a serious omission. This has consequences for the SPM conclusion (SPM-14, lines 19-24). See also our comments for chapter 12, page 37, line 14. [Government of Netherlands]	Corrected.
11-46	11	1	1	7	50	The Summary is heavily weighted towards the near-term projection results (nearly 5 pages) whereas little emphasis is put on the new area of initialised near-term climate prediction (1 page). I think these two aspects need to be more balanced. [Antje Weisheimer, United Kingdom]	The ES has been overhauled. The balance reflects the assessments made and the priorities we assign to them. The "newness" of predictons was considered in the setting of the priorities.
11-47	11	1	12	1	16	In the same way as in other chapters add also for the contributing autorths the country of origin [Government of Germany]	Such referencing is in accord with IPCC policy and not a Chapter responsibility.
11-48	11	1		129		Very interesting chapter and a good read. Could be more balanced in places, and there seems a certain enthusiasm for ENSEMBLES. The reporting of uncertainties could also be a bit more homogeneous, some findings more confident than I would expect. A major uncertainty is effect on nearterm if the aerosol predictions are underestimated for Asia and China. Could this be discussed a bit clearer? [Gabriele Hegerl, United Kingdom]	This has been a major focus in discussions since the SOD among the chapter authors. This has been given a great deal of close attention. Text has been inclduded to deal with this issue very carefully. Please see ES and relevant Sections of main text.
11-49	11	1		200		17. This paragraph refers to the entire Chapter 11. Chapter 11 reviews some of the published information on	The climate models used in for projections are

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the topic "Near-term Climate Change: Projections and Predictability". These projections and predictions are based exclusively on the same IPCC climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitute a fraud. [Igor Khmelinskii, Portugal]	assessed in Ch 9. The hindcast skill of prediction systems is described in the chapter. Models, being models, are indeed imperfect, but nevertheless exhibit skill in reproducing past climate, and hindcast skill is evident in some variables. Despite their imperfections they are the best available tools for projections.
11-50	11	1				I really liked Chapter 11! Need to check cross-referencing to incorrect sections in Chapter 8. [Joanna Haigh, United Kingdom]	Done
11-51	11	2	1	1	20	The introduction of the Executive Summary mainly focusses on the projection part of this chapter. I feel more emphasis should be put on the near-term prediction aspect. [Antje Weisheimer, United Kingdom]	The ES has been overhauled. The balance reflects the assessments made and the priorities we assign to them. The "newness" of predictons was considered in the setting of the priorities.
11-52	11	2	1	7	49	The ES is far too long [Gunnar Myhre, Norway]	The ES has been substantially reduced.
11-53	11	2	1	7	49	This ES is very long ! [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The ES has been substantially reduced.
11-54	11	2	1			Executive Summary: The style of this ES is somewhat different from the other chapters. I think it is useful to have the two paragraphs on "Predictability, Prediction and Projection" and "Predictability" (the 2nd and 3rd paras on page 2) but the first para could perhaps be shortened. [Jan Fuglestvedt, Norway]	The ES has been substantially reduced.
11-55	11	2	1			Executive summary: Currently the ES is very long, and should be condensed to a more useful length. In particular the lengthy introductory chapeau is not required for an ES. [Thomas Stocker/ WGI TSU, Switzerland]	The ES has been substantially reduced.
11-56	11	2	3	2	3	We suggest not to open executive summary with a question which reads more in the style of FAQ type language [Thomas Stocker/ WGI TSU, Switzerland]	Question has been deleted.
11-57	11	2	4	2	28	It is noted that in the same chapter on the same page the term "near-term" is used with different interpretation (future decades up to mid-century versus the next several years, up to a decade. This is rather confusing indeed and onyl one interpretation should be used throughout the whole report. [Klaus Radunsky, Austria]	The broad definition adopted alows us to assess scientific literature for predictions and predictability which examine multi-yr-decadal, the period we have agreed to assess for projections after cross-chapter discussion (2016-2035) and literature on air quality and atmospheric compositon out to mid-century (and beyond). We also assess hindcasts from CMIP5 and other sources and the hindcasts cover different periods again. This definition therefore allows Ch 11 to assess literature that is pertinent to the IPCC WG1 report, but is beyond the scope of Ch 12.
11-58	11	2	8	2	9	In this chapter near-term Climate Change is defined as "future decades up to mid-century". But Atmospheric composition, chemistry & Air quality will be assessed in this chapter until 2100. This is inconsistent with the definition [Government of Germany]	ES has been reworded to address this point.
11-59	11	2	8	2	10	It is stated clearly that non-CO2 gases are considered through to 2100 in this chapter, but it should also be ensured that this is made clear to readers of Chapter 12. [European Union]	Ch 12 CLAs have confirmed they will do this.
11-60	11	2	19	2	19	the same as for line 8 to 9! [Government of Germany]	ES has been reworded to address this point.
11-61	11	2	22	2	22	"Predictability" depends on successful prediction over the complete range intended to be covered, to a satisfactory level pf accuracy. No attempt to carry out this procedure is to be found anywhere in this Chapter, so there is no reason to suppose that any of its conclusions are worth consideration.as opinions on pedictability. [Vincent Gray, New Zealand]	"Predictability" has a special defintion as outlined in the chapter. The reviewer seems to be referring instead to to predictive skill. Predictive skill is estimated using hindcast skill. Hindcast skill is assessed in the chapter.
11-62	11	2	22	2	36	At a certain point predictions become projections, and the differences discussed here are unnecessary and confusing. How should the reader assess the differences in the following text between projection and	We agree that the difference between predictions and projections is not fundamental. A point along these

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						prediction, and why are they treated differently. If you think the initialised predictions are inferior to the projections in some way, such as insufficient ensemble members or large drifts, then be up front about how new these efforts are. [Noel Keenlyside, Norway]	lines is made in FAQ 11.2 for example. However, the production of predictions and projections in CMIP5 involves a different group of models and different technical processes are applied. Furthermore, there is distinct literature on the two topics and the impact of intialisation as it is conducted in current predictions is an important topic not considered in projections literature. Distinguishing between predictions and projections therefore makes sense for a scientific assessment of this type.
11-63	11	2	24	2	35	Suggest adding a sentence in here of the form "Some natural variations in climate will never be predictable in a deterministic sense as the arise from the chaotic nature of the atmospheric circulation, for example." I think we need to manage expectations. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	We broadly agree with this statement although this paragraph no longer appears.
11-64	11	2	24	2	36	I have not found a clear definition of the timescales discussed in this chapter. While there seems to be clear terminology (decadal/near-term predictions - yr 1-10 lead time, and near-term projection - focus on years 2016-2035), I have not found a prominent statement describing this difference (either in Box 11.1 or FAQ 11.1 or in the present paragraph). [European Union]	In the Box we distinguish between predictions and projections based on initialization or its lack and information sought (i.e. the explicit prediction of the furture evolution of the system) but do not distinguish between them based on timescale. Would prefer to leave the explicit timescale comment to the Sections. In any case this paragraph no longer appears.
11-65	11	2	28	2	28	The sentence "using models which are initialized with observation-based information" can be revised to be "using models which are initialized with observation-based information or using dynamical and statistical combined approaches". [Jianqi Sun, China]	Although we accept that this is a correct statement this paragraph no longer appears.
11-66	11	2	29	2	30	What does "only over several decades mean"? We suggest deleting this part. Further, if "climate projection" is defined as the response to external forcing, it could be done for any time-horizon or time-mean. [Government of United States of America]	Yes . However, this paragraph no longer appears.
11-67	11	2	31	2	33	This definition of predictability is a philosophical concept, of little use in climate sciences. Indeed, it cannot be evaluated or measured. 'Perfect model' experiments do not measure intrinsic predictability, but the spread of a given model. A definition of 'model predictability' or 'model family predictability' might be much more useful. [Michel Déqué, France]	The ES is now considerably rewritten. We would argue that predictability is a well defined concept for a physical system although we certainly agree that in most cases it can only be estimated using models of such a system as is mentioned. However, this paragraph no longer appears.
11-68	11	2	31			It is not commonly agreed that "predictability is an intrinsic property of the climate system." [Government of United States of America]	While the term "predictability" is often misused (e.g. as a synonym for forecast skill) we do believe that it is properly characterized as an intrinsic property of the climate system. However, this paragraph no longer appears.
11-69	11	2	32	2	33	Either delete or define "under ideal circumstances." [Government of United States of America]	The paragraph no longer appears
11-70	11	2	33	2	33	Either delete or define "For a particular case" [Government of United States of America]	The paragraph no longer appears
11-71	11	2	41			What does "medium amount of evidence and agreement" mean? Also in line 45 [Antje Weisheimer, United Kingdom]	This statement no longer appears
11-72	11	2	43	2	45	Discussion of predictability associated with internal and forced components states that predictability associated with the forced component increases with time. This is poorly worded. I believe that the authors mean that the the contribution of the forced component to predictability increases in time. The same wording is used on page 13. [Government of United States of America]	Agreed. Although this statement no longer appears here, this is certainly better wording and we use it elsewhere.
11-73	11	2	46	2	48	For extratropics there is specific differentiation between ocean and land is made. Is the related statement over tropics and mid-latitude applicable for both over ocean and land. Also, is someplace in the document tropics,	The paragraph no longer appears

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						mid-latitudes, extratropics defined? [Government of United States of America]	
11-74	11	2	46	2	48	Rewrite sentence " Results are model-dependent with medium agreement .....". The second half of the sentence doesn't read well. [Aneesh Subramanian, India]	The paragraph no longer appears
11-75	11	2	46	2	49	Why is the forced component not strong in the Arctic where the projections agree to show the strongest warming? Maybe comment on the uncertainty in predictions for the Artic related to uncertainty in the sea-ice. [Antje Weisheimer, United Kingdom]	The paragraph no longer appears
11-76	11	2	47	2	47	"predictability" is missing between "that" and "of" at the end of the line [François Massonnet, Belgium]	The paragraph no longer appears
11-77	11	2	47			It is not clear what the 'predictability of the forced component' means. According to the definition in the previous paragraph, predictability is 'the extent to which the future of the system could be predicted under ideal circumstances' and it is 'an intrinsic property of the climate system'. This makes sense for initial state predictability. But what does this mean for the forced component? Surely, given a model with no biases in its response, and perfect knowledge of the future evolution of the forcings (these assumptions would seem to be consistent with the definition of predictability as an intrinsic property of the system), the forced component of change is perfectly predictable. Is the meaning that in the regions mentioned the forced component is large compared to the unforced variability? This isn't the same as saying that the predictability of the forced component is highest here. [Nathan Gillett, Canada]	The paragraph no longer appears
11-78	11	2	47			it is likely that the internally generated component….this seems a very staightforward finding, long known that there is large variability over extratrop oceans and I am not sure I would subscribe to 'modest' variability over land, its quite strong in the cold season. In any case, surprising to see this finding in the ES [Gabriele Hegerl, United Kingdom]	The paragraph no longer appears
11-79	11	2	47			The following wording is suggested: .. And it is likely that the predictability of the internally generated component … [Klaus Radunsky, Austria]	The paragraph no longer appears
11-80	11	2	49	2	49	Should [11.2.1] be [11.2.2.1]? [Government of Canada]	The paragraph no longer appears
11-81	11	2	49			The section cited here (11.2.1) is just an introduction and doesn't provide evidence to support this text. [Nathan Gillett, Canada]	The paragraph no longer appears
11-82	11	2	51	2	52	Is this statement really supported. Also, what about predictability of West African Rainfall [Noel Keenlyside, Norway]	The paragraph no longer appears
11-83	11	2	55	2	57	Many studies have found evidence of predictability in the Southern Ocean, these are not mentioned here [Noel Keenlyside, Norway]	The paragraph no longer appears
11-84	11	2		7		Executive Summary It is difficult to follow the flow/thought process of this section because it does not read in the same way that it is presented. It appears that information is presented out of order or that it is missing some context. [Government of United States of America]	The ES has been overhauled, shortened and restructered. The restructure was considered carefully, taking this issue into account.
11-85	11	2		7		Executive Summary This section does a good job of summarizing the findings from scientific literature on estimating the 'near-term' climate. The initial description of the terminology is quite useful to the reader. [Government of United States of America]	Thank you. No further response required.
11-86	11	2		7		Executive Summary Models that helped to draw conclusions are not consistently mentioned throughout the summary. If models are going to be mentioned some of the time, then they should be used throughout. If not, then the reader asks, "based on what model?" The figures do help once out of the Executive Summary. [Government of United States of America]	Names of models used in Ch 11 are given in the text in either Ch 11 (predictions) or Ch 12 (projections).
11-87	11	2		7		In the Executive Summary, the decadal predictions are presented with language that does not appear in the body of the text, e.g., "high confidence", "medium confidence". It would have been better to have introduced this language in the body of the report and explained what it actuall means. [Government of United States of America]	The calibrated language in the ES matches precisely what is in the main text.
11-88	11	3	1	3	30	There is no discussion here on the impact of initial state on global temperature. Over the last decade global	A discussion about this point has now been included

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						warming did not increase at the same rate as in the prior decades. Why? What are the policy implications of not flagging the possibility that this may continue? [Noel Keenlyside, Norway]	in the text, which also makes reference to the newly introduced Box 9.2.
11-89	11	3	1	4	9	These sections are poorly organized. Should the section about "Uncertainties in Future Anthropogenic and Natural Forcing" be under "Near-term climate projection"? [Government of United States of America]	ES has been rewstructered, and this point was taken into account in the restructutre.
11-90	11	3	1	7	50	overall a very good executive summary, with good use of conditional confidence statements. Could be a little shorter but I realize the chapter tries to cover a lot [David Lobell, United States of America]	Thank you. Length of ES has been reduced considerably.
11-91	11	3	2	3	2	I wonder if a better title for this section couldl be 'The Potential for Decadal Prediction" [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	The streamlined title accurately reflects the discussion.
11-92	11	3	2	3	30	Sorry, I find these two paragraphs of the executive (!) summary virtually incomprehensible. Please re-phrase for non-experts. [Jochen Harnisch, Germany]	This part of ES has been rerwritten and is now clearer.
11-93	11	3	2	3	30	Please include some discussion on how model dependent the results are. [Antje Weisheimer, United Kingdom]	The ES now contains the following sentence "While there is high agreement between systems that the initialization consistently improves the skill for these indices and regions (for the North Atlantic SSTs more than 75% of the forecast systems agree on the improvement with the initialization), there is also high agreement that it can consistently degrade others like the predictions of the equatorial Pacific temperatures in terms of correlation."
11-94	11	3	4	3	6	Say "predicted" indices here ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	ES has been overhauled. Sentence no longer appears.
11-95	11	3	4	3	30	Chapter 10 concludes that the lack of observed warming since 1998 is consistent with internal variability compensating forced warming. The implications of this should be a topic in Chapter 11, but there seems little consideration of this in the early sections -even if Figures 10.5 and 6 show such temperatures. (This issue is noted on p58.) Was the lack of warming predicted in simulations starting in 1998 or 2000? What is the prediction for the decade from 2010, if the global surface is anomalously cool at the start? [Government of Australia]	This issue is now dealt with in detail in a new Box in Chapter 9. Ch 11 now refers to this Box.
11-96	11	3	4	3	30	All the discussions or conclusions on decadal predictions are based on four-year averages time series (e.g., 2-5 yr or 6-9 yr). Actually, 10-year-average-based discussions should also be included. 10-year variability of Nino 3.4 index and East Asian surface air temperature are improved significantly (Wang et al, 2012), comparing with the no-initialized simulations. Improvements in sea surface temperature over Tropical Eastern Pacific and Indian Ocean can be also found in Fig. 9 of Wang et al (2012), while the performance in North Pacific degrades. This conclusion is not consistent with that on Lines 24-25 on Page 3. The reviewer thinks 10-year-average time series can also reflect the decadal variability, and should also be considered in this chapter. Please add the cited paper (Wang et al, 2012) for the near-term experiment by FGOALS-g2 to the reference list. Wang, B., et al, 2012: Preliminary evaluations on skills of FGOALS-g2 in decadal predictions. Adv. Atmos. Sci., Doi: 10.1007/s00376-012-2084-x [Bin Wang, China]	All single-model results are referenced in the text, including now FGOALS-g2. The ES only contains conclusions that apply to the CMIP5 multi-model because the report looks for robust results resulting from a wide consensus.
11-97	11	3	10	3	10	improves the correlation and the root mean square error skills (an error can not be improved, at most reduced) [Daniela Matei, Germany]	Agreed. But sentence no longer appears.
11-98	11	3	10	3	11	Is this statement accurate, are there regions where skill is degraded? [Noel Keenlyside, Norway]	The sentence was not appropriate. It has been changed to "The initialization of the climate system improves the skill of the global-mean temperature and AMV index as well as the temperature correlation over the North Atlantic, regions of the South Pacific and the tropical Indian Ocean". The existence of regions with

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							a skill degradation has now been included in the ES: "While there is high agreement between systems that the initialization consistently improves the skill for these indices and regions (for the North Atlantic SSTs more than 75% of the forecast systems agree on the improvement with the initialization), there is also high agreement that it can consistently degrade others like the predictions of the equatorial Pacific temperatures in terms of correlation"
11-99	11	3	11	3	11	Should not the reference be to Figures 11.6 and 11.7? [Daniela Matei, Germany]	References to the figures have been modified according to the new figure numbering.
11-100	11	3	11	3	12	is "the current retrospective prediction experiments" referring to the CMIP5 runs? If so, suggest to clarify this here. [Thomas Stocker/ WGI TSU, Switzerland]	This is now referred to in the ES as "The CMIP5 retrospective prediction experiments as well as other sources..."
11-101	11	3	12	3	15	Why highlight something that doesn't have statistically significantly positive skill ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The reason for highlighting the lack of robust skill in the wide Pacific basin is its relevance for the neighbouring continental surfaces and the increasing evidence that skilful information might appear under certain conditions.
11-102	11	3	13	3	15	What would be the implication of the statement "there might be some initial states that can produce skill in predicting IPO…"? Would one know beforehand? [Government of United States of America]	Statement no longer made in ES
11-103	11	3	14			This can be said for any prediction system and any phenomenon (e.g. astrology) [Michel Déqué, France]	Statement no longer made in ES
11-104	11	3	17	3	19	"There is high confidence that the retrospective prediction experiments for forecast periods of 1 to 18 years have statistically significant regional temperature correlations with the observations (exceeding0.6 over much of the globe) " [Jianqi Sun, China]	Statements re prediction have been rewritten.
11-105	11	3	17	3	30	Although volcanoes are mentioned below this point, I wonder if they should be introduced here as these results are contingent upon either knowing about future volcanoes or assuming that no volcanoes explode in the prediction period. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	The first sentence of the decadal prediction section of the ES reads now "The quality of multi-annual climate information has been assessed using initialized and non-initialized (historical simulations and RCP4.5 projections after 2005) predictions, conditioned on the knowledge of major volcanic eruptions."
11-106	11	3	17		30	Sentence 'The differences…' not sure what that means - in what variable? Overall, this section is hard to follow and seems to elevate findings of correlations that are not significantly higher than others (am I getting this right) a bit more than necessary [Gabriele Hegerl, United Kingdom]	This section has now been completely rewritten and emphasizes the results that point at improvements in the climate information available.
11-107	11	3	17			This overly technical depiction of confidence about correlations between retrospective experiments and observations is out of place in the summary. First, surely we know what the correlations between the retrospective prediction experiments and observations are; after all, the result from the retrospectives are in. So describing confidences that correlations will be >0.6 should not be the point. The real question is what confidence do we have that such significant correlatons will continue in the future in the real-world? [Government of United States of America]	This section has now been completely rewritten and emphasizes that 1) this is the first time that an appropriate verification of the climate information in the historical simulations has been undertaken from a climate prediction perspective and 2) there are improvements in the quality of such prediction information with the initialization of the simulations. There is no way to guarantee that the forecast quality estimates found for the past will directly apply for the near future, but as in many other climate-prediction problems such as seasonal prediction, they are the best information available to make decisions based on prediction information.
11-108	11	3	19	3	22	The bold text states that the initialization improves the temperature correlations, yet the next sentence states	This section has now been completely rewritten. The

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						that the differences between the intialized and uninitialized predictions are not significant at the 90% confidence level. These seem to be contradictory statements. [Government of United States of America]	skill improvements with the initialization can be found for, among other indices, global-mean temperature. At the regional scale, the improvements are not widespread.
11-109	11	3	19	3	22	If the differences between initialised and non-initialised forecasts are not significant, how can the initialisation then improve the temperature correlation? [Antje Weisheimer, United Kingdom]	This part has been rewritten. The several regions mentioned are those where the differences in skill are statistically significant. In addition, these regions show agreement in the improvement between the different forecast systems considered.
11-110	11	3	21	3	22	Say explicitly that much of the predictability is therefore coming from the forced component. [Nathan Gillett, Canada]	This has been included in the ES.
11-111	11	3	21	3	22	"The differences between initialized predictions and the non-initialized predictions are not statistically different with 90% confidence level". First, what are the climate variables that the authors talk about? Second, this sentence looks contradictory with the preceding sentence : "The initialization improves the temperature correlation over the North Atlantic, regions of the South Pacific and small continental areas". When these two sentences are read after each other , I cannot conclude whether or not the initialization is an effective approach. [François Massonnet, Belgium]	This part has been rewritten. Most of the paragraph refers to surface temperature, with a few sentences about precipitation. The skill improvements with the initialization can be found for, among other indices, global-mean temperature. At the regional scale, the improvements are not widespread. This has been made clearer in the new version of the text.
11-112	11	3	21	3	27	Some figure numbers in the Executive summary are incorrect. For example on page 3, in the decadal prediction section, 11.8 and 11.9 are mis-labeled. [Government of United States of America]	all references to figures chjecked and corrected as required
11-113	11	3	21			Reference to Figure 11.7? [Daniela Matei, Germany]	all references to figures chjecked and corrected as required
11-114	11	3	22	3	22	What does 'with 90% confidence level' mean here? The p value cited should be the probability of getting a more extreme value of the sample statistic (than the one actually obtained from the data) under the null hypothesis. Hence 90% should be a very long way from statistical significance. [Government of Australia]	Statement no longer made,
11-115	11	3	24	3	41	the fact that RCP scenarios used in CMIP5 don't span a plausible range of aerosols is very important, it's not clear why a simple figure is not included to show the differences between the SRES and RCP emissions. Is this included in another chapter that is cross-referenced? [David Lobell, United States of America]	ES now says: "For greenhouse gas forcing the new RCP scenarios are similar in magnitude and range to the AR4 SRES scenarios in the near-term, but RCP aerosol and ozone precursor emissions are much lower than SRES by factors of 1.2 to 3".
11-116	11	3	25	3	27	What is the basis for this statement? What does "medium confidence" mean and how is it derived from the analysis in Fig. 11.8? [Antje Weisheimer, United Kingdom]	The IPCC guidance report on the uncertainty language recommends to use the following dimensions to evaluate the validity of a finding: the type, amount, quality, and consistency of evidence (summary terms: “limited,” “medium,” or “robust”), and the degree of agreement (summary terms: “low,” “medium,” or “high”). Generally, evidence is most robust when there are multiple, consistent independent lines of high-quality evidence. For findings with high agreement or robust evidence, but not both, one is expected to assign confidence. Medium confidence is used here because there is high agreement in the results obtained in the papers of Corti et al. (2012) and Doblas-Reyes et al. (2013), but as this is only one piece of evidence, we can just assume that there is limited or medium evidence that the predictions are reliable. Corti, S., A. Weisheimer, T.N. Palmer, F.J. Doblas-Reyes and L. Magnusson (2012). Reliability of decadal predictions.

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							[WWW]Geophysical Research Letters, 39, L21712, doi:10.1029/2012GL053354. Doblas-Reyes F.J., I. Andreu-Burillo, Y. Chikamoto, J. García-Serrano, V. Guemas, M. Kimono, T. Mochizuki, L.R.L. Rodrigues and G.J. van Oldenborgh (2013). Initialized near-term regional climate change prediction. Nature Communications, doi:10.1038/ncomms2704.
11-117	11	3	27	2	30	What is the physical basis for the claim that the specified forcings lead to positive skill in forecasting precipitation over land? [Antje Weisheimer, United Kingdom]	The physical basis is yet not established, although some authors have pointed at the role of some of the indices that have positive skill like the AMV.
11-118	11	3	27			Reference to Figure 11.8? [Daniela Matei, Germany]	all references to figures chjecked and corrected as required
11-119	11	3	29	3	30	I thought there was some increased skill due to initialisation in some regions; seems to be discussed this way below [Noel Keenlyside, Norway]	This is now mentioned in the ES. The regions are the North Atlantic, regions of the South Pacific and the tropical Indian Ocean for temperature. No improvements have been found for precipitation.
11-120	11	3	29			Should '95% confidence level' be replaced with '5% confidence level'? (See previous comment in relation to this, and follow conventional statistical terminologies) [Government of Australia]	Sentence no longer appears.
11-121	11	3	32	3	57	Given how long the ES is, could this section by snythesised better by drawing out the implications for likely future warming ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	ES is now substnatially shortened and restructured.
11-122	11	3	34	3	41	I like this paragraph. I think it describes the issue well. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Thank you. No further response required.
11-123	11	3	34	3	41	The fact that the RCPs do not span the previous range of aerosols considered is a key point and is clearly stated at TS-38. [European Union]	Issue is now discussed in ES.
11-124	11	3	36	3	40	This is an exceedingly important caveat, and while its folded in throughout and especially in the discussion of Fig 11.33, it can hardly be overemphasized as a difficulty with most of the near term results. [Government of United States of America]	Issue is now discussed in ES.
11-125	11	3	36	3	40	I really like this chapter with exception of how the aerosol trends of the RCPs are discussed. I am not sure what lead to these misinterpretations, but there are a number of issues with this headline paragraph, which requires corrections also in subsequent sections: 1) it is not correct that the "RCPs collectively represent the low end of future emissions scenarios of aerosols and other short-lived reactive gases". All RCPs are based on intermediate assumptions about air pollution controls (see eg Riahi et al, 2011 for underlying assumptions of RCP8.5). The RCP aerosol emissions follow thus rather “middle of the road” pathways, particularly if compared to projections of other major studies that have been published recently. I will give some examples for SO2: Global SO2 emissions levels of the RCP 4.5, 6, and 8.5 by 2030 are about the same as the “current legislation” scenarios (ie, central cases) of the IEA (2012) and the Global Energy Assessment (2012). As a matter of fact all three RCPs are by a factor of three higher in 2030 than the low SO2 projection of the GEA. In addition, the sulfur emissions of RCP2.6 is about the same as the low IEA scenario and a factor two higher than the lower bound estimate of the GEA. 2) Given above point it is thus also not correct to characterize the RCPs as showing “rapid reductions”. They are certainly not rapid compared to lower bound scenarios that have explored the implications of stringent air quality legislation, such as the GEA (2012). 3) Similarly, SRES A2 and A1FI are not representative of the “high end” of future emissions for aerosols. Both scenarios are outdated with respect to aerosol emissions and represent rather outliers compared to the recent literature. They are also inconsistent with the empirical evidence of recent inventories showing that emissions of SO2 are on a declining pathway since many years by now (see eg Garnier et al (2011) for the World, or Zhang et al, 2012 for China; US EPA 2012 for the US). By contrast A1FI and A2 show pronounced increases of aerosol emissions over the short term, which is extremely unlikely taken into account both observed trends	We have fully revised the discussion of RCPs in light of additional evidence for SO2, noting however, that while the RCPs appear on track over the past decade for SO2, this does not necessarily apply to other aerosol species or ozone precursors. Our prior conclusion in the SOD was based on the van Vuuren et al. Climatic Change 2011 Figure 7 and the accompanying text, "This is mostly due to the RCPs’ shared assumption of stringent air pollution policies increasing proportionally with income (van Ruijven et al. 2008). As such, one may conclude that the RCPs show a range of plausible development pathways for air pollutants and policy interventions, but they are not fully representative of the literature on air polluting emissions, as the set does not include scenarios which assume that very little or no reduction of emissions will be achieved. This may limit the use of the RCPs for specific air pollution applications". We note that the reviewer is a co-author on this van Vuuren et al paper.

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						of the past as well as legislated pollution controls in the relevant countries for the years ahead. REFS: Riahi, K., Rao, S., Krey, V., Cho, C., Chirkov, V., Fischer, G., Kindermann, G., Nakicenovic, N., Rafaj, P. RCP 8.5-A scenario of comparatively high greenhouse gas emissions (2011) Climatic Change, 109 (1), pp. 33-57. IEA, 2012, World Energy Outlook 2012, OECD/IEA, International Energy Agency, ISBN: 978-92-64-18084-0 GEA, 2012: Global Energy Assessment - Toward a Sustainable Future, Cambridge University Press, Cambridge, UK and New York, NY, USA and the International Institute for Applied Systems Analysis, Laxenburg, Austria. Granier, C., Bessagnet, B., Bond, T., D'Angiola, A., van der Gon, H.D., Frost, G.J., Heil, A., Kaiser, J.W., Kinne, S., Klimont, Z., Kloster, S., Lamarque, J.-F., Liousse, C., Masui, T., Meleux, F., Mieville, A., Ohara, T., Raut, J.-C., Riahi, K., Schultz, M.G., Smith, S.J., Thompson, A., van Aardenne, J., van der Werf, G.R., van Vuuren, D.P. Evolution of anthropogenic and biomass burning emissions of air pollutants at global and regional scales during the 1980-2010 period (2011) Climatic Change, 109 (1), pp. 163-190. Zhang O, He K and Huo H 2012 Cleaning China's Air Nature 462 161-162. US EPA 2012 National Emissions Inventory (NEI) Air Pollutant Emissions Trends Data, 1970 - 2012 Average annual emissions, all criteria pollutants. http://www.epa.gov/ttn/chief/trends/index.html [Keywan Riahi, Austria]	
11-126	11	3	38	3	39	This statement "there is robust evidence that collectively these represent the low end of future emissions scenarios for aerosols" is incorrect and is not supported by the literature. See more detailed comments below on the chapter text. [Steven Smith, United States of America]	Please see response above.
11-127	11	3	38	3	39	This statement "There is robust evidence that accompanying controls on methane (CH4) emissions would offset some of this sulphate-induced warming" is also incorrect. See below. [Steven Smith, United States of America]	Statement removed.
11-128	11	3	40	3	40	chapter citations wrong or incomplete: 1.3.x?, 9.x.x.? & 11.5.3.1. is not existent [Government of Germany]	Citations to other sections being carefully checked and updated.
11-129	11	3	43	3	45	This conclusion regarding the impact of reductions in sulphate aerosols is overstated in my view. The RCPs all simulate a progressive decrease in aerosols over the course of the full 21st century to close to pre-industrial levels. My understanding is that the RCP SO2 emissions may be considered as a lower bound on SO2 emissions. Chapter 10 shows that the aerosol-attributable cooling to present from preindustrial has a best estimate of ~0.4 K (see Figure 10.4). Thus a plausible range of aerosol-induced warming is ~ 0.4 K/century over the 21st century if SO2 emissions follow the RCP trajectory. This is small compared to the GHG-induced warming over the same period. The current wording 'there is medium confidence that this could lead to rapid near-term warming' implies to me that the warming induced would be much larger than that due to GHGs alone. Better wording would be 'there is medium confidence that this could enhance near term warming by approximately X% compared to the warming due to GHGs alone.' [Nathan Gillett, Canada]	On the basis of Figure 10.4 plus a few other published studies, we have revised this statement to point to an impact of a few tenths of degrees and we have fully overhauled discussion of RCP aerosol emissions and climate responses in Section 11.3.6.1
11-130	11	3	46	3	49	The phrasing needs to be more careful here. We know the sign of BC emission reductions is likely to be cooling. The problem is we don't have a technology that only reduces BC. So it is the response to BC control measures that has uncertain sign. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Good point. It is now explicitly stated that BC is a warming agent (first sentence of ES bullet focused on impacts from near-term climate forcers). The statement regarding control measures has been revised in our efforts to shorten the chapter but still emphasizes the large uncertainty and Section 11.3.6.1 discusses that this uncertainty is due to co-emitted species.
11-131	11	4	6			Solar forcing: the 11-yr cycle at least over the satellite period is much larger than any low-f so I find this statement a bit overly worried about the lack of low-f solar forcing in the runs. Also because somewhere in chapter 11 or 12 (forgot which one) it is spelled out that even if there was a Maunder Minimum like event it would make very little difference to projections. [Gabriele Hegerl, United Kingdom]	Projected changes in natural forcing are now removed from the Chapter 11 Executive Summary as this topic is properly dealt with in Chapter 8.
11-132	11	4	7	4	7	In "... projections of near term climate ...", the expression "near term" should be "near-term". [Gan Zhang, United States]	Accepted.

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11-133	11	4	23			It is stated that no major volcanic eruption is presumed, but if Pinatubo counts as a major eruption, then the projection presented here would withstand one such eruption in the period 2016-2035. As noted elsewhere in this chapter, and in earlier chapters, Pinatubo gave a peak cooling of about 0.5K for a year, with cooling declining quite rapidly after that. So averaged over twenty years, a net contribution of at most 0.05K would be expected from a Pinatubo-type eruption. The projected range of 0.4-1.0K is quoted to only one decimal point. [Adrian Simmons, United Kingdom]	Accepted. The extent to which the assessed range may be sensitive to future volcanic eruptions has been clarified.
11-134	11	4	24			The following wording is suggested: .. , and also the medium evidence that .. [Klaus Radunsky, Austria]	Noted. This sentence has now been rephrased.
11-135	11	4	27	4	28	two times "that" [European Union]	Noted. This sentence has now been rephrased.
11-136	11	4	27		29	If aerosol forcing is really stronger wouldn't then move the entire uncertainty envelope down or get bigger rather than squish the uncertainty towards the lower end? [Gabriele Hegerl, United Kingdom]	The evidence that the uncertainty envelope is skewed was based on SoD Fig 11.33, not any assumptions about aerosol forcing. However, this statement has now been omitted.
11-137	11	4	28			"It is more likely than not that that actual warming will be closer to the lower bound of 0.4°C than the upper bound of 1.0°C (medium confidence)". --> two probability statements in one sentence are quite hard to understand. Can you reformulate this sentence and all other ones that have double probability statements. [Christof Appenzeller, Switzerland]	Accepted. This statement has now been rewritten.
11-138	11	4	31	4	49	Shouldn't the sequence of these two paragraphs be exchanged? [Jochen Harnisch, Germany]	Accepted. These paragraphs have been rewritten.
11-139	11	4	33	4	33	Insert 'in the same climate model' after 'scenarios' [Government of Australia]	Accepted. This sentence has been rephrased to improve clarity.
11-140	11	4	37	4	38	In the near term, model uncertainty and natural variability therefore dominate the uncertainty in projections of global mean temperature.' This conclusion is correct although it could well be that model uncertainty is at least partly a dominant term in near-term projections because of the applied large changes in aerosol forcing in the near-term projections throughout the RCP scenarios. Models are know to be variable sensitive to aerosol changes. This specific model deficiency might get very prominent in the near-term projections because of the large aerosol changes assumed throughout the RCP scenarios to which models might respond differently. By comparing with model simulations without rapid aerosol changes the importance of the aerosol changes on model uncertainty can be fully assessed. Maybe a short remark could be made on increased model uncertainty during time periods of (assumed) rapid aerosol changes (both over last 30 years and the coming 30 years), see also same comment on same text in TS P40 L35-36) [Michiel van Weele, Netherlands]	Accepted. Detailed discussion is beyond the scope of the ES, but the ES now contains the statement: "Projections of near-term climate show modest sensitivity to alternative greenhouse gas emissions scenarios, but substantial sensitivity to uncertainties in aerosol emissions, especially on regional scales and for hydrological cycle variables (high confidence)."
11-141	11	4	39	4	40	Please check the number of the figure 11.32, it seems to be the figure 11.32b. [Government of Germany]	Panels a and b of Fig 11.32 are both relevant to this assessment point
11-142	11	4	42	4	43	Seems to me something is either high confidence ot it isn't; I don't see how it can be caveated with "particularly". [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted and amended
11-143	11	4	42	4	44	What are new levels of warming relative to pre-industrial? Is "new levels of" needed? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. This paragraph has been re-written
11-144	11	4	42	4	49	It would be good to cross check these statements with Ch12 and Ch13 to make sure they are consistent [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Consistency has been checked.
11-145	11	4	42	11	49	What is missing here (and elsewhere in the chapter) is information on probability of even higher warmings being reached. The risk, for example, that 3 C warming may be reached by 2050 under RCP8.5 is highly relevant information for mitigation or adaptation planning. Summary statements should not be confined to only those upon which a high likelihood can be placed. [Government of Australia]	Accepted. Table 11.2 has been extended in collaboration with Chapter 12. Note however, that it is not possible to provide likelihood assessments for all time frames, because of insufficiently high confidence. See 11.3.6.3.
11-146	11	4	42	11	49	What is missing here (and elsewhere in the chapter) is information on probability of even higher warmings being reached. The risk, for example, that 3 C warming may be reached by 2050 under RCP8.5 is highly relevant infomation for mitigation or adaptation planning. Summary statements should not be confined to only those upon which a high likelihood can be placed. [Penny Whetton, Australia]	Accepted. Table 11.2 has been extended in collaboration with Chapter 12. Note however, that it is not possible to provide likelihood assessments for all time frames, because of insufficiently high confidence.

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							See 11.3.6.3.
11-147	11	4	45			This discussion needs to be clarified regarding the issue of whether these are one year (temporary) crossings or more or less permanent crossings that are being reported. [Government of United States of America]	Accepted. The SoD assessment was based on 10 year means. The final assessment is based on 20 year means and therefore represents "more or less permanent crossings". The exact time period used is 2016-35 and is specified.
11-148	11	4	45			0.6 since preindustrial? Or 1750? And that's got to be somewhat uncertain given that we don't know the SH mean temperature around 1750 and even the NH one is quite uncertain? (see chapter 5) - I understand that the sentence avoids this uncertainty by just 'assuming' but thats a bit misunderstandable or even misleading isnt it? [Gabriele Hegerl, United Kingdom]	Noted. There is no unambiguous definition of "pre-industrial" climate. The 1850-1900 reference period was chosen because this is the earliest period for which there is an adequate instrumental record. The term "pre-industrial" has been removed to avoid any misunderstanding.
11-149	11	4	57	4	57	Replace '–0.1oC' with '0.1oC' since this follows 'cooling' in the sentence [Government of Australia]	Noted. However, this paragraph has now been rewritten.
11-150	11	4	57	4	57	Presumably 'conditions' are 'solar conditions', not 'terrestrial conditions'. If so, insert 'solar' before 'conditions' [Government of Australia]	Noted. However, this paragraph has now been rewritten.
11-151	11	5	3	5	3	How small is 'regional' here? Does it mean continental-scale, sub-continental scale? [Government of Australia]	Accepted. Text has been amended to clarify.
11-152	11	5	6	5	16	Is it possible to quantify the land-sea warming ratio over shorter time scales? It is quantified in Ch12 [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Noted. In so far as we are aware this has not been done in the literature, and is potentially complicated in the near term by the changing role of anthropogenic aerosols. We therefore consider it appropriate to leave the more quantitative discussion to Chapter 12.
11-153	11	5	10			This statement on the emergence of signal is relative to a baseline period around today, but it's misleading in my view. In many tropical regions the summer signal has ALREADY emerged from variability. The implied message that it will take several model decades for the signal to be seen and felt is dangerous. One could say "emerges (or has already emerged in some regions) more quickly in the summer season...". The same issue is relevant for figures that put a year on a map when the signal emerges. [Reto Knutti, Switzerland]	Accepted. Text has been amended to clarify.
11-154	11	5	14	5	14	Please consider expressing this using the terms defined in the AR5 Uncertainty Guidance Note, i.e., provide explicit level of confidence rather than stating "less confidence". [Thomas Stocker/ WGI TSU, Switzerland]	This statement no longer appears
11-155	11	5	16			I suggest replacing 'tropospheric warming' with 'enhanced greenhouse gases'. I think that the exact mechanism whereby the Brewer-Dobson circulation is strengthened under climate change is still subject to debate, and writing 'tropospheric warming' implies a simple radiative effect. This might be driven by changes in wave sources in the troposphere, or by changes in the thermal structure of the tropopause region, which might be influenced by the effect of GHGs in the stratosphere not only a warming troposphere. [Nathan Gillett, Canada]	This statement no longer appears
11-156	11	5	20	5	26	The 'rich-get-richer' pattern of precip change is true to first-order, but I think there are subtle regional variations so we should be wary of stating it as a general rule. Statements about regional precip change should be more nuanced (here and in Ch 12). [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
11-157	11	5	22			The time period is confusing - the introductory sentence implies a discussion of change over the next few decades; then changes from 1986 to 2005 are discussed as "likely to only be significant"….Don't we know what these changes are since this occurs in the past? [Government of United States of America]	Taken into account - text revised. 1986-2005 is the reference period to assess climate changes in this chapter
11-158	11	5	28	5	31	worth adding a sentence here on changes in relative humidity and vapor pressure deficit, which are arguably more relevant than specific humidity [David Lobell, United States of America]	Taken into account - text revised. This paragraph refers to Fig.11.18 that will be replaced by new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b)

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							evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%). The number of CMIP5 models used wil be indicated in the upper-right corner of each panel.
11-159	11	5	29	5	29	"There is little robustness......" what does this mean in relation to confidence? Where possible, please use the standard language. [Government of Australia]	Accepted - text revised . "Little robustness" will be changed to "low-confidence"
11-160	11	5	30	5	30	The more common form of "... surface run off" should be "... surface runoff". [Gan Zhang, United States]	'runoff" now used throughout (following IPCC reports on water and SREX
11-161	11	5	31	5	32	What about the chances of Arctic sea ice in Sep nearly vanishing under other RCPs ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The assessment for near-term climate has been done for RCP8.5 to be consistent with the assessment for RCP8.5 throughout the 21st century in Ch 12.
11-162	11	5	31			A clear statement of near-term climate change projections and predictability of regional drought is lacking. It would be extremely valuable to provide a narrative on regional drought predictions and projections that addressed the relative role of anthropogenic warming and natural variability at the scales of interannual, decadal and multidecadal [Government of United States of America]	In response to this comment, we have (i) added additional panels to the former Figs 11.16-18 (SOD numbers), where we now also present E-P. In addition, we discuss droughts in an additional paragraph. The difficulty here is that the overwhelming majority of studies did merely address the far-term, and few only considered the near-term. We don't think that there is sufficient evidence for a substantial near-term assessment of droughts.
11-163	11	5	31			Section 11.4.2 is a typo. Should be 11.3.2 (also lines 16, 26). [Government of United States of America]	Corrected.
11-164	11	5	37	5	39	Clarify that stratospheric ozone recovery only plays a role in austral summer [Government of Australia]	The ES and main text now specify that the ozone recovery will be a dominant factor in austral summer
11-165	11	5	37	5	40	The statement is made here that the Hadley Circulation boundary and SH storm track are unlikely to move poleward as rapdily as in recent decades. Figure 11.19 and 11.20 show SAM-like wind anomalies in the near-term projections relative to a 1986-2005 climatology, and on average poleward displacement of the Hadley Cell and dry zone over the same period. Neither figure compares the rate of change in the future with the rate of change in the recent past. While 11.4.2.4.2 and 11.4.2.4.3 argue that the rate of change in the near-term projections is likely to be smaller than the rate of change in the recent past, the studies cited do not directly demonstrate this. No mention is made here of the fact that the effect of ozone recovery is mainly in DJF, and in all the other seasons, the effects of GHGs are not strongly opposed by ozone. Thus I think a conclusion more directly supported by the evidence shown in the chapter and in the cited studies would be that the southern boundary of the Hadley Circulation and the Southern Hemisphere strom track are likely to shift poleward in the annual mean in coming decades. Then state that with say medium confidence that the rate of poleward shift in boreal summer is likely to be smaller than in recent decades due to the effect of stratospheric ozone recovery. [Nathan Gillett, Canada]	Statements have been revised to refocus on the role of ozone in austral summer, the Thompson et al. (2011, Nature Geosci.) review article is referenced. Assessment is likely that there will be a poleward expansion and likely that in austral winter the poleward expansion will be slower than in recent decades, given the results of Swart and Fyfe, Thompson et al. (2012, Nature Geosci.), Eyring *et al.* (2013), and the studies summarized in the SOD (Polvani et al. 2011.b, Arblaster et al., 2011; McLandress et al., 2011).
11-166	11	5	39	5	39	The statement that 'it is unlikely that [the meridional position of the Southern Hemisphere storm track] will continue to expand poleward as rapidly as in recent decades' implies that the Southern Hemisphere storm track has moved poleward rapidly in recent decades. The position of the strom track is not directly observed and can only be determined from reanalyses. Swart and Fyfe (2012) show in their Figure 3 that there has not been a significant trend in the annual mean position of the SH storm track in reanalyses over the period 1979-2010. (On the other hand there was a significant increase in its strength). Most of the studies describing a poleward shift in the storm track have used models, and most of the observational studies on circulation change in the Southern Hemisphere have used a SAM index based on SLP. I suggest either changing the statement to focus on the strength of the SH storm track, or making clear that this statement is about model simulations not observations. Swart, N. C. and J. C. Fyfe (2012), Observed and simulated changes in the Southern Hemisphere surface westerly wind-stress, Geophys. Res. Lett., 39, L16711,	Statements have been revised.

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						doi:10.1029/2012GL052810. [Nathan Gillett, Canada]	
11-167	11	5	39	5	40	Could cross-reference with Ch12 at this point to note that in the long term the trends in S. Hem. storm tracks are projected to re-emerge, [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Our ES was extremely long. We cross reference to chapter 12 in the main text.
11-168	11	5	39	5	40	The 'unlikely' statement requires support. The statement does not take into account that greenhouse gas forcing will be larger in the near-term than in previous decades which could lead to a similar or larger expansion even with ozone recovery. Swart and Fyfe, GRL, 2012 show that the rate of change in the jet position over coming decades is similar to recent trends. [Government of Australia]	Assessment of a reduced rate of poleward expansion is now "medium confidence", given the results of Swart and Fyfe which appear to disagree with those of Thompson et al. (2012, Nature Geosci.) and the studies summarized in the SOD (Polvani et al. 2011.b, Arblaster et al., 2011; McLandress et al., 2011).
11-169	11	5	54	5	55	I wonder if it is possible to cross-reference here to recent observations of the slowing of the rate of warming. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Cross rerefence to observed changes now included in main text. Assessment has been changed to "very likely".
11-170	11	5	55	5	55	Fig. 11.28 seems to be wrong in this context (ocean warming vs. sea ice concentration) [Government of Germany]	Reference to figure corrected.
11-171	11	5	55	5	55	chapter citations wrong: 11.4.4 non existent! [Government of Germany]	Reference to section corrected.
11-172	11	5	55	5	55	Fig. 11.28 is referenced, but this is a sea ice figure whereas the text deals with ocean temperature; shouldn't it be Fig. 11.24 which shows the time series of ocean temperature for different depths? [François Massonnet, Belgium]	Reference to figure corrected.
11-173	11	5	55			"Figure 11.28" would be "Figure 11.24" [Yoshimitsu Chikamoto, United States of America]	Reference to figure corrected.
11-174	11	5	56			See comment immediately above. Can the statement withstand one Pinatubo-type eruption in the twenty years of the projection? [Adrian Simmons, United Kingdom]	Statement has been modified to reflect that it is contingent on the absence of multiple Pinatubo-type eruptions.
11-175	11	6	4	6	4	A general information about the expectations for sea surface salinity would be helpful, like: "Changes in sea surface salinity are expected in response to changes in precipitation, evaporation and run-off; In general (but not in every region), salty regions are expected to become saltier and fresh regions fresher." [Government of Germany]	We agree that salinity is an important and interesting variable to consider. However, our chapter is already very long and so we had to draw the line somewhere. In our view we have taken a subset that has a higher priority.
11-176	11	6	5	6	6	Please check the origin of the following sentence, because the cited origin chapter "11.4.4" does´nt exist. "There is medium confidence that there will be increases in salinity in the tropical and (especially) subtropical Atlantic, and decreases in the western tropical Pacific over the next few decades [11.4.4]." [Government of Germany]	Reference to section corrected.
11-177	11	6	6	6	6	chapter citations wrong: 11.4.4 non existent! [Government of Germany]	Reference to section corrected.
11-178	11	6	8	6	9	This sentence does not read well. Rewording is needed. It only speculates that the AMOC could trengthen, but didn't mention why? It is internal AMOC variability or force response? [Aixue Hu, United States of America]	Now specified that possibility of temporary AMOC acceleration largely due to internal variability.
11-179	11	6	11	6	28	p.6: What are the projected changes to the climate modes that have previously been shown to be correlated or that impact the cryosphere and climate in the polar regions ? -- Written dec 2, 2012 [Aneesh Subramanian, India]	Summary statement revised to "likely" with medium confidence based on a subset of models that better simulate recent observations that may or may not be related to modes of circulation
11-180	11	6	13	6	28	If area decreases faster than volume, then thickness must increase, which is opposite to the recent trends. Some confidence level on model predictions would be welcomed. [Olivier Boucher, France]	"likely" with medium confidence now is the assessment for a nearly ice-free Arctic in terms of area; numbers for thickness here in the ES have been deleted and discussed in the chapter text
11-181	11	6	14	6	16	The statement 'there is low confidence that these changes could be predicted with any certainty over the next several decades' has several problems in my view. First, it is not clear what this means - that for the next few	This text has now been deleted in the revised ES text and is discussed in greater detail in Ch. 12

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						decades we will not be able to make skillful predictions of abrupt change in the cryosphere, or that predictions of abrupt change in the cryosphere for the next several decades have low confidence. Second by stating that there is the possibility of abrupt change, but that we have low confidence in predictions, this might be read as implying that any part of the cryosphere could exhibit an abrupt change any time, and we can't predict it. But for many parts of the cryosphere the probability of abrupt changes is quantified either here or in other parts of the report. Thus, in most cases we can make a statement about the overall likelihood of these events, even if we can't make deterministic predictions. I suggest that this statement, if retained, is focused on specific variables (sea ice and snow), and that the ability to make deterministic predictions of abrupt changes is set in context with the overall likelihood of abrupt changes. [Nathan Gillett, Canada]	
11-182	11	6	15			number given as -4%+-1.9% -- are you comfortable to provide this precision for the error bar? [Thomas Stocker/ WGI TSU, Switzerland]	agreed--number rounded to 2%, but this text new deleted from ES, but this rounding will be used in the main chapter text
11-183	11	6	16	6	18	Note a slightly different wording in Chapter 12 exec summary. The period is not the same and it refers to seasonal rather than late summer changes. [Olivier Boucher, France]	we now refer to September specifically, and have coordinated with ch 12
11-184	11	6	16	6	18	This seems much too cautious a finding given the observed rate of loss of ice, not just area but particularly volume. While it may be that this is what models show, their simulation of sea ice loss has been far too slow. While most of the focus to address this discrepancy has been on improving the representation of the physics, I'd like to suggest that it could be problems with the forcing. First, might it be that the reductions in SO2 emissions in the North Atlantic basin nations and Russia/eastern Europe that has cleared up Arctic haze has allowed a larger amount of direct solar to reach the snow/sea ice surface in spring, leading to earlier melting, albedo decrease, and so ice loss? Second, might it be that the climate models have been specifying a uniform latitudinal concentration of methane, whereas observations indicate that the high latitude concentration is 200 ppb higher than the global average; using a 20 year GWP, this is equivalent to 15 ppm of CO2 (and would be higher if really used the 10-year GWP given the methane lifetime), and this would mean the forcing being used is something like 20 -30 years behind where it should be, and that could bring agreement of model and observations. So, it might well be that the models just have had errors in forcing in high latitudes near the surface, and so observations are right and the sea ice could virtually disappear in summer this decade. I just think that this statement is far too dependent on model simulations that have not been accurately representing sea ice loss and has to be changed (as does the discussion later in the chapter--I won't comment back there). [Michael MacCracken, United States of America]	There could be many sourced of speculation about forcing not included in the models that could affect the cryosphere, and these are discussed in Ch. 9
11-185	11	6	17	6	18	The words "a very distinct possibility" should be replaced with more standard calibrated language. [Government of Australia]	wording changed, now "likely" with medium confidence
11-186	11	6	17	6	18	What is a very distinct possibility ? Use IPCC uncertainty language. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	wording changed, now "likely" with medium confidence
11-187	11	6	17	6	18	"is a very distinct possibility" appears rather vague and unclear to us. Can this be replaced with a more quantitative statement? [Thomas Stocker/ WGI TSU, Switzerland]	wording changed, now "likely" with medium confidence
11-188	11	6	17	6	18	a very distinct possibiliy' should be replaced with more standard calibrated language. [Penny Whetton, Australia]	wording changed, now "likely" with medium confidence
11-189	11	6	17			Is this date the date for a temporary crossing (opening of the sea ice) or a final (permanent) crossing? [Government of United States of America]	wording changed, now "likely" with medium confidence
11-190	11	6	18	6	21	These percentages are for ice extent, not ice area (see difference in Chapter 4, page 8, lines 46-50). [Thierry Fichefet, Belgium]	agreed, ice extent wording used and is now consistent with ch 12, though this text has been removed in the ES, though the wording is now used in the body of the text
11-191	11	6	18	6	24	These percentages come from the CMIP5 multi-model mean. This should be mentioned. [Thierry Fichefet, Belgium]	text now deleted from ES text, but this is now noted in body of text
11-192	11	6	18	6	28	No uncertainties are given for any of these numbers [Peter Stott, United Kingdom of Great Britain & Northern	text now deleted from ES text, but this is now noted in

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						Ireland]	body of text
11-193	11	6	20	6	20	since the word 'decreases' has already been used, replace '-28%' with '28%' and '-6%' with '6%' [Government of Australia]	text now deleted from ES text, but this is now noted in body of text
11-194	11	6	20	6	20	I would suggest to change "decreases" by "changes", since negative values are then displayed. This will avoid redundancy. [François Massonnet, Belgium]	text now deleted from ES text, but this is now noted in body of text
11-195	11	6	22	6	22	Same, change "Reductions" in "Changes" [François Massonnet, Belgium]	text now deleted from ES text, but this is now noted in body of text
11-196	11	6	23	6	23	There is a typo: the change in sea ice volume in September for RCP4.5 should be -43%, not -4%. The full text (p.42 l.46) contains the correct value [François Massonnet, Belgium]	text now deleted from ES text, but this is now noted in body of text
11-197	11	6	23	6	23	"-4% for September" obviously incorrect, should be "-43% for September" to be consistent with p. 11-42, line 46 [William Merryfield, Canada]	text now deleted from ES text, but this is now noted in body of text
11-198	11	6	26	6	27	"Reduction in annual mean near surface permafrost" is confusing terminology and what the authors are actually referring to is an increase in thaw depth. Annual mean does not make sense as permafrost by definition is frozen ground that exists for at least 2 years. This terminology should be avoided and to some extent is meaningless. Additional comments related to this are provided below. [Sharon Smith, Canada]	here and elsewhere we refer the reader to the glossary for the definition we use for "near surface permafrost"
11-199	11	6	32	6	33	"very likely" is somewhat inconsistent with Chapter 12 who state that 'it is virtually certain that, in most places, there will be more hot and fewer cold temperature extremes as global temperatures increase' [Government of Australia]	The two assessments are consistent, i.e. the probability of such changes increases from "very likely" in the near-term to "virtually certain" in the long term. In the near-term, the signal to noise ratio (and thus the probability) is smaller.
11-200	11	6	38			High-percentile daytime winter temperatures? As written the text just describes warming of daytime winter temperatures - this is not an extreme. [Nathan Gillett, Canada]	This part of the sentence has been dropped.
11-201	11	6	44	6	44	remove "regionally" [Annalisa Cherchi, Italy]	Done
11-202	11	6	44	6	44	Delete "regionally". [J. Graham Cogley, Canada]	Done
11-203	11	6	57	6	57	" for example ENSO, AMO and the IPO" is changed to "for example ENSO, NAO, AMO and the IPO" [Jianqi Sun, China]	Sentence has been removed.
11-204	11	6	57	6	57	"... including for example ENSO, AMO and the IPO." The term "AMO" was replaced with "Atlantic multi-decadal variability (AMV)" in the recent references (e.g., Häkkinen et al. 2011, Science) for language accuracy concerns. AMV has already been used in previous paragraphs of this chapter. But in the orther chapters (e.g., Chapter 14), AMO is more commonly used by the authors. If "AMV" and "AMO" are used to describe the same phenomenon, it would be more reasonable to stick to one certain form. [Gan Zhang, United States]	Sentence has been removed.
11-205	11	7	2	7	7	why do you extend the assessment for Tropical Cyclones to the end of the century? As indicated by the reference to Chapter 14, the assessment for Tropical Cyclones to the end of the century is given in Ch14 and should thus be in their Executive Summary, not here. [Thomas Stocker/ WGI TSU, Switzerland]	The discussion of end-of-century is removed, pointer to SREX and Chapter 14.
11-206	11	7	2	7	49	I would have expected to read something about the near future development of the terrestrial biosphere C sink strength in this chapter [European Union]	Terrestrial C cycle is not in the scope of CH11 -- Chapters 6 and 12 which we point to in Section 11.3.5.
11-207	11	7	2			"We have low confidence" vs. "There is low confidence" -- please consider a non-personal formulation. The WGI AR5 Style Guide suggests to avoid personal pronouns as much as possible (see also general comment) [Thomas Stocker/ WGI TSU, Switzerland]	Rephrased.
11-208	11	7	3	7	3	add "to assess forecast quality, based for the most part on past periods with a much weaker ocean observing system." [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Many more important sentences have been removed from the ES such as the fact that forecast skill assumes that volcanic eruptions will be known or the sentences about the improvement of the simulation of

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							the hiatus with the initialization. If those have been removed because there is not enough space, I then wouldn't include this one in the ES. I'd start with the other two instead.
11-209	11	7	11	7	14	But this begs the question of what about the chances for the near term given this is a high impact albeit low probability event ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The integrative discussion of abrupt climate change mechanisms is in Chapter 12, so this statement has now been removed from the Chapter 11 Executive Summary.
11-210	11	7	18	7	18	I don't think it is helpful to say that there is "high confidence" something will change, as obviously there is zero chance that it will stay exactly the same. Maybe say that there is "high confidence" that ozone will respond to emission changes. [William Collins, United Kingdom of Great Britain & Northern Ireland]+G643	Statement has been completely re-written and focuses first on comparison of SRES v RCP emission-driven changes. Then it compares range of possibilities for near-term under climate vs. emission scenarios, and finally provides a sense of regional-scale emission-driven changes.
11-211	11	7	23	7	26	Also mention that a warming climate will also accelerate the meridional circulation in the stratosphere and thus increase the stratosphere-troposphere exchange of ozone, and that this contributes to increased ozone concentrations mostly in the subtropics and midlatitudes (see comment no. 10). [Twan van Noije, Netherlands]	We have shortened the mechanistic discussion in the ES due to space limitations and emphasize that for air quality we are focusing on surface air. We do discuss this potential feedback of increasing STE on baseline O3 in Section 11.3.5
11-212	11	7	25	7	25	add "in the period tested" at end of sentence. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	These statements are general discussion of mechanism so not intended to apply to a specific period. They have been re-written and shortened to address space limitations.
11-213	11	7	25			Should 'evidence' read 'confidence'? [David Stevenson, United Kingdom]	No, we mean evidence here. There are some studies, but they vary in regional focus so we do not have a sufficiently good basis to assess agreement and thus provide a confidence statement on the basis of agreement + evidence.
11-214	11	7	28	7	28	PM2.5 - please add an explanation and ensure that the acronym is explained at least in the Annex of Acronymes. [Government of Germany]	We have removed PM2.5 from the ES and define it in the relevant sections.
11-215	11	7	28	7	30	This sentence is not accurate. Accroding to Figures 11.31ab, near term O3 air quality will degrade over North America and Europe also under RCP8.5. For East Asia, O3 air quality degrades under two scenarios (RCP8.5 and RCP6.0), and PM2.5 air quality degrades only in RCP 6.0. [Hong Liao, China]	The intent here is to provide an overview of the general trends within regions under most scenarios, considering the information in Annex II as presented visually in Figures 11.31ab and 11.30. We have fully revised this statement to improve clarity.
11-216	11	7	28	7	30	AR5 tried to discuss the changes of air quality including fine particulate matter (PM2.5) in the near future. But one issue we need to consider in the projections is the RCP scenarios have already assumed that aerosol emssions rapidly reduced in the next few decades. So there is a logical confusion between causes and results. [Shaojie Song, United States of America]	Yes projections are fundamentally tied to the emission scenarios as discussed in Section 11.3.5.
11-217	11	7	30	7	32	This is a bit of a tautology. There is less spread in the RCP scenarios, which have all used the same assumptions, than in the SRES scenarios, which have used a range of assumptions. I would not use a confidence statement here, as it is so dependent on scenarios on which we have no confidence. Why not say something like "The range in projected air quality changes is much narrower across the RCP scenarios than across the SRES scenarios. This is due to the common assumption of aggressive air quality controls. However we do not have confidence that such an assumption is entirely justified". [Olivier Boucher, France]	Thanks, we have completely revised the ES statements, including a direct comparison of the range of emissions across RCP scenarios versus other scenarios (CLE and MFR). Note that we need to avoid value judgments, i.e., avoid saying which scenarios are most plausible
11-218	11	7	30	7	32	I think the statement on the narrowness of the RCPs needs some care. While what is written is accurate, it is simply a statement about the different constructions of 2 sets of scenarios. Whereas I do think we have a clearer understanding of the likely future air quality by 2100 compared to AR4 and this expert assessment	Indeed, we have carefully revised our discussion of the RCPs and their implications for air quality projections

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						should be brought out here. RCPs are likely to be towards the low end of possible future (non-methane) emissions, whereas the SRES A2 is now thought to be way beyond anything that's likely. So I think it can be said that range of probable air quality futures is significantly narrower (and cleaner) than thought at the time of AR4, but not as narrow as the RCPs would suggest. [William Collins, United Kingdom of Great Britain & Northern Ireland]	
11-219	11	7	43	7	49	This statement looks weak as it stands. This should be replaced by one of the stronger statements in 11.3.5.2.3 that say "Meteorological conditions tied to ozone and PM events are likely to increase", and "it is likely that, statistically , a warming climate will exacerbate extreme ozone and PM pollution events". [William Collins, United Kingdom of Great Britain & Northern Ireland]	We have fully revised our assessment to clarify the difference in our confidence of projecting the meteorological conditions conducive to extreme air pollution events versus the temperature-driven feedbacks operating during these events for which we have more confidence.
11-220	11	8	1			Introduction: too long [Antje Weisheimer, United Kingdom]	The introduction is only 1.1 pages long. Given the breadth of topics we have been assigned to coverr this does seem too long to us. Perhaps you inciorrectly included Box 11.1 in your assessment of length. This will appear separately in the final report.
11-221	11	8	5	8	5	Unless there a glossary that defines commonly used terms, please state mid-century means 2050. [Government of United States of America]	'Mid-century' is used more broadly than this so we may use the term to simplify the text when assessing studies that have used various peiods in and around 2050.
11-222	11	8	19	8	19	"Committed warming"…Committed change may not be for warming everywhere. We believe "committed change" is a better wording. [Government of United States of America]	agreed. Changed.
11-223	11	8	26	8	27	One might note that the need to go from the traditional 30-year scale to the decadal scale of climate change was already expressed during the first World Climate Conference in Geneva in 1979. [Robert Kandel, France]	Thank you for this tid bit. However we have to keep text short and so did not mention this.
11-224	11	8	26	8	44	A short definition of predictions and projections s(which anticipates the long explanation in BOX 1) should be given before these two paragraphs. [Susanna Corti, Italy]	brief explanation of predictons provided near beginning of in Section 11.1 (Introducton)
11-225	11	8	30	8	31	Suggest changing to something like "The goal with such predictions is to increase forecast skill by exploiting any predictability in internally-generated climate variability and correcting model responses to previous external forcing" [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	corrected.
11-226	11	8	32	8	32	After "… internally generated climate variability.", I would add "as well as its interaction with external forcings (such as aerosols which can have a regional signature)". This aspect is missing in the introduction [Christophe CASSOU, France]	corrected.
11-227	11	8	32	8	32	"initial state of internally generated climate variability". Is this different from the initial state of the system? [Ramon de Elia, Canada]	no longer in revised chapter
11-228	11	8	32	8	34	It's not clear enough from this and the following sentences what these "major challenges" are. Are they really major? [Antje Weisheimer, United Kingdom]	corrected.
11-229	11	8	33	8	38	The sentence starting with "Thus both foci..." is a bit obscure. Here as well a rephrasing is needed. [Susanna Corti, Italy]	removed
11-230	11	8	38	8	38	typo "he" - "the" [Annalisa Cherchi, Italy]	Corrected in revised chapter.
11-231	11	8	38	8	38	something is wrong with this sentence "in he other focus of CMIP5" [Andreas Fischer, Switzerland]	corrected
11-232	11	8	38	8	38	"in he other focus of " he -> the [Dan Hodson, United Kingdom]	corrected
11-233	11	8	38	8	38	he -> "the" [Noel Keenlyside, Norway]	corrected
11-234	11	8	38	8	38	"he" -> "the" [Holger Pohlmann, Germany]	corrected

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11-235	11	8	38	8	38	in the focus of [Aneesh Subramanian, India]	corrected
11-236	11	8	38	8	38	In ".. are circumvented in he other focus ...", "he" appears like a typo and it should be "the". [Gan Zhang, United States]	corrected
11-237	11	8	38	8	44	suggest to refer to Annex I: Atlas, in addition to the reference to Chapters 12-14; text from page 25, lines 21-28 could be used [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
11-238	11	8	38			Typo: "he" [Adrian Simmons, United Kingdom]	corrected
11-239	11	8	39	8	39	are often referred to as... [Aneesh Subramanian, India]	corrected.
11-240	11	8	41	8	41	"asessments" -> assessments [Dan Hodson, United Kingdom]	corrected
11-241	11	8		12		Section 11.1 and 11.2: The time scale of decadal climate prediction is 1-10 years; its lower part is also covered by existing seasonal-to-interannual climate prediction. For example, using a comprehensive climate model, Luo et al. (2008) showed two-year lead prediction of ENSO and its related climate anomalies over the globe. Besides, recent studies have found that external radiative forcing or global warming has considerable impact on seasonal-to-interannual climate prediciton (Doblas-Reyes et al. 2006; Liniger et al. 2007; Cai et al. 2009; Luo et al. 2011). More and more seasonal-to-interannual climate prediciton models have also implemented time-varying radiative forcing. In this regards, the way to perform seasonal-to-interannual climate prediciton and that to preform decadal climate prediction are the same. It would be desirable to add some introductions of the link and differences between the seasonal-to-interannual and decadal prediction, current achievements of seasonal-to-interannual prediction, and the reasons why we need to go [Jing-Jia Luo, Australia]	Brief discussion of linkages between seasonal and decadal prediction included, but more discussion limited by space requirements. Some references noted are included.
11-242	11	8		12		beyond the seasonal-to-interannual prediction, etc. References: 1) Luo, J.-J., S. Masson, S. Behera, and T. Yamagata, 2008: Extended ENSO predictions using a fully coupled ocean-atmosphere model. J. Climate, 21, 84-93. 2) Doblas-Reyes, F. J., R. Hagedorn, T. N. Palmer, and J.-J. Morcrette, 2006: Impact of increasing greenhouse gas concentrations in seasonal ensemble forecasts. Geophys. Res. Lett., 33, L07708, doi:10.1029/2005GL025061. 3) Liniger, M. A., H. Mathis, C. Appenzeller, and F. J. Doblas-Reyes, 2007: Realistic greenhouse gas forcing and seasonal forecasts. Geophys. Res. Lett., 34, L04705, doi:10.1029/2006GL028335. 4) Cai, M., C.-S. Shin, H. M. Van den Dool, W. Wang, S. Saha, and A. Kumar, 2009: The role of long-term trends in seasonal predictions: Implication of global warming in the NCEP CFS. Weather and forecasting, 24, 965-973. 5) Luo, J.-J., S. Behera, Y. Masumoto, and T. Yamagata, 2011: Impact of global ocean surface warming on seasonal-to-interannual climate prediction. J. Climate, 24, 1626-1646. [Jing-Jia Luo, Australia]	see above response.
11-243	11	8				Add to footnote that the external forcings might be held constant across the forecast period in the numerical integration. [Government of Australia]	The fact that forcing is held constant in noted in the body of chapter 11 (11.2)
11-244	11	9	2	9	2	Does this imply that all inferences about the "decadal prediction" are relative to "1986-2005" climatology? [Government of United States of America]	Yes.
11-245	11	9	2	9	2	Would it be better to use the period 1986-2005 rather than 1971-2000 to be consistent with the rest of the report? [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Agreed. Rewritten and consisten.
11-246	11	9	3	9	3	typo "inis" - "is" [Annalisa Cherchi, Italy]	typo corrected
11-247	11	9	3	9	3	"is" instead of "inis" [Andreas Fischer, Switzerland]	typo corrected
11-248	11	9	3	9	3	"inis" -> "is" [Dan Hodson, United Kingdom]	typo corrected
11-249	11	9	3	9	3	"inis" in "The focus inis on the 'core' near-term period..." is possibly a typo. [Gan Zhang, United States]	typo corrected
11-250	11	9	3			… inis (typo) [Government of France]	typo corrected
11-251	11	9	4	9	4	the focus "is" on the 'core'..... [Aneesh Subramanian, India]	typo corrected
11-252	11	9	11			Box 11.1 (Climate Prediction, Projection and Predictability) is superb. In fact, I thought that Box 11.1 and	Thank you. Unfortunately not everyone agrees

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						Section 11.3 are, for the most part, very very well done. Congratulations to the authors. [Fyfe John, Canada]	
11-253	11	9	13	0	0	Box 11.1: I think the Box is much too long and not concise enough. The slit into internally generated and externally forced climate components seems to over-simplify things a bit. Fig 11.1a is not very helpful, I feel. I would instead suggest to include Fig. 2 from "Decadal Prediction - Can it be skilful?" by Meehl et al., BAMS (2009) showing a schematic of the temporal evolution of the initial and boundary value problems with decadal predictions benefitting from both. [Antje Weisheimer, United Kingdom]	We have tried hard to be both clear and concise and feel that this information is useful to readers who are not as familiar with the ideas and terms as the reviewer. Fig 11a. This is a relatively complicated figure that is meant to be a schematic although the results are from a particular model. We would like to retain it since it illustrates the several terms we are trying to illustrate. We now introduce a schematic similar to Fig 2 of Meehl et al. to enhance the discussion.
11-254	11	9	13	11	31	I count only two citations to papers in this section, which is based on a large number of studies in the literature. Perhaps there should either be no citations in the text or some more could be added. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	The Box is meant to be explanatory rather than a survey or assessment that would reference the literature. The references that appear in the Figure captions do so so as to give some providence for them. We hope this is acceptable.
11-255	11	9	13			Box 11.1: you currently seem to use forecast as a synonym for prediction. But we found the switching back and forth between the two terms rather confusing. For example in Box 11.1 there are subheadings for "Climate Prediction", "Climate Predictability" and "Forecast Quality"; under the subheading "Climate Prediction" there are entries on "climate prediction or climate forecast", "deterministic forecast" and "probabilistic climate prediction". We felt it would be good to stick as much as scientifically justified to a single term, i.e., "prediction". BTW, if forecast is continued to be used, would it make sense to add mention it in the Glossary? [Thomas Stocker/ WGI TSU, Switzerland]	We do use forecast and prediction interchangeably and make a point of this since it is common usage. We now add "forecast" to the subheading to stress this. The two terms are used interchangeably and appear as synonyms in dictionaries for instance. We would like to retain the freedom to use either term as both are often found in the literature we and agree that this should be in the Glossary.
11-256	11	9	20	9	20	Figure 1 - This figure should highlight the difference between projections and predictions. However , in my opinion, it does not address clearly the point. First of all the red line should not be referred in the figure as the “ensemble mean simulation”. This is just the mean over the possible different realisations of the evolution of the global mean temperature given a specified external forcing. The red line represents the forced component because averaging over a big number of possible realisation we filter out the natural variability of the system correspondent to that prescribed forcing. Secondly I do not find really necessary and relevant to have the external forcing associated with volcanoes in the figure. Rather I would enlarge (or make a second panel) of the part of figures related to the prediction (the blue line and shading). As it is not much clear. For example it is not so easy to distinguish the increasing in the spread of the blue shading with lead time. [Susanna Corti, Italy]	Our intent with the Figure was to indicate, in a schematic way, the difference between the "simulation" of climate over past years, the "projection" of climate into the (near) future and an initialized "prediction". We have introduced the difference between simulation and projection in the text and Figure caption to make it explicit that the red line is the mean of an ensemble of simulations and estimates the forced component. The discussion makes the point about averaging over the ensemble.The Figure has been revised for

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							clarity.
11-257	11	9	20	9	20	Is this figure 11.1a based on simulations (reference?) or is it artificial? Why do the thin blue lines appear to start from a range of values -mostly below the observation for 2007? [Ian Watterson, Australia]	While the results are based on simulations and forecasts and are not artificial they are meant to be illustrative so the particular details and model involved are not invoked.
11-258	11	9	20	9	25	Fig 11.1a. This figure illustartes the concept of forced and internal components. It is a bit confusing though, because the initialized forecasts (thin blue lines) are not a subset of the forced projections (thin orange lines). I think this might be caused by a bias in the forced response. Perhaps it would be better to illustrate the concept for a different index that does not have such a clear bias. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	We believe that the new version of the Figure satisfies this comment.
11-259	11	9	21	9	21	Why does this discussion not use 1986-2005 base period? If the reason is given somewhere else, please refer to that chapter/section/FAQ. [Government of United States of America]	The Figure is revamped and now uses the "standard" reference period.
11-260	11	9	21	9	22	"A change … is represented as" total = forced + internal. The possibility that this seperation may not be possible or meaningful is not mentioned until lines 35-37. I recommend moving the caveates and comments in lines 35-39 earlier, say at line 23. [Timothy DelSole, United States of America]	Yes, the revamped version of the Box now discusses this earlier, at the beginning of the Box, and, we hope, makes the use of the terms clear.
11-261	11	9	21		21	Why not extend the period up to 2010? [Ibouraïma YABI, Benin]	Don't understand this comment since the axis goes beyond 2010. If it refers to the reference period, this is now changed to the standard period.
11-262	11	9	27	9	39	My understanding is that the term 'climate projection' rather than prediction is also used to indicate that a projection is conditional on the particular emissions scenario assumed. [Nathan Gillett, Canada]	Yes, this is now stressed with a distinction made between "climate simulation" based on specified forcing as for the 20th century and a "climate projection" based on future "scenario dependent" forcing.
11-263	11	9	29	9	53	Are definitions for climate projections and predictions consistent across WG1? [Government of United States of America]	We think this is reasonably consistent since "projection" has been used in a number of reports and appears in the Glossary. Prediction also appears and is used consistently in Chapter 11.
11-264	11	9	29	9	53	The difference between "climate projection" and "climate prediction" is not at all clear. Climate Projection is defined as a process that determines the evolution of forced component " while only the envelope of internal component needs to be specified. Climate prediction is defined as the process that "future evolution of some aspect of the climate system encompassing both forced and internally generated components." If one follows this argument, would seasonal forecasts where there is no attempt to predict the internal component of the atmosphere, be projections or predictions? The point authors may be missing is how the 'Earth System" is broken into "external" and "internal" components, and once this is done, all other aspects are relative to this. [Government of United States of America]	We believe the revised text for simulation, projection, and prediction is now clearer. The terms should also appear in the Glossary. The text notes that the prediction is not of the day to day progression but of some statistic of the system. This would seem to be in accord with a seasonal prediction for the internal component of the atmosphere where, for instance, the prediction is of the anomaly

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							of the seasonal mean temperature.
11-265	11	9	30	9	30	1960 to 2015, not 1900 to 2000. [Fyfe John, Canada]	The text no longer makes this reference.
11-266	11	9	30	9	30	Should it be mentioned that the extensions past present (or probably past 2005) are based on plausible yet hypothetical external forcings? [Fyfe John, Canada]	Yes, this is now done.
11-267	11	9	30	9	30	figure, removal capital F. [Aneesh Subramanian, India]	This no longer appears
11-268	11	9	35	9	36	The equation 'total = forced + internal" assumes not only that F and I are independent, but also additive. [Timothy DelSole, United States of America]	Yes, please see the response to comment 261.
11-269	11	9	35	9	37	This sentence is misplaced here. It should be moved up (close to line 22) where the definition of internal and forced components is given. Furthermore I would add that we assume that this separation is meaningful as a first order approximation. [Susanna Corti, Italy]	Please see the response to comment 261
11-270	11	9	37	0	0	What does "operationally" mean? [Antje Weisheimer, United Kingdom]	Replaced by "in practice"
11-271	11	9	37	9	37	"Tf is obtained by averaging…" The word 'obtained' should be 'estimated.' [Timothy DelSole, United States of America]	Yes.
11-272	11	9	37	9	37	What does "operationally" mean? "In practice" will be a better wording. [Government of United States of America]	Yes.
11-273	11	9	37	9	37	More precisely, "an estimate of Tf is obtained" [Jouni Räisänen, Finland]	Yes. Text is modified.
11-274	11	9	38	9	39	Internal component "averages to near zero" only for large ensembles. [Government of United States of America]	Yes, now indicated.
11-275	11	9	38			Replace 'obtained' with 'estimated'. This is an estimate of T_f, not the true value - it is uncertain due to residual internal variability, forcing uncertainty, and model uncertainty. [Nathan Gillett, Canada]	Yes
11-276	11	9	39			We recommend making the point here explicit by adding a clause like "because the initialization condtions themselves are arranged to ensemble average to zero" to the end of this sentence. [Government of United States of America]	The discussion concerns an ensemble of simulations which are distant from their initial conditions so I don't believe that this applies.
11-277	11	9	41	0	0	Climate prediction: Too long. For me the essence of that sub-section is in 2 sentences starting in line 49 to 51. [Antje Weisheimer, United Kingdom]	We feel it is important to distinguish climate from weather prediction and to give an example of a climate forecast output. The section is somewhat reduced and rewritten.
11-278	11	9	42	9	53	It may be useful to elaborate a bit more on the observational-based initial conditions which are key for the understanding of the predictions/projections distinction, e.g., give an example. [Thomas Stocker/ WGI TSU, Switzerland]	The section has been rewritten in what is hoped a clearer way based on these comments. In particular the difference between the trajectories from the initialized forecast and those of the simulations are noted explicitly.
11-279	11	9	46	9	46	Fig 11.1is a nice illustration but it could generate misunderstandings.For example, that forecasted warming is notably less than projected for the next decade and probably beyond. Is this to be thought of as a schematic or the conveyed information is valid? [Ramon de Elia, Canada]	This is a schematic although based on model output. The example forecast is now for a different period so as to make the plot clearer and avoid the possible misunderstanding. The text notes that initialization can result in forecast that differs from a

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							simulation result and this is the message we wish to convey.
11-280	11	9	47	9	47	Add a sentence or change the phrasing to clearly specify that the models used for predictions are exactly the same as the ones used for projections. The current phrasing may let think that they are different. [Christophe CASSOU, France]	Yes, we now indicate they are often the same.
11-281	11	9	47	9	49	This description of a GCM is very simplistic and naïve. Specifically, the fact that a host of physical processes are parameterized in terms of the large scale, resolved variables is not mentioned. I believe it is misleading to imply GCMs solve "the equations of fluid mechanics, thermodynaics, cloud physics..." I presume GCMs are described carefully in earlier parts of the IPCC report. Perhaps the same phrasing should be used here. [Timothy DelSole, United States of America]	The discussion now avoids this and there is now a reference to Chapter 9 which discusses and assesses models.
11-282	11	9	51	9	51	Fundamental difference yes - but it is probably also the ONLY major difference, apart from in the interpretation of the results [Government of Australia]	This sentence is now omitted.
11-283	11	9	55	9	56	"... that if the governing equations are integrated forward in time from identical initial conditions the evolution of the system is reproducible." It may help improve the readability to adjust the expression order and put the "if ..." part after "the evolution ..." [Gan Zhang, United States]	This sentence is now omitted.
11-284	11	10	3	10	3	p.10: The blue lines are not very distinct. If the lines can be made more visually distinct, this would help the reader interpret the difference better. -- Written dec 2, 2012 [Aneesh Subramanian, India]	The revamped Figure now displays purple lines which seem clear in our version at least.
11-285	11	10	3			We recommend making the point here very very explicit by adding a clause "centered on a best estimate of the 2007 state of the climate system" after the word "conditions" [Government of United States of America]	The wording is now changed to indicate the initial conditions are observation-based estimates. We avoid using "best" estimate which suggests a single initial state and a single forecast.
11-286	11	10	5	10	6	This sentence is unclear to me. It should be simple to explain. [Antje Weisheimer, United Kingdom]	The section is now rewritten.
11-287	11	10	5	10	11	There are numerous problems with this paragraph. The ensemble mean does not "attempt to predict the actual evolution," rather, it provides the best point estimate, in the sense of minimizing the mean square error. The 'truth' is not expected to match the ensemble mean, rather the truth is viewed as a randomly chosen ensemble member. The term 'forecast error' could be defined less confusingly, namely as the difference between observed and ensemble mean. The clause 'If differences in initial conditions represent observational and analysis errors" seems unnecessary: even if the ICs are not accurate and generate initialization shock, I would still characterize the difference from observations as "forecast error." The spread does not indicate "the likelihood," but rather the 'uncertainty'. Some of this material is covered much better in sec. 11.3.1.1 (Uncertainty in Near-term climate projections), so perhaps these paragraphs could be consolidated. [Timothy DelSole, United States of America]	Following the suggestion, the prediction section is now shortened and rewritten in a way that we hope avoids these difficulties. Please also see the response to comment 290.
11-288	11	10	6	10	7	I'm not sure I agree that, philosophically, the mean of a ensemble is meant to actually capture the observed behaviour of a system. It should be the best estimate of the predictable component of the observations (for an unbiased ensemble) but the observations will always be contaminated by unpredictable noise/chaos. It is often the case that the ensemble mean gives the best skill score but this is just an emperical finding. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Please see the response to comment 290
11-289	11	10	6	10	7	Ensemble mean forecast does not attempt to predict the actual evolution. It is individual ensemble members that attempt to predict the actual evolution. In fact, ensemble mean, by design, can NEVER predict the internal component of variability (even by chance). Ensemble mean attempts to predict the most likely outcome, and doing so maximizes deterministic measures of skill (e.g., AC or RMSE). [Government of United States of America]	Yes, this was misstated and the text has been modified in line with this comment.
11-290	11	10	6	10	7	Suggest changing to something like "The ensemble mean forecast (the dark blue line) removes unpredictable components and is the best attempt to predict the actual evolution (the black line) in Figure 11.1a"	Please see the response to comment 287

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						[Doug Smith, United Kingdom of Great Britain & Northern Ireland]	
11-291	11	10	6			… interest.The (typo) [Government of France]	Text is edited.
11-292	11	10	9	10	9	"Under suitable circumstances" What are these? Either specify or delete. [Government of United States of America]	The phrase has been replaced by an alternative formulation which we hope conveys the intended information.
11-293	11	10	9	10	15	The distinction between probabilistic and deterministic is not really made here. In line 11, the deterministic forecast is said to generate spread, which in turn can be used to estimate a probability distribution. Next, a probabilistic prediction is said to take the form of a probability distribution. Thus, by definition, an ensemble deterministic forecast is probabilitistic, hence there is no distinction. In fact, I would argue that a deterministic forecast can be interpreted as a probabilistic forecast in which the probability is a delta function (has no uncertainty). [Timothy DelSole, United States of America]	The section has been rewritten in a clearer way based on these comments. We no longer attempt to make this distinction explicit since, as is pointed out, it has a technical aspect which would be out of place in the Box.
11-294	11	10	11	0	0	This sentence is also not clear. What's the link of a probability distribution when talking about deterministic forecasts? [Antje Weisheimer, United Kingdom]	Please see response above.
11-295	11	10	13	10	13	We recommend that a better wording will be "…takes form of likely probability for an outcome." [Government of United States of America]	Please see response above.
11-296	11	10	20	0	0	Climate predictability: Too long. [Antje Weisheimer, United Kingdom]	It is now somewhat shortened although it is a topic the reader may not be familiar with so deserves some discussion.
11-297	11	10	21	10	21	We suggest deleting "A physical system…subsequent states." Not sure how it connects with the subsequent discussion. Alternatively, say that even for a deterministic system, uncertainties in initial conditions can lead to uncertain outcomes. [Government of United States of America]	This was inserted in response to an earlier comment. However the text is now removed.
11-298	11	10	21	10	24	These sentences effectively state "Predictability represents an upper limit to forecast skill." This assertion can be proven explicitly using information theory, as shown by DelSole (2005, J. Atmos. Sci., 3368-3381). [Timothy DelSole, United States of America]	Yes, although this statement has been edited out.
11-299	11	10	27	10	27	vice versa' does not make sense here [Timothy DelSole, United States of America]	Seems OK in the sense that when states separate slowly and the pdf broadens slowly predictability is high.
11-300	11	10	30	10	35	Maybe this could be cut. [Antje Weisheimer, United Kingdom]	We have reduced this somewhat but feel it is useful to retain as explanatory material
11-301	11	10	36	10	45	This paragraph does not add much to the chapter, and seems repetitive. I recommend deleting it, and moving some of its contents to earlier paragraphs. [Timothy DelSole, United States of America]	Yes. This paragraph has been removed.
11-302	11	10	36	10	45	In describing climate predictability and how it is quantified, it is stated that the estimate is model-based. We believe this is different from the Forecast model in the sense that the model is considered to be perfect. If so, this could be spelled out so that the reader would be able to distinguish the two and appreciate why the forecast quality is generally lower than the predictability. [Government of United States of America]	Yes. In response to this and other comments we have removed the paragraph.
11-303	11	10	36	10	45	I felt this whole paragraph is a bit out of place and doesn't fit in there very well. [Antje Weisheimer, United Kingdom]	Yes. Paragraph is removed as noted above.
11-304	11	10	44	10	44	What is discussed is also refereed as "conditional predictability" [Government of United States of America]	Paragraph removed as noted above.
11-305	11	10	48	10	54	Much of this material is repetitive with lines 21-34. Seems like this could be organized better. [Timothy DelSole, United States of America]	We have removed the sentence involved.
11-306	11	10	49			The way this sentence defines 'retrospective forecast' is unclear: is it 'the average', or is it 'forecasts made for	Wording has been changed to avoid this possible

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						past cases.' It is the latter, but the sentence could be interpreted as the former. [Timothy DelSole, United States of America]	misperception.
11-307	11	10	56	11	6	This paragraph is difficult to undestand because the concepts have not be introduced. What is the "corresponding predictability measure"? The legend of Fig.11.2 is not explicit enough and "potential skill" in the graph is used instead of "corresponding" predictability. [Christophe CASSOU, France]	The wording has been changed to try to avoid this difficulty. This is meant to convey the importance of initial conditions vs forcing as the time over which the forecast is averaged increases.
11-308	11	10	56	11	6	To make this discussion easier to follow in terms on the labelling in Fig. 11.2, suggest modifying to (with [] denoting added or modified text): "Predictability and forecast quality, and their dependence on the time average and the range of the forecast, are illustrated in Figure 11.2 which plots the global average of the local correlation skill score and the corresponding predictability measure[, termed "potential skill",] for temperatures averaged over periods from a month to a decade. Initialized forecasts exhibit enhanced skill compared to uninitialized simulations at shorter time averages and forecast ranges but this advantage declines for longer timescales. In this example at least (based on Boer et al., 2012) the [actual] skill[s] of the initialized predictions and of the uninitialized simulations become indistinguishable beyond about a three-year average forecast. The corresponding [potential skills] do not converge with timescale, however, suggesting that gains in [actual] forecast skill may be possible." [William Merryfield, Canada]	Please see the response to comment 308.
11-309	11	10	57			There is not enough information in fig. 11.2 to understand it. Is the skill computed at each grid point then averaged, or is the temperature averaged first, then the skill computed? How is actual and potential skill measured? Boer (2012) is cited, but the references have only Boer (2011). [Timothy DelSole, United States of America]	Please see the response to comment 308. The reference has been updated.
11-310	11	11	1			What model or set of models was used to make the predictions described? [Nathan Gillett, Canada]	The text now indicates that the result is from a model and a reference is given if the reader wants details.
11-311	11	11	4			indistinguishable' – no error bars on corresponding figure to make this a robust statement [Ed Hawkins, United Kingdom]	Since the Box is intended to be explanatory, the text has been changed to make the statement descriptive rather than quantitative.
11-312	11	11	6	11	6	Please state that the "potential skill" estimate is based on perfect model approach. The inference about that "potential skill" is "room for improvement" has the caveat that observations have the same signal-to-noise as the model. It is very easy to think of system that have low signal-to-noise because of noise being large, and can have lower "potential skill" than actual skill (particularly for deterministic measures like AC. As an example of this see Fig. 2 in Mehta et al., 2000, 27, 1, 121-124 - Oceanic influence on NAO..[Mehta, V. M., M. J. Suarez, J. V. Manganello, and T. L. Delworth (2000), Oceanic influence on the North Atlantic Oscillation and associated northern hemisphere climate variations: 1959–1993, Geophys. Res. Lett., 27(1), 121–124, doi:10.1029/1999GL002381.]. So please don't relate to difference in potential predictability with "room for improvement" unless some other basic validations (e.g., comparison of total variance etc.) is also given. [Government of United States of America]	We now state that the predictability is model-based and omit the "room for improvement" statement as suggested.
11-313	11	11	14	11	14	for emphasis and consistency with main text (p. 11-11 line 4), could modify to: "[uninitialized] simulations (light grey shading) and the [initialized] forecasts (light blue shading). The grey areas along the axis broadly indicate" [William Merryfield, Canada]	The figure is revamped and no longer contains shading.
11-314	11	11	24		29	Which model (s)? Globally averaged temperature? If its multimodel, would be good to show the individual ones as well, Helen Hanlons paper over Europe had one model with poor skill score for European summer drag the skill score of the MM down (Hanlon et al., 2012b). [Gabriele Hegerl, United Kingdom]	Please see the response to comment 311.
11-315	11	11	31	11	31	the concept of "perfect predictability", shown also in some figures, should be included/explained [Annalisa Cherchi, Italy]	There is a heading labelled "Climate predictability" in the box which attempts to explain this.

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11-316	11	11	33	0	0	Section 11.2 Near-term predictions: The way this section is written lacks homogeneity. There is a lot of repetition where similar facts are explained several times at different places throughout the section using different terminology and a too varying level of scientific language. There is lot of potential to shorten the Chapter in this section by avoiding overlap and explain certain results in a more straight forward and structured way. [Antje Weisheimer, United Kingdom]	The section has been considerably rewritten in response to this and other comments
11-317	11	11	33			I think this section would benefit from more explanation of the various measures of skill shown and discussed. Overall there is a tendency to use specialist language without full explanation. For example in Fig 11.3 the meaning of 'mean squared distance' is not explained. In Figure 11.2, correlation skill score is not defined, nor is the 'corresponding predictability measure'. Brier skill score is not defined or described in Figure 11.8. Root mean square skill score is not defined or explained in Fig 11.7. This topic is new to IPCC, so although these terms may all be clear to specialists, I think it would be helpful to take more time to define, described and interpret these various diagnostics in the chapter. Also, many of the plots do not appear to come from the literature and have not cited refs, so if the reader wants more information on a particular plot or diagnostic, there is not original source to go to. This strengthens the case for more description of the diagnostics shown. [Nathan Gillett, Canada]	Please note that the chapter is already long. It is not the role of the IPCC report to provide educational material. References for people who want to follow up on the definitons are provided. Nevertheless the Brier skill score has been explained in the caption of Fig 11.8 and references to the literature have been added to the figure captions.
11-318	11	11	35	0	0	11.2.1 Introduction: After 11 pages into the Chapter with an executive summary and another Introduction before, I feel this introduction should be either shortened or merged with the following sub-sections. [Antje Weisheimer, United Kingdom]	The ES has been recduced substantially. 11.2.1 has been shortened.
11-319	11	11	35	12	20	Comment on section 11.2.1. The uncertainties associated with the strong hypotheses made a-posteriori to dig out the forecast signals from raw predictions are completely absent in the present version but are crucial, in my opinion. Among what I would call "methotodologies uncertainties", I would mention the estimation of the drift in presence of volcanic eruptions, the very crude way drifts are removed, the hypothesis of addititivity of the drift and the signals to be forecast and so on. The drift issue is central in decadal forecast, even in anomaly initialisation, and it is indeed a huge difference with the projections. All this issue is a bit but not enough stated throughout the text in the chapter in general. It is extremely important especially for the impact community to warn them and make them understand that they cannot use decadal predictions without a priori and specific treatments (leadtime) of the outputs ... It is also extremely important to state that we don't know, to my knowledge, how to debias the daily fields traditionnaly used for extreme studies. [Christophe CASSOU, France]	We agree and aspects such as model "drift" are discussed as part of model error in Section 11.2.3 rather than in this introductory section. We also agree with the reviewer that there is a certain methodological uncertainty and have made it clearer in the revised text. The important role of, and the difficulties associated with, the calculation of the predicted anomalies have been described in section 11.2.3.1, where the issues mentioned by the reviewer have been added
11-320	11	11	37	12	20	This is the 3rd or 4th time forecast uncertainty has been discussed so far in this chapter. Plus, uncertainty is discussed in even more detail in sec. 11.3.1.1. All these statements about uncertainty should be collected and consolidated into a single coherent section, instead of sprinkled and repeated throughout the chapter in multiple places. [Timothy DelSole, United States of America]	The section has been considerably reduced. We feel there is a difference in kind in the uncertainty associated with forecasts and simulations. Initial condition uncertainty is very much more important while uncertainty associated with forcing is less so for GHGs and more so for volcanic aerosols and so on. For this reason we hope retaining some discussion in the reduced introduction is suitable. See also response to 318.
11-321	11	11	37	15	18	Section 11.2.2: There is an overall reliance of the chapter on solely a dynamical climate modeling approach with no discussion of the merits of alternative approaches that may provide a similar level of skill and reliability such as either statistical or hybrid modeling approaches. The decadal prediction discussion barely touches on statistical approaches two of which are part of the experimental decadal prediction effort hosted by the Met Office. Hoerling, M., J. Hurrell, A. Kumar, L. Terray, J. Eischeid, P. Pegion, T. Zhang, X. Quan, and T. Y. Xu, 2011 (August): On North American decadal climate for 2011-20. J. Climate, 24, 4519-4528. doi:10.1175/2011JCLI4137.1 have published a paper on a hybrid approach for the predictability of North American climate which takes into account both forced climate change and natural decadal-scale climate variability over the next decade. Given the current skill and reliability of dynamical climate modeling produced near-term climate change projections and predictions, the assessment should broadly survey and assess all methods and approaches to inform decision makers on near-term changes in climate and associated impacts. Two of our reviewers noted this. [Government of United States of America]	The reason for the lack of observation-based studies is the limited data base rather than their neglect. In any case, the reference alluded to is now added to the diagnostic predictability section where it appears with other statistical approaches, a number of which are recent and recently added. Statistical approaches are also noted in the section on prediction. Paco: Statistical methods are referred to in section 11.2.3.1. The reference provided by the reviewer has been added to that section.

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11-322	11	11	38	11	39	It is not appropriate to say "predictions are usually made with coupled climate models." It depends on who you are-- I usually make predictions with empirical models. The discussion should give a balanced presentation of dynamical and empirical predictions, especially since studies suggest that empirical models can be better than dynamical models (Newman 2012). [Timothy DelSole, United States of America]	The sentence the reviewer mention has been corrected and a explicit mention of empirical-statistical methods has been included. Newman's reference has been added to section 11.2.3.1. Also, the revised text clarifies that the skill level for near-surface temperature is similar for both dynamical and statistical methods and that a combination of the two types of methods will be explored in the future.
11-323	11	11	43	11	46	This paragraph should also state that skill of decadal prediction is also limited by predictability limits, and it is not just a matter of improving models. Also, reducing uncertainty below that the observed system has will lead to incorrect predictions, and so in itself, it is not a desired goal. Better estimating uncertainty is the right goal. [Government of United States of America]	The statement concerning improvement to predictions no longer appears since it is difficult to formulate precisely. We cannot understand,however, how to reduce observational uncertainties below what they are, although we can understand that misunderstanding the level of uncertainty can be misleading.
11-324	11	11	48	11	53	Initialisation is also limited by model error [Noel Keenlyside, Norway]	Model inadequacy has been added.
11-325	11	11	49	11	50	Obscure sentence. [Antje Weisheimer, United Kingdom]	It is the term normally used and has both a technical and nominal meaning. Nevertheless (sigh) "assimilate" is replaced by "incorporate" even though this is somewhat vague. Paco:The sentence has been modified to "To the extent that the initialization procedure is successful the model state will incorporate the effect of past radiative forcing on the climate system."
11-326	11	11	53	11	53	perhaps useful to reemphasize why initial errors grow: "[inevitably] grow as the forecast progresses due to the chaotic nature of the climate system thereby limiting the time for which the forecast will be useful." [William Merryfield, Canada]	Although we agree, we avoid reemphasizing this for space considerations, and since other comments chide us for repitition. Paco: Done.
11-327	11	11	55	11	56	Errror .. Will introduce errors … Not a very good sentence. [Antje Weisheimer, United Kingdom]	The first "Errors" has been changed to "Deficiencies".
11-328	11	11	55	11	58	I think the authors should think carefully about using the word “error” here and elsewhere in the chapter unless it is for some standard term such as Mean Squared Error. To the layman, error gives the impression that climate scientists have made stupid mistakes, which is hopefully not the case! A more neutral word would be “discrepancy”. It would be worth pointing out in this definition that model uncertainty means uncertainty in predictions of future observations due to the use of imperfect models. This section could be consistent with definitions in 12.2.2 and should cross-reference it. [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Helpful point and we replace error with "imperfections" and also change the wording somewhat. Paco: The word error has been kept to mean the discrepancies between the predictions and the observations. However, it was decided the use of "inadequacy" to refer to the lack of knowledge in the design of dynamical models. The reviewer's comments have been used to increase the consistency in the decadal prediction section.
11-329	11	11	55			Model uncertainty is treated rather loosely in this chapter. There is an explicit assumption that considering outputs from multiple models incorporates "model uncertainty". But there are several elements to model uncertainty: (a) the kinds of differences and problems with the equations within models that is alluded to here, but also (b) issues with what the models have been constructed and (more or less) calibrated to recreate. The climateprediction.net make compelling arguments that most IPCC ensembles are too narrow because they draw strictly from models that are all tuned/constructued to reproduce as closely as possible a very narrow range of visions and experiences of "how the current climate works". In their explorations of "parameter uncertainties" (which is an element of "model uncertainty" that falls within the loose language here, but which is essentially ignored in this chapter), they have loosened those tunings in plausible ways, and as a result develop much broader ensembles. Our suggestion here and throughout is to be much clearer about what all constitutes "model uncertainty" and to avoid, studiously, the false equivalence between multimodel spread and a full characterization of model uncertainties. An example of the "parameter uncertainty = model uncertainty"	The reviewer might refer to the sentence "A second ENSEMBLES contribution (DePreSys; Smith et al., 2010) was run by the Met Office using a nine-member ensemble of HadCM3 model variants sampling modelling uncertainties through perturbations to poorly constrained atmospheric and surface parameters." This sentence does not equate the spread of the forecast system to an estimate of the model uncertainty. The spread of the predictions is the result of sampling the initial-condition uncertainty and the model uncertainty, the latter by using either perturbed parameterizations (such as the

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						assumption appears on page 11-18, line 30. Note: This is mostly a language thing, rather than any strong recommendation that the analyses or results be changed. [Government of United States of America]	climateprediction.net effort, which in fact does not perform predictions) or multi-models. It is hence impossible to separate the contribution of each source of uncertainty to the spread given that each simulation has different initial conditions and a different forecast system or model version. The reviewer's suggestion has been taken into account by adding this explanation to the revised text.
11-330	11	11	57	10	57	Please define "local correlation skill score". [Fyfe John, Canada]	This is in reference to pg 10, ln 57. This is no longer used in the text and the meaning is stated more precisely in the Figure caption.
11-331	11	11				Sections 11.2 and 11.3 could be better integrated. An example is the discussion of Fig. 11.33 and the associated statement that the actual warming will "more likely than not" be closer to the lower bound of 0.4C than the upper bound of 1.0C. While there may be other factors (e.g., model errors, insufficient aerosols) to suggest that the CMIP5 projections are too warm, one should also consider the results of the initialized predictions (Fig 11.1a) that appear to support a slower warming than the projections would indicate in the near term. [Government of United States of America]	This is now considered in sections 11.2 and 11.3, but also in Box 9.2.
11-332	11	12	1	12	6	Add also the effect of the anthropogenic aerosols in addition to the volcanic ones. The latter are responsible for most of the dicrepencies in the RCP over the 2006-2035 period and explain a very large part of the forcing uncertainties. As stated in several sections of the report, the aerosol forcing might be responsible for the recent "plateau" in the late 2000's. [Christophe CASSOU, France]	This is now referred to in the revised text.
11-333	11	12	4			This statement can be misread (if extreme care is not taken by the audience) to imply confidence about a broader issue; therefore recommend "will not change" --> "may not change" [Government of United States of America]	The wording has now been changed to "not expected to change appreciably"
11-334	11	12	9	12	9	Forecast uncertainty in this line may refer to "initial conditions uncertainty" [Ramon de Elia, Canada]	We agree but feel it is clear from the context.
11-335	11	12	9			In the interest of total clarity, we recommend " system generate" be replaced with "system, taken together, generate" [Government of United States of America]	The section has been edited somewhat and this suggestion is incorporated
11-336	11	12	14	12	14	Which box? [Susanna Corti, Italy]	Reference to Box no longer appears.
11-337	11	12	24	11	29	This has either been said before or belongs to the very beginning of the Chapter. [Antje Weisheimer, United Kingdom]	The paragraph referred to in this comment has been removed.
11-338	11	12	24	12	28	This 5-line sentence is convoluted, not grammatically correct, and can probably be omitted. [Timothy DelSole, United States of America]	Paragraph removed.
11-339	11	12	24	12	28	This sentence is very difficult to follow. Suggest restructuring to: "The scientific impetus for decadal prediction [has arisen] from improved understanding of the physical basis of long timescale variations in climate and improvements in climate models (Chapter 9), the availability of information on the state of the atmosphere, ocean, cryosphere and land (Chapters 2–4) and [] predictability studies, [initial] decadal forecasting attempts and [] the development of multi-model and other approaches for combining, calibrating and verifying climate predictions[, considered in this chapter]." [William Merryfield, Canada]	Paragraph removed.
11-340	11	12	31	0	0	11.2.2.1 Predictability Studies: I don't think the distinction between prognostic and diagnostic predictability studies is very useful. Also, the overall weight put on predictability studies versus Climate Prediction (11.2.2.2) is too high. [Antje Weisheimer, United Kingdom]	This section has been revised and reduced. However, the distinction between prognostic and diagnostic predictability studies is retained since the approach is different and usually the form of the results are different. There is considerably less space devoted to predictability than to prediction.
11-341	11	12	33	12	38	This paragraph is very repetitive with sec. 11.1. I recommend removing this paragraph and consolidating its contents with similar statements in 11.1 [Timothy DelSole, United States of America]	Now omitted

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11-342	11	12	35	0	0	verisimilitude? [Antje Weisheimer, United Kingdom]	Now omitted (but not because of verisimilitude)
11-343	11	12	44	12	49	add "Pohlmann et al. (2012)" Pohlmann, H., D. M. Smith, M. A. Balmaseda, N. S. Keenlyside, S. Masina, D. Matei, W. A. Müller, and P. Rogel, 2012: Predictability of the mid-latitude Atlantic meridional overturning circulation in a multimodel system. Submitted to Climate Dynamics. [Holger Pohlmann, Germany]	Yes, done
11-344	11	12	56	13	3	It is confusing to talk about the PDO, PDV and IPO which seem to be much the same thing. Would it be clearer to say that Pacific variability has been diagnosed in different ways (ie. the PDO and IPO) but will be refered to here as PDV? [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Yes, done
11-345	11	12				There is no mention of the the relationship between useful predictability and realism of representation of the phenomenon of interest. For initialized prediction this presumably becomes more important the longer the timescale. For example, early on some models were used for seasonal forecasts even though their climate did not include ENSO. What is the impact of a forecast of the AMOC with a model that has weak inherent AMOC? [Government of United States of America]	Yes, the text now notes that the predictability estimates are model based and that the realism of the AMOC in the models is not easily assessed.
11-346	11	13	8	13	12	Too long a sentence. [Antje Weisheimer, United Kingdom]	Please see response to 348.
11-347	11	13	8			Although they are a bit older, references to Gershunov et al (e.g., 1999, Eos [Gershunov, A., T. P. Barnett, and D. R. Cayan (1999), North Pacific interdecadal oscillation seen as factor in ENSO-related North American climate anomalies, Eos Trans. AGU, 80(3), 25, doi:10.1029/99EO00019.]) and McCabe and Dettinger (1999, Int. J. Climatol. 19: 1399–1410) are called for here. [Government of United States of America]	The part of the section dealing with mechanisms, rather than predictability as such, is now removed. It was also thought that the discussion would be difficult for the reader unless there was a lot of explanation. The text now refers readers to other chapters for a discussion of the variability mechanisms if they are not familiar with them. .
11-348	11	13	12	13	12	add "There may be also an influence from the stratosphere (Reichler et al. 2012; Manzini et al. 2012)" Reichler, T., J. Kim, E. Manzini, and J. Kröger, 2012: Stratospheric connection to Atlantic climate variability. Nature Geoscience, in press; Manzini, E., C. Cagnazzo, P.G. Fogli, A. Bellucci, and W. Müller, 2012: Stratosphere - Troposphere coupling at inter-decadal time scales: Implications for the North Atlantic Ocean. Geophys. Res. Lett., 39, L05801, doi:10.1029/2011GL050771. [Holger Pohlmann, Germany]	Please see the response to comment 348.
11-349	11	13	12			Should be referencing various recent publications by Bromirski et al at Scripps Institution of Oceanography (e.g., recent Eos cover story [Bromirski, P. D., A. J. Miller, and R. E. Flick (2012), Understanding North Pacific sea level trends, Eos Trans. AGU, 93(27), 249, doi:10.1029/2012EO270001.]) here. [Government of United States of America]	Sea-level is beyond the scope of Ch 11. Please see Chapter 13 for a full discussion of sea level.
11-350	11	13	14			"deep" ocean quantities. This terminology needs to be clarified. Most figures show upper ocean temperatures, not deep ocean. Do the authors mean "sub-surface"? Figure 11.25, for example, shows very little warming signal below 1000 m. [Government of United States of America]	This section has been edited for length and the connection to predictability. Upper ocean temperature is now referred to.
11-351	11	13	27			think' should be 'thick'. This sentence is not grammatically correct. [Timothy DelSole, United States of America]	The figure no longer appears.
11-352	11	13	33	13	39	Would papers by Zanna et al. need to be cited here? [Antje Weisheimer, United Kingdom]	Yes, now done.
11-353	11	13	34	13	39	Add a reference to Zanna (2012) and Newman (2012) who found that LIM gives predictions comparable and sometimes better than CMIP5 initialized hindcasts: Zanna L., 2012: Forecast Skill and Predictability of Observed Atlantic Sea Surface 290 Temperatures. J. Climate, 25, 5047-5056. Newman, M., 2012: An empirical benchmark for decadal forecasts of global surface temperature anomalies. Submitted to J. Climate. [RYM MSADEK, United States of America]	We consider Newman (2012) to be a study of forecast/prediction skill/quality rather than of predictability as such. Zanna (2012) is now referenced.
11-354	11	13	37	13	38	Swap order of HS09 and Tziperman et al 2008, and add 'respectively' after 'model output'. Also Zanna (2012) perform a similar analysis using observed SSTs. [Ed Hawkins, United Kingdom]	OK. Also added Hawkins et al reference

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11-355	11	13	39	13	39	of instead of "of from" [Aneesh Subramanian, India]	Sorry but we can't find this.
11-356	11	13	39			'Several decades' is not true for North Atlantic SSTs [Ed Hawkins, United Kingdom]	this is amended
11-357	11	13	45	13	45	Increase the following sentence: "Luo et al. (2012) develop a statistical- dynamical combined approach by considering the regulation of internally generated oscillations like AMO and IPO, and find moderate prodictability for SST in the Atlantic and the Pacific. " Please refer to Luo, F.-F., S. Li, Y.-Q. Gao, et al., 2012: A new method for predicting the decadal component of global SST, Atmos. Oceanic Sci. Lett., 5,in press. [Jianqi Sun, China]	This is understood to be a forecasting method rather than a study of "predictability" as such.
11-358	11	13	46	13	47	The definition of 'potential predictability' is too vaguely described to be meaningful. Also, I believe that the most obvious interpretation of this sentence is incorrect. Potential predictability is defined here essentially as the ratio of low-frequency variance to total variance. This definition does not match that given by Madden (1976, Mon. Wea. Rev.), and a simple counter example shows it is incorrect. Specifically, we all agree that white noise is totally unpredictable, yet the ratio of low-frequency to total variance is non-zero (since white noise has power at all frequencies). Thus, taken literally, this definition implies that white noise is 'potentially predictable,' which seems inappropriate. Madden defined PP as the amount of variance at low frequencies that exceeds some estimate of 'weather noise' derived from high-frequencies (i.e,. 'the white-noise low-frequency extension'). There are many ways to estimate weather noise, many of which are reviewed in (Feng et al., Mon. Wea. Rev, 2012, submitted). Boer (2000) essentially proposed the 'Shukla-Gutzler' method, which DelSole and Feng (2012, Mon. Wea. Rev., 2012, in press) showed has serious problems. In general, the Shukla-Gutzler method underestimates potential predictability (a fact shown more clearly in Feng et al, 2012). [Timothy DelSole, United States of America]	The meaning of the term "potential predictability" (PP) as used in this section is now clarified by means of a simple equation in the text. After talking to the reviewer in order to understand the comment, the text now also indicates that there is a statistical test that should be considered before the fractional variance can be considered to represent "potential predictability". It is also noted that that there are various approaches to PP. Feng et al. (which includes the reviewer) has not yet been accepted for publication but in a preprint the authors consider 4 different methods of estimating PP on seasonal timescales, each with its attendant statistical aspects. In the 4 cases treated, results depend on how the method applied and the nature of the data. Some of these aspects were considered in Boer (2004) and are similar to, but not the same as, the so-called SG method analyzed in DelSole and Feng(2012). The difficulties with the methods depend on the situation and the approximations and results in Figure 11.4 are believed to be essentially correct and to give useful information on the geographic distribution of PP for decadal means of temperature.
11-359	11	13	46	13	56	Hawkins et al, 2011, shows that the potential predictability, as measured by the ratio of low-frequency to high frequency, is not a robust measure of expected skill in all regions In particular, there was skill found in the tropical North Atlantic, which was not a region which had significant potential predictability. Ref = Hawkins et al, 2011, 'Evaluating the potential for statistical decadal predictions of SSTs with a perfect model approach' [Jonathan Robson, United Kingdom]	Hawkins et al consider the potential predictability measure p although their calculation may be biased (it is not clear from their text) and no statistical tests are mentioned. As now noted explicitly in the text, it is not claimed that $p > 0$ implies forecast skill as such but only the "potential" thereof. As for the Hawkins et al result in the tropical Atlantic where skill is claimed when p is small, the result does not appear to be robust across models or across forecast methods in Figs 1-3. and includes an unexpected increase in apparent skill at longer compared to shorter timescales for the case in question. Boer et al, (2013) provide a derivation that limits $r^2 < p$ for suitably calcualted quantities and it is not clear that this relationship is breached if confidence intervals are considered. Even if it were, it would not indicate that potential predictability measures are somehow everywhere incorrect but might indicate that this region somehow violates the assumptions made in calculating p.
11-360	11	13	46	13	57	the details of definition/computation of potential predictability described and shown also in fig.11.4 should be	The basic idea of potential predictability is now made

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						given [Annalisa Cherchi, Italy]	clearer I hope. Details of computation etc. are available in the references.
11-361	11	13	46			Which "long time scales"? Here and in the caption to the figure, no indication is given of what is meant (exactly) by this term as used here. [Government of United States of America]	The caption now explains that it is the variance of decadal means that is considered.
11-362	11	13	51	13	53	This discussion and Figure 11.4 do not specify what temperature is being shown. Is it upper ocean? Please be specific. [Government of United States of America]	It is surface air temperature and this is now stated explicitly in the caption.
11-363	11	13	52	13	52	Fig 11.4. Near Antartica we can see higher predictability at 10year than at 5year for internally generated varaince. This appears elsewhere too but not as obvious. An explanation may be useful. [Ramon de Elia, Canada]	The figure now concentrates on the variance of decadal means since the assumptions in calculating p are better satisfied in this case
11-364	11	13	55	13	57	This sentence is no longer accurate, since as described in the caption to Fig. 11.4 (p. 11.14, lines 2-5) it does in fact show results based on CMIP5. [William Merryfield, Canada]	Yes, corrected.
11-365	11	14	8	14	14	It is mentioned above that the results of potential precitability studies may depend on the verisimillitude of the model use (nice phrase by the way). Perhaps this summary section could expand on that? Are current models likely to over- or under-estimate potential predictability? [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Boer and Lambert (2008) shows that the pooled multi-model standard deviation of annual mean temperature and precipitation compares well with an observation-based estimate as justification for further calculation and other publications take various approaches to this aspect. The intent here is to remind the reader that the results are model based without going into detail. As far as I am aware there are no publications assessing the likely over- or under-estimation of predictability using current models.
11-366	11	14	8	14	14	p.14: Emphasizing the role of the ocean feedback mechanisms and how it impacts the long timescales predictability studies would help the summary better. Is it due to ocean heat storage or memory in oceanic modes of variability such PDO, ENSO and other decadal and quasi-decadal oscillations ? -- Written dec 3, 2012 [Aneesh Subramanian, India]	The summary is considerably changed but it is not possible to give these details in the current state of the science.
11-367	11	14	9		9	I think the concensus still does not mean reliability. I think that rather tend towards the approaches and reliable, but not concensuels [Ibouraïma YABI, Benin]	The summary is considerably changed and I hope this is no longer a concern
11-368	11	14	11	14	12	Precipitation ahs not been discussed before and I'm surprised to see this statement in the summary. What's the evidence for it? [Antje Weisheimer, United Kingdom]	The text now includes a paragraph referencing the few available studies of precipitation predictability on decadal timescales that we are able to find.
11-369	11	14	14			"4-9 yrs" does not appear in the section being summarized and thus should not be freshly introduced in the summary subsection, before the case is made for it. [Government of United States of America]	Yes, this no longer appears.
11-370	11	14	19	14	19	Graham, R., and Co-authors, 2011: New perspectives for GPCs, their role in the GFCS and a proposed contribution to a ‘World Climate Watch’. Climate Research, 47, 47-55, doi: 10.3354/cr00963. [Government of United States of America]	This subsection has been removed to comply with the chapter length restrictions.
11-371	11	14	21			Eyring et al. (2010) describes projections of ozone change (based on a particular scenario) - it doesn't deal with deterministic predictions. [Nathan Gillett, Canada]	This was a mistake. The reference is no longer there, as well as the whole subsection, which has been removed to comply with the chapter length restrictions.
11-372	11	14	31	11	32	This is also seen in Fig. 5 of Keenlyside and Ba (2010) Keenlyside, N. S., and J. Ba, 2010: Prospects for decadal climate prediction, Wiley Interdisciplinary Reviews: Climate Change, 1, 627-635 [Noel Keenlyside, Norway]	The reference has been added.
11-373	11	14	32	14	38	Very long und poorly structured setence. [Antje Weisheimer, United Kingdom]	The sentence has been revised.
11-374	11	14	32			We feel this sentence is difficult to read due to its length and the number of issues it attempts to cover. We	The sentence has been revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						recommend clarification. [Government of United States of America]	
11-375	11	14	34			The authors mention "the amplitude of the relaxation term", but this seems out of place. What does that have to do with assimilation of SST? [Government of United States of America]	The relaxation term used in the coupled simulation that is used to generate the initial conditions is purely empirical. We found important to clarify that this methodology suffers from this fundamental caveat.
11-376	11	14	36			change Matei et al. 2012 to Matei et al. 2012a (Matei, D., H. Pohlmann, J. Jungclaus, W. Müller, H. Haak, and J. Marotzke, 2012 : Two tales of initializing decadal climate predictions experiments with the ECHAM5/MPI-OM model. Journal of Climate, doi:10.1175/JCLI-D-11-00633.1.) [Daniela Matei, Germany]	Done.
11-377	11	14	41	14	45	Here there are also several relevant publications from the GFDL group: e.g., Zhang, Shaoqing, Anthony Rosati, and Thomas L Delworth, October 2010: The adequacy of observing systems in monitoring AMOC and North Atlantic climate. Journal of Climate, 23(19), doi:10.1175/2010JCLI3677.1. [Noel Keenlyside, Norway]	The reference is very useful and has been added.
11-378	11	14	42	14	42	A reference is missing for this statement. [European Union]	The sentence has been modified and a reference added.
11-379	11	14	48	14	48	In addition to Hazeleger 2012b, other studies assessing full field and anomaly initialization are: (1) Smith, D. M., R. Eade and H. Pohlmann, 2012, A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction, Climate Dynamics, submitted (in revision) (2) Magnusson, L., M. Balmaseda, S. Corti, F. Molteni and T. Stockdale (2012), Evaluation of forecast strategies for seasonal and decadal forecasts in presence of systematic model errors. Accepted [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The references have been added.
11-380	11	14	48	48	14	About the these two initialisation approaches see also: [Susanna Corti, Italy]	The references have been added.
11-381	11	14	48	48	14	Smith DM, Eade R, Pohlmann H (2012) A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction. Submitted to Clim Dyn [Susanna Corti, Italy]	The references have been added.
11-382	11	14	48	48	14	Magnusson, L., M. Balmaseda, S. Corti, F. Molteni and T. Stockdale . (2012), Evaluation of forecast strategies for seasonal and decadal forecasts in presence of systematic model errors. Climate Dynamics DOI 10.1007/s00382-012-1599-2 [Susanna Corti, Italy]	The references have been added.
11-383	11	14	48			add Smith, D. M., R. Eade, H. Pohlmann, 2012: A comparison of anomaly and full field initialization for seasonal to decadal climate prediction. Clim. Dyn. reference [Daniela Matei, Germany]	The reference has been added.
11-384	11	14	50			"climate": A more common terminology would be the model's "climatology", but even that begs the question: Has this terminology (for long-term mean behavior/assymptote of a model) been defined previously? If not, clarification or definition is needed here. [Government of United States of America]	'climatology' now used. 'Climatology' is a widely used term. A definition is easily obtained for the readers who don't know what it means
11-385	11	14	52	14	53	Please revise the last sentence in this paragraph as the following: "This may be at least partially offset by using anomaly initialization in which observed anomalies are added to the model climate to produce initial conditions (see 11.2.3.1), or by using dynamic bias correction in which multi-year monthly mean analysis increments already produced in full-observation initialization are incorporated into each integration step of ocean model during hindcasts and forecasts (Wang et., 2012)." (Please refer to Comment No.20 for the reference paper: Wang et al, 2012) [Bin Wang, China]	Added to section 11.2.3.1.
11-386	11	14				Section 11.2.2.1.3 Summary, There is no mention in the summary of the fact that potential preidctability is regionally and variable dependent - This should be added as it is an important point. [Jonathan Robson, United Kingdom]	The Summary has been rewritten and now includes the statement "There is evidence of multi-year predictability for both the internally generated and externally forced components of temperature over considerable portions of the globe ….." which indicates that predictability varies with location. The summary also indicates that temperature predictability differs from that for precipitation.
11-387	11	15	8	15	11	The aspect that our models are not accurate because of model uncertainty and model error, has not been mentioned before with this being the first place I found. Maybe it deserved a more expanded note. For a	Model inadequacy has been referred to at the beginning of the chapter. The reference mentioned by

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						recent comparison of the different methodological approaches to account for model uncertainty see Weisheimer et al., GRL 2011. [Antje Weisheimer, United Kingdom]	the reviewer has been added.
11-388	11	15	13	15	18	I miss some criticism of the multi-model approach, see for example Masson & Knutti, GRL (2011) for a hierarchical custer analysis of the CMIP-3 models showing that the quasi-independence assumption is not valid. [Antje Weisheimer, United Kingdom]	The criticism and the reference have been added.
11-389	11	15	13		18	MM is also used extensively in detection and attribution. There are meanwhile more papers out with predictions of the mm analyzed - (eg Hanlon et al 2012 just one example) [Gabriele Hegerl, United Kingdom]	The references to the CMIP5 multi-model are listed in section 11.2.3
11-390	11	15	15			Consider referencing: US National Multi-Model Ensemble project (http://www.cpc.ncep.noaa.gov/products/NMME/) [Government of United States of America]	No valid reference to the NMME could be found at the time of writing.
11-391	11	15	22	0	0	Sub-Section Decedal prediction Experiments: Too long [Antje Weisheimer, United Kingdom]	The text has been cut to reduce the length of the chapter.
11-392	11	15	22			Section 11.2.3: There is substantial duplication between material in the 4th and 5th paragraphs of 11.2.3.1 and subsections 11.2.3.3/11.2.3.4. Suggestions: 1) Truncate 4th paragraph of 11.2.3.1 (p. 15 lines 50-57) to "This recent recognition that decadal climate prediction is important has motivated the research community to design coordinated experiments. The ENSEMBLES project (van Oldenborgh et al., 2012), for example, has conducted a multi-model decadal retrospective prediction study, and the Coupled Model Intercomparison Project phase 5 (CMIP5) proposed a coordinated experiment that focuses on decadal, or near-term, climate prediction (Meehl et al., 2009b; Taylor et al., 2012); both experiments are described further in subsections that follow. Results from the CMIP5 coordinated experiment (Taylor et al., 2012) are the basis for the assessment reported here." (Deleted material and references in p. 15 lines 54-57 appear again at p. 18 lines 8-16.) 2) Move the paragraph on p. 16 lines 2-19 to be the first paragraph under "11.2.3.4 CMIP5 Decadal Prediction Experiments", removing references to ENSEMBLES which is covered in the preceding paragraph. In my opinion these changes will help "11.2.3.1 Decadal Prediction Experiments" flow better and lend some introductory focus to "11.2.3.4 CMIP5 Decadal Prediction Experiments" which lacks context as currently written. [William Merryfield, Canada]	The whole section 11.2.3.1 has been restructured, collecting information from other sections of the chapter and introducing in detail all types of decadal prediction experiments. The sections 11.2.3.3 and 11.2.3.4 just discuss results in the revised version.
11-393	11	15	24			This introductory matter is redundant with the previous sections. More to the point, because there is clearly a different author writing this material, it introduces different terminologies etc which only confuses and complicates the presentation of the chapter as a whole.This discussion needs to be cut back or at least brought into better terminological agreement with thepreceding parts of the chapter. [Aside: multiple typos begin to appear in this subsection.] [Government of United States of America]	The whole section 11.2.3.1 has been restructured, collecting information from other sections of the chapter and introducing in detail all types of decadal prediction experiments. The sections 11.2.3.3 and 11.2.3.4 just discuss results in the revised version.
11-394	11	15	33			Recommend "is paid" --> "has been paid" [Government of United States of America]	The sentence has been removed.
11-395	11	15	34	15	34	typo "skillful" [Annalisa Cherchi, Italy]	The text uses British English.
11-396	11	15	38	15	39	"an initialized prediction.." This should appear earlier, perhaps in page 11. [Ramon de Elia, Canada]	The explanation has been left here because there are different options to define the uninitialized predictions.
11-397	11	15	44	15	44	Suggest deleting "and future", or explaining how inititialization could correct the model response to future forcings [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The text has been deleted.
11-398	11	15	45	15	45	Kharin et al. (2007) reference doesn't seem pertinent--perhaps Kharin et al. (2012) is what is meant? [William Merryfield, Canada]	Corrected.
11-399	11	15	45			Reference to HS2011 may not be appropriate here? [Ed Hawkins, United Kingdom]	Reference removed.
11-400	11	15	47	15	47	typo "studies" [Annalisa Cherchi, Italy]	Corrected.
11-401	11	15	47	15	47	"sutides" -> "studies" [Holger Pohlmann, Germany]	Corrected.
11-402	11	15	47			… sutides (typo) [Government of France]	Corrected.
11-403	11	15	47			Typo – studies [Ed Hawkins, United Kingdom]	Corrected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-404	11	15	50	15	57	Ordering of paragraphs is somewhat odd here, this one for example seem to belong much earlier, even before the section on intialisation. [Noel Keenlyside, Norway]	The whole section 11.2.3.1 has been restructured, collecting information from other sections of the chapter and introducing in detail all types of decadal prediction experiments.
11-405	11	15	51	15	51	The citation to the ENSEMBLES project should be: van der Linden P, Mitchell JFB. 2009. ENSEMBLES: Climate change and its Impacts: Summary of research and results from the ENSEMBLES project. Met Office Hadley Centre: Exeter, UK. [Andreas Fischer, Switzerland]	That reference is not appropriate because it doesn't contain any details about the decadal prediction experiment.
11-406	11	15	51	15	51	A more appropriate reference for the ENSEMBLES project would be: van der Linden, P., and J.F.B. Mitchell (eds.) 2009: ENSEMBLES: Climate Change and its Impacts: Summary of research and results from the ENSEMBLES project. Met Office Hadley Centre, FitzRoy Road, Exeter EX1 3PB, UK. 160pp [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	That reference is not appropriate because it doesn't contain any details about the decadal prediction experiment.
11-407	11	16	2	16	2	typo; retrospective [European Union]	Done.
11-408	11	16	2			retrospective' is misspelled [Timothy DelSole, United States of America]	Done.
11-409	11	16	4	16	4	delete "a" [Holger Pohlmann, Germany]	Done.
11-410	11	16	5	16	5	“a the” typo [Aneesh Subramanian, India]	Done.
11-411	11	16	5			a the' ? [Timothy DelSole, United States of America]	Done.
11-412	11	16	10	16	10	Some groups have not included a volcanic background for future predictions to avoid discontinuity with the historical period. Indeed it is very different to add a constant weak background instead of a strong and short-lasting events. The estimation of the drift as a function of leadtime computed from hincasts does not hold for the former case. I would suggest to add a column in the Table 11.1 that documents (yes/no) the use of a volcanic background for the 2005 forecast. [Christophe CASSOU, France]	The CMIP5 protocol for the decadal experiment specified the treatment of the volcanic background, but it turned out that some groups chose not to include it. Unfortunarely, its impact is not certain at this stage as no published information is available yet. Furthermore, the issueis a technical one, andt the chapter is too tight to find extra space to explain why such an entry appears in the table. Therefore, we decided not include the suggested entry.
11-413	11	16	14	16	14	To clarify, we suggest changing to "operational prediction system where no future information can be used." [Government of United States of America]	Done.
11-414	11	16	21			Comment on Table 11.1. This table is the same as the one in the ZOD and has not be updated since. The update is mandatory for the final version. In particular, the CNRM-CM5 model used by CERFACS for the near term exercise should be described. Here are the information to be added: Centre Europeen de Recherche et de Formation Avancee en Calcul Scientifique (Cerfacs), France / CNRM-CM5 /1.4oL31 / 1oL42 / no /Ocean assimilation (NEMOVAR-COMBINE) / no/ no/ start dates [Christophe CASSOU, France]	Table 11.1 has been updated to include all the models that participated in the CMIP5 decadal experiment. Information on CNRM-CM5 is now available in the table.
11-415	11	16	24	16	24	Rather than say “An important difficulty in climate prediction arises from model biases” it may be better to say “The calibration approach used to adjust for model biases is another important source of uncertainty in climate predictions (e.g., Ho et al. 2010)”. Ho CK, Stephenson DB, Collins M, Ferro CAT, Brown SJ (2012): Calibration strategies: a source of additional uncertainty in climate change projections. Bulletin of the American Meteorological Society, 93, 21-26. [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Done.
11-416	11	16	24	16	32	The drift is even larger than the signals predicted in some cases. This is also obvious from Fig. 11.5 [Noel Keenlyside, Norway]	Done. Illustrating this point is the main motivation of including Fig 11.5.
11-417	11	16	24	16	32	I don't think the discussion of the model bias/drift issues and its implications for near-term predictability and predictions is prominent enough. I wish there was more on this (but I think I've said this in previous reviews already). In part this was already said in 11.2.2.2.3. [Antje Weisheimer, United Kingdom]	This discussion, as many other discussions in the text, had to be summarized due to the total length of the chapter.

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11-418	11	16	27	16	27	Other paper showing this are: (1) Smith, D. M., R. Eade and H. Pohlmann, 2012, A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction, Climate Dynamics, submitted (in revision) (2) Magnusson, L., M. Balmaseda, S. Corti, F. Molteni and T. Stockdale (2012), Evaluation of forecast strategies for seasonal and decadal forecasts in presence of systematic model errors. Accepted [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	References added.
11-419	11	16	27	16	29	Why be so vague about how exactly how biases are removed? A possible sentence could be: "A large part of the biases can be removed by subtracting out the lead-time dependent climatology of the forecasts." This is described in mathematical detail in lines 40-45. These parts should be consolidated into a single paragraph. [Timothy DelSole, United States of America]	Done.
11-420	11	16	29	16	29	Figure 5 is very useful, but it should be also shown the time evolution of the global mean sea surface after the removal of the model bias. I would propose (and I believe it is necessary) a four panel figure with the time series of the biased and unbiased model simulation. The unbiased model simulation can give an idea of the multi model spread. [Susanna Corti, Italy]	The version of the figure in the revised text responds to the reviewer's requirement.
11-421	11	16	29	16	29	I do not find that easy to see the time scale of the global SST drift in figure 11.5 [Ramon de Elia, Canada]	Figure 11.5 has been redone.
11-422	11	16	29	16	29	Figure 5 should read Figure 11.5 [Government of Canada]	Done.
11-423	11	16	29			"Figure 5" would be "Figure 11.5" [Yoshimitsu Chikamoto, United States of America]	Done.
11-424	11	16	32	16	32	We recommend adding a citation for pitfalls for using linear approach. [Government of United States of America]	Done.
11-425	11	16	35	16	38	This figure makes no sense to me. What am I supposed to learn from the spaghetti curves? Why aren't the initial conditions initially close to observations? This figure needs much more explanation, and I question whether it even should be presented. [Timothy DelSole, United States of America]	We decided to include this figure to illustrate the drift and the systematic error to those readers who are not familiar with initialized simulations. This is also one of the few figures in the whole report that show how much dynamical models differ in their climatology from observational references. The far left extreme of each curve does not represent the initial condition, but the first twelve-month running mean. During the first year, models have already drifted substantially, and the curves start far from the observational reference.
11-426	11	16	40	17	13	Too detailed explanation, not appropriate for this type of assessment. Please refer to the relevant literature instead. [Antje Weisheimer, United Kingdom]	Those two paragraphs have been simplified.
11-427	11	16	40		47	In my opinion the mathematical equations are needed since it is the software that take into account. [Ibouraïma YABI, Benin]	That paragraph has been simplified.
11-428	11	16	45	16	45	delete "which" [Holger Pohlmann, Germany]	Done.
11-429	11	16	45	16	45	“which This” typo needs to be corrected [Aneesh Subramanian, India]	Done.
11-430	11	16	50	16	52	More start dates are desirable for a robust estimation of skill in terms of anomaly correlation, but more ensemble members are preferable for estimating skill in terms of root-mean-square error [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Done.
11-431	11	16	50			Additional reference, Sakaguchi et al.2012, This might not be the case if, for instance, the predicted temperature trend differs from the observed trend (Fyfe et al., 2011 [Fyfe, J. C., W. J. Merryfield, V. Kharin,G. J. Boer, W.-S. Lee, andK. von Salzen (2011), Skillful predictions of decadal trends in global mean surface temperature, Geophys. Res. Lett., 38, L22801, doi:10.1029/2011GL049508]; Kharin et al., 2012 [Kharin, V. V., G. J. Boer, W. J. Merryfield, J. F. Scinocca, and W.-S. Lee (2012), Statistical adjustment of decadal predictions in a changing climate, Geophys. Res. Lett., 39, L19705, doi:10.1029/2012GL052647.] Sakaguchi et al., 2012). This study shows mean bias for surface air temperature trend over various spatiotemporal scales from selected (7 each) CMIP3 and CMIP5 models. Sakaguchi, K., X. Zeng, and M. A. Brunke (2012), The hindcast skill of the CMIP	The Box 11.2 deals with in detail with the issue of reliably predicting trends.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						ensembles for the surface air temperature trend, J. Geophys. Res., 117, D16113, doi:10.1029/2012JD017765. [Government of United States of America]	
11-432	11	16	52	16	53	I don't understand the how the bias correction in "Office 2011" differs from that described here. If it is the same then the word "roughly" should be removed. If it is different this should be explained and justified. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	"Roughly" has been removed.
11-433	11	16				Figure 11.5: I assume each line is an individual ensemble member - this should be stated in the caption. It might also be clearer to use different colours for each model - this would highlight the drift and biases common to each model. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Different colours have been used for each model and the explanation that each line represents a member has been added.
11-434	11	17	1	17	1	"…these problems…" please specify, which problems. Bias correction? Or more specifically, initialization issues? [Government of United States of America]	The beginning of the sentence has been changed to "To reduce the impact of the drift".
11-435	11	17	6	17	7	For anomaly initialization, if the mean of model forecast is removed, bias correction is still there. So anomaly initialization does not really remove bias problem. [Government of United States of America]	The sentence does not imply that bias correction is not necessary in the case of anomaly initialization, but that the bias correction might not require to be time dependent.
11-436	11	17	6	17	7	Sampling error is also an issue for full field initialization - especially if there is a small hindcast set such as the core CMIP5 start dates [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The sentence has been changed to "Sampling error in the estimation of the mean climatology affects the success of this approach, which is also the case for the full-field initialization, although it is affected to a smaller degree by the drift".
11-437	11	17	7	17	7	After …to a smaller degree by the drift", I would add something like "due to the inadequation between the amplitude and location of the observed imposed anomalies and the intrinsic model variance". [Christophe CASSOU, France]	We think that this makes the paragraph more difficult to read.
11-438	11	17	9	17	10	add "Smith et al. (2012)" Smith, D. M., A. A. Scaife, G. J. Boer, M. Caian, F. J. Doblas-Reyes, V. Guemas, E. Hawkins, W. Hazeleger, L. Hermanson, C. K. Ho, M. Ishii, V. Kharin, M. Kimoto, B. Kirtman, J. Lean, D. Matei, W. J. Merryfield, W. A. Müller, H. Pohlmann, A. Rosati, B. Wouters, and K. Wyser, 2012: Real-time multi-model decadal climate predictions. Climate Dynamics, in press. [Holger Pohlmann, Germany]	Done.
11-439	11	17	10	17	10	About relative merits of the two initialisation approaches see also: [Susanna Corti, Italy]	Done.
11-440	11	17	10	17	10	Smith DM, Eade R, Pohlmann H (2012) A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction. Submitted to Clim Dyn [Susanna Corti, Italy]	Done.
11-441	11	17	10	17	10	:Magnusson, L., M. Balmaseda, S. Corti, F. Molteni and T. Stockdale . (2012), Evaluation of forecast strategies for seasonal and decadal forecasts in presence of systematic model errors. Climate Dynamics DOI 10.1007/s00382-012-1599-2 [Susanna Corti, Italy]	Done.
11-442	11	17	10	17	10	About relative merits of the two initialisation approaches see also: [Susanna Corti, Italy]	Done.
11-443	11	17	10	17	10	Smith DM, Eade R, Pohlmann H (2012) A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction. Submitted to Clim Dyn [Susanna Corti, Italy]	Done.
11-444	11	17	10	17	10	:Magnusson, L., M. Balmaseda, S. Corti, F. Molteni and T. Stockdale . (2012), Evaluation of forecast strategies for seasonal and decadal forecasts in presence of systematic model errors. Climate Dynamics DOI 10.1007/s00382-012-1599-2 [Susanna Corti, Italy]	Done.
11-445	11	17	10	17	10	Also by Smith, D. M., R. Eade and H. Pohlmann, 2012, A comparison of full-field and anomaly initialization for seasonal to decadal climate prediction, Climate Dynamics, submitted (in revision) [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Done.
11-446	11	17	12			There is a pretty clear model "cool bias" throughout Fig 11.5. Thus we feel that "large systematic error" is needlessly vague here. Also is there any explanation available for that bias? [Government of United States of America]	The explanation of the model bias pertains to Chapter 9.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-447	11	17	15	0	0	11.2.3.2 Forecast Quality Assessment: Too long. This sub-section talks a lot about probabilistic forecasts which is in quite a contrast to the rest of the chapter and indeed to the motivation for near-term predictions. There is also a mis-balance between introducing a lot of probabilistic forecast scores and actual results shown. As far as I am aware, the paper by Corti et al., GRL (2012) is the only one to assess decadal predictions from a probabilistic point of view in terms of relaibility diagrams, see also Fig. 11.8. All the other diagnostics is determinstic based on the ensemble mean. [Antje Weisheimer, United Kingdom]	This section has been substantially reduced.
11-448	11	17	17	17	17	The distinction between model validation and forecast qualitiy is not explained futher. The next paragraphs all continue about forecast quality, when one would expect at least some information about model validation as well [Emma Daniels, Netherlands]	Model validation pertains to Chapter 9.
11-449	11	17	17	17	17	"... forecast quality assessment is typically made NRC ..." The sentence is a bit confusing. The meaning of "NRC" was not clearly indicated in this chapter. [Gan Zhang, United States]	The sentence has been corrected to "A distinction between model validation and forecast quality assessment is typically made, where model validation informs about the mechanisms responsible of the mean differences between the simulations and observational references while forecast quality is estimated to provide users with information about several properties of the predictions"
11-450	11	17	17	17	30	This is the 2nd or 3rd time the distinction between predictability and skill is discussed in this chapter (e.g., see p10, lines 21-34). These separate statements should be consolidated into a single paragraph or two, instead of sprinkled and repeated in different parts of the chapter. [Timothy DelSole, United States of America]	The consistency of the two sections has been improved.
11-451	11	17	17	17	30	This paragraph is poorly written, and hard to understand. We recommend that it be re-written. [Government of United States of America]	The paragraph is necessary as is because the terms that define the different attributes of the quality of the predictions are not common to the climate-change community.
11-452	11	17	17	17	30	This introduction of verification concepts confuses ideas and so needs to be rewritten more carefully. Accuracy is not precision – one can have very precise predictions (i.e. small spread) that are a long way from the true observations. The mean distance between forecasts and observations, accuracy, requires low bias as well as high precision. Skill is more general than just the accuracy of the system – it is the complete joint distribution between forecasts and observations of which accuracy is just one measure. The word metric implies distance and so applies only to accuracy in this paragraph – the word measure would be less misleading. Use of the glossary in J&S 2011 might help improve the definitions here. [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Done.
11-453	11	17	17		37	This section occasionally uses the term metric for specific measures. The term metric is genrally misleading as it is a mathematiclly fixed definition containing e.g distances. To commonly use a term throughout this section I would recommend another the term e.g. „measure“ [Wolfgang Müller, Hamburg]	Done.
11-454	11	17	21	17	21	"acccuracy of a .. precision" In a scientific sence, accuracy is often used to mean the closeness of an measurement to an observation, whereas precision is the uncertainty of that measurement. This sentence appears to conflate the two - it may benefit from re-wording. [Dan Hodson, United Kingdom]	The definition is now "The accuracy of a forecast system refers to the average distance/error between forecasts and observations"
11-455	11	17	23			"ultimately eliminate"? Models are always imperfect, so this is an unfounded and unnecessary promise. [Government of United States of America]	Done.
11-456	11	17	23			We think that this discussion of skill vs reliability is likely to just confuse most readers. Although its also an oversimplification, would it be possible to tilt these definitions towards "as used here, skill is a metric of deterministic accuracy whereas reliability is a metric of probabilitistic accuracy"? [Government of United States of America]	This is incorrect. Reliability is a measure of probabilistic trustworthiness. We believe that it's very important to offer the reader an appropriate description of what forecast quality mean.
11-457	11	17	24	17	24	We are not sure why "reliability" alone is a property of a "property of a specific forecast system?" So does any other skill measure. RMSE is specific to a forecast system. [Government of United States of America]	The bias is anothern property specific of a system. Bias and relliability are several aspects of the systematic error of a forecast system.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-458	11	17	26	17	27	The statement that a foecast is reliable if a user can rely on a forecast to make a decision seems strange to me. If I understand the definition correctly, a forecast which always predicted the climatological relative frequencies of events would be perfectly reliable, but would not have any skill. It's not clear to me that this would be a forecast which could be relied on to make a decision. [Nathan Gillett, Canada]	Many users of climate information rely on climatological estimates to make decision. This is because either they don't trust climate predictions or the climate predictions are not skilful. Even for those users, obtaining a good estimate of the local climate (not just the mean, but the whole climatological pdf) is very difficult due to the lack of quality observations. A good prediction should improve its skill with respect to a climatological forecast, or any other naive prediction, while providing trustworthy (i.e. reliable) information.
11-459	11	17	27	0	0	explain the word "reliable" by saying one can "rely" on something doesn't sound very logical to me [Antje Weisheimer, United Kingdom]	This description is used because many users of climate information rely on climatological estimates to make decision. This is because either they don't trust climate predictions or the climate predictions are not skilful. Even for those users, obtaining a good estimate of the local climate (not just the mean, but the whole climatological pdf) is very difficult due to the lack of quality observations. A good prediction should improve its skill with respect to a climatological forecast, or any other naive prediction, while providing trustworthy (i.e. reliable) information.
11-460	11	17	27	17	28	The first paragraph of 11.2.3.2 discusses accuracy, skill and reliability as three distinct attributes of a forecast system, and then states "Accuracy and reliability are aspects of the forecast quality that can be improved by improving the individual forecast systems or by combining several of them into a multi-model prediction." This seems to imply that skill cannot be improved by system improvements or multi-model combination, which goes against my intuition and experience. Was this meaning intended? If so then I think it deserves a few words of explanation. [William Merryfield, Canada]	Skill being a relative measure of accuracy, it was clear to us that it could also be improved by the means described. The paragraph has been completely revised
11-461	11	17	27	17	29	add "possibly increasing forecast skill by unequal weighing (Weigel et al. 2010, DelSole et al. 2012)" Weigel, A. P., R. Knutti, M. A. Liniger, and C. Appenzeller, 2010: Risks of model weighting in multi-model climate projections. J. Clim., 23, 4175-4191; DelSole, T., X. Yang, and M. K. Tippett, 2012: Is Unequal Weighting Significantly Better than Equal Weighting for Multi-Model Forecasting? Quart. J. Roy. Meteor. Soc., DOI:10.1002/qj.1961 [Holger Pohlmann, Germany]	Done.
11-462	11	17	28	17	28	typo: skilful [European Union]	Done.
11-463	11	17	29	17	30	Why single out 'reliability' as being improvable by post-processing? All three metrics can be improved by post-processing. [Timothy DelSole, United States of America]	While reliability can be improved in the post-processing of a single system, accuracy needs other sources of information to be improved. However, the sentence has been removed.
11-464	11	17	29	17	30	"... Furthermore, the reliability can be increased by statistical post-processing of the predictions." Related references should be added here. In particular, some new statistical post-processing method has been developed recently, which can well improve the prediction of climate for East Asia and even high latitudes where the models always show the worst predictability. Additionally, the dynamical downscaling using regional models should also be considered. Thus the sentence is suggested to change to "... Furthermore, the reliability can be increased by statistical post-processing of the predictions (Wang and Fan, 2009; Lang and Wang, 2010; Sun and Chen, 2012; Chen et al., 2012; Liu and Fan, 2012; Gu et al., 2012) and by dynamical post-processing using regional climate models (Wang et al., 2011; Yu, 2012). [References: (1) Wang H. J., K. Fan, 2009: A new scheme for improving the seasonal prediction of summer precipitation anomalies, Wea. Forecasting, 24, 548–554. (2) Sun J. Q., H. P. Chen, 2012: A statistical downscaling scheme to improve global precipitation forecasting, Meteorol. Atmos. Phys., 117, 87-102. (3) Chen, H. P., J. Q. Sun, and H. J. Wang, 2012: A statistical downscaling model for forecasting summer rainfall in China from DEMETER hindcast	The sentence has been removed to simplify the section.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						datasets, Wea. Forecasting, 27, 608-628. (4) Liu, Y., K. Fan, 2012: Improve the prediction of summer precipitation in the southeastern China by a hybrid statistical downscaling model, Meteorol Atmos Phys, 117, 121-134. (5) Lang, X. M., H. J. Wang, 2010: Improving extraseasonal summer rainfall prediction by merging information from GCMs and observations, Wea. Forecasting, 25, 1263-1274. (6) Gu, W. Z., L. J. Chen, W. J. Li, and D. L. Chen, 2011: Development of a downscaling method in China regional summer precipitation prediction. Acta Meteorol. Sinica, 25, 303-315. (7) Wang, H. J., E. T. Yu, S. Yang, 2011: An exceptionally heavy snowfall in Northeast China: large-scale circulation anomalies and hindcast of the NCAR WRF model, Meteorol. Atmos. Phys., 113, 11-25. (8) Yu, E. T., 2012: High-resolution seasonal snowfall simulation over Northeast China. Chinese Science Bulletin, doi: 10.1007/s11434-012-5561-9.] [Dabang Jiang, China]	
11-465	11	17	29	17	30	"... Furthermore, the reliability can be increased by statistical post-processing of the predictions." Some following related references should be added here. Wang H. J., K. Fan, 2009: A new scheme for improving the seasonal prediction of summer precipitation anomalies, Wea. Forecasting, 24, 548–554. Sun J. Q., H. P. Chen, 2012: A statistical downscaling scheme to improve global precipitation forecasting, Meteorol. Atmos. Phys., 117, 87-102, doi 10.1007/s00703-012-0195-7. Lang, X. M., H. J. Wang, 2010: Improving Extraseasonal Summer Rainfall Prediction by Merging Information from GCMs and Observations, Wea. Forecasting, 25, 1263-1274. Chen, H. P., J. Q. Sun, and H. J. Wang, 2012: A Statistical Downscaling Model for Forecasting Summer Rainfall in China from DEMETER Hindcast Datasets, Wea. Forecasting, 27, 608-628. [Jianqi Sun, China]	The sentence has been removed to simplify the section.
11-466	11	17	29	17	30	delete last sentence as not relevant here. [Antje Weisheimer, United Kingdom]	Done.
11-467	11	17	35	17	36	ROC is a probabilistic measure, not deterministic [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	ROC can be used both in a deterministic and probabilistic context: Mason, S. J., and N. E. Graham, 2002: Areas beneath the relative operating characteristics (ROC) and levels (ROL) curves: statistical significance and interpretation. Quarterly Journal of the Royal Meteorological Society, 128, 2145-2166. As the ROC has not been used yet in decadal prediction, this part of the paragraph has been removed.
11-468	11	17	35	17	36	Why is the ROC curve a recommended metric for deterministic forecasts? [Antje Weisheimer, United Kingdom]	ROC can be used both in a deterministic and probabilistic context: Mason, S. J., and N. E. Graham, 2002: Areas beneath the relative operating characteristics (ROC) and levels (ROL) curves: statistical significance and interpretation. Quarterly Journal of the Royal Meteorological Society, 128, 2145-2166. As the ROC has not been used yet in decadal prediction, this part of the paragraph has been removed.
11-469	11	17	36			The introduction of ROC here without any definition or explanation will confused or lose audience. [Government of United States of America]	As the ROC has not been used yet in decadal prediction, this part of the paragraph has been removed.
11-470	11	17	42	17	44	We are not sure what the sentence "A ratio of one…well calibrated prediction system" means and how it relates to signal-to-noise. There are lots of places where signal-to-noise is much less than one, and it does not mean that a well calibrated forecast system that should replicate a signal-to-noise less than one is not "well calibrated." [Government of United States of America]	The spread-to-RMSE ratio has not been related to the signal to noise ratio.
11-471	11	17	47			In several parts of this chapter, the claim is made that autocorrelation in the time series is taken into account in the significance test. As far as I know, the validity of this procedure has never been established. In addition, the actual procedure is not described and not clearly identified with a reference. [Timothy DelSole, United States of America]	The procedure used is a conservative estimate of the effective sample size that tries to reduce the number of degrees of freedom used in the statistical inference analysis of the skill measures. References to papers

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							that use the method described in von Storch and Zwiers (1999) where the basic procedure is described, are included in the text. Statistical analysis in climate research, by Storch, H. von and Zwiers, F.W. Cambridge University Press, Cambridge, 1999
11-472	11	17	48	27	50	There is also bias in other skill measures such as reliability so it would be helpful to cite this recent paper: Ferro, C. A. T. and Fricker, T. E. (2012), A bias-corrected decomposition of the Brier score. Q.J.R. Meteorol. Soc., 138: 1954–1960. doi: 10.1002/qj.1924 [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Done.
11-473	11	17	48			The reference for Gangstø R, Weigel AP, Liniger MA, Appenzeller C Comments on the evaluation of decadal predictions , Climate Research, accepted, DOI 10.3354/cr01135 [Christof Appenzeller, Switzerland]	Done.
11-474	11	17	50			Hanlon et al (2012a, J Climate in press) also uses confidence intervals based on bootstrap methods. (sorry for continuing to advocate our paper but there isnt that much out yet..) [Gabriele Hegerl, United Kingdom]	Done.
11-475	11	17	53	17	54	The relationship between size of verification sample, and its influence on errors in the estimate of skill scores is well known. A citation is: Kumar, A., 2009: Finite samples and uncertainty estimates for skill measures for seasonal predictions. Mon. Wea. Rev, 137, 2622-2631. And although the title of paper refers to seasonal, the relationships discussed are universal. [Government of United States of America]	Done.
11-476	11	17	55	0	0	"can vary from generation to generation" - unclear [Antje Weisheimer, United Kingdom]	The sentence has been changed to "The skill of seasonal predictions can vary from a generation of forecast systems to the next one".
11-477	11	17	55	17	55	suggest changing "from generation to generation" to "over time", since "generation" can refer to stages of forecast system development [William Merryfield, Canada]	That was the intention. The sentence has been changed to "The skill of seasonal predictions can vary from a generation of forecast systems to the next one".
11-478	11	17	55	17	55	The phrase "generation to generation" is misleading or ambiguous; "decade to decade" would be much clearer, and represent what is shown in the papers referred to. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	The sentence has been changed to "The skill of seasonal predictions can vary from a generation of forecast systems to the next one".
11-479	11	17	55			What does 'from generation to generation' mean here? Generation of prediction system? Explain. [Nathan Gillett, Canada]	The sentence has been changed to "The skill of seasonal predictions can vary from a generation of forecast systems to the next one".
11-480	11	17	56	17	56	There is no question that skill of decadal prediction "might" vary from one period to another, it will vary. We have seen enough from weather and seasonal prediction how skill goes up and down to doubt that it will not happen for decadal predictions. The sentence also contradicts an earlier statement about "conditional skill." [Government of United States of America]	The sentence has been changed to "The skill of seasonal predictions can vary from a generation of forecast systems to the next one".
11-481	11	18	3	18	3	Please remove a "skill" in "... continuous ranked probability skill skill score ..." [Gan Zhang, United States]	Done.
11-482	11	18	3	18	4	The CRPSS is mentioned but never used in this report, so why having it in at all? [Antje Weisheimer, United Kingdom]	The CRPSS is mentioned because it is one of the few probabilistic scores used in decadal forecasting in Goddard et al (2012).
11-483	11	18	6	0	0	11.2.3.3 Pre-CMIP5 Deacadal Prediction Experiments: Too long [Antje Weisheimer, United Kingdom]	As other sections, this one has also been reduced.
11-484	11	18	6	18	36	One would expect some results of the pre-CMIP5 experiments here, or at least a reference to where in the chapter these can be found. [Emma Daniels, Netherlands]	Some pre-CMIP5 studies are in fact discussed here.
11-485	11	18	6			Both the "initialization" and "initialisation" forms appear in this section (maybe the other sections, too), This is understandable since different authors contribute to the writing, but it may be better to adopt a uniform term if there is no special concerns. [Gan Zhang, United States]	Agreed. Chapter now uses "initialization" and "initialize" throughout.

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11-486	11	18	10	18	14	add "Matei et al. (2012)" Matei, D., H. Pohlmann, J. Jungclaus, W. Müller, H. Haak, and J. Marotzke, 2012: Two tales of initializing decadal climate prediction experiments with the ECHAM5/MPI-OM model. J. Climate, doi:10.1175/JCLI-D-11-00633.1 [Holger Pohlmann, Germany]	Done.
11-487	11	18	10	18	14	add "Kröger et al. (2012)" Kröger, J., W. Müller, and J.-S. von Storch, 2012: Impact of different ocean reanalyses on decadal climate prediction. Climate Dynamics, doi:10.1007/s00382-012-1310-7. [Holger Pohlmann, Germany]	Done.
11-488	11	18	11	18	11	It would be usefull to qualify the skill found. [Ramon de Elia, Canada]	Done.
11-489	11	18	11	18	14	add Matei et al.2012a and Kröger, J. and W. Müller and J.-S. von Storch, 2012, Impact of different ocean reanalyses on decadal climate prediction. Climate Dynamics, doi:10.1007/s00382-012-1310-7. [Daniela Matei, Germany]	Done.
11-490	11	18	14	18	15	Prior to the CMIP5, decadal prediction experiment is also performed by a coupled global climate model FGOALS_gl developed by the State Key Laboratory of Numerical Modeling for Atmospheric Sciences and Geophysical Fluid Dynamics (LASG) within the Institute of Atmospheric Physics (IAP), Chinese Academy of Sciences (CAS) (Wu and Zhou 2012). Nine different start dates are considered for the hindcast run. Compared with the non-initialized predictions, the skill was greatly enhanced by the initialization in the tropical central-eastern Pacific and mid-latitude northeastern Pacific (Wu and Zhou 2012). [References: Wu, B., and T. Zhou, 2012: Prediction of decadal variability of sea surface temperature by a coupled global climate model FGOALS_gl developed in LASG/IAP. Chinese Science Bulletin, 57, 2453-2459, doi: 10.1007/s11434-012-5134-y.] [Tianjun Zhou, China]	Done.
11-491	11	18	38	0	0	11.2.3.4 CMIP Decadal Prediction Experiments: way too long. I feel this sub-section is especially too long and too much detail is given. This also related to the figure caption, e.g. Fig 11.6., Fig 11.7, Fig 11.9, Fig 11.10a. I would suggest to shorten this section radically and only show selected results in a few well explained figures. [Antje Weisheimer, United Kingdom]	The section has been shortened.
11-492	11	18	38	24	8	The structure of this sub-section is not clear. While it starts with the presentation of results, it frequently goes back to introduce methodological choices (e.g. p 21, para starting in line 31). Also, the order in which the quantities are discussed is not overly intuitive (AMV, IPO, AMV/AMOC, PDO, AMOC, and then SST). [European Union]	The section has been revised and shortened.
11-493	11	18	38			Section 11.2.3.4: consider structuring the section using section-internal headings [Thomas Stocker/ WGI TSU, Switzerland]	The section is now shorter and might not require section-internal headings.
11-494	11	18	40	18	41	"Global" is repeated. A better phrasing would be "Indices of global-mean temperature, the Atlantic multi-decadal variability (AMV) and the interdecadal Pacific oscillation (IPO) are used as benchmarks" [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Done.
11-495	11	18	40	18	43	This paragraph needs revising: as currently written it seems to state that the AMV and IPO are used as benchmarks to assess ability to predict the AMV and IPO, and generally makes little sense. [William Merryfield, Canada]	The paragraph has been corrected.
11-496	11	18	40			"Global mean temperature, …., as well as global mean temperature indices"? If this isn't redundant then it needs some explanation. [Government of United States of America]	The paragraph has been corrected.
11-497	11	18	45			Lee et al., 2006(?) J Climate (Lee, Zwiers etc cited in AR4 Ch9) pioneered this, worth citing [Gabriele Hegerl, United Kingdom]	A reference to the explanations in section 11.2.3.1, where there is a reference to Lee et al., has been included.
11-498	11	18	51	18	53	along the forecast time' is an awkward phrase. 'with subtle differences…' is so vague as to be meaningless-- I suggest removing it or clarifying what exactly is being said. [Timothy DelSole, United States of America]	This sentence has been removed.
11-499	11	18	53	18	53	I would suggest "when measured by RMSE" instead of "although only in the RMSE sense". The existing text makes it sound as if the RMSE measure is secondary. In fact, reproducing the sign of temperature changes over recent decades is fairly trivial. It is the amount of warming that is important, and RMSE captures this much better than ACC. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Done.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-500	11	18	56	18	56	shows decadal variability' What does this mean? White noise also shows decadal variability, but I presume this is not the kind of variability that is being referenced here. [Timothy DelSole, United States of America]	The sentence has been changed to "The AMV index shows a large fraction of its variability on decadal time scales".
11-501	11	18				Here and elsewhere, the authors mention "statistically significantly skilful", without mentioning the level of significance and how it is measured or what it is relative to. Please add a significance level to such statements. If possible, please also consider adding a short explanation as to how significance is measured. [Government of United States of America]	The details about the inference test are included in the figure captions instead of in the section to increase the readability of the main text.
11-502	11	19	1	19	1	would have thought ealier references more important: Enfield et al. 2001, Folland et al. 1986, Sutton and Hodson 2005 AMV has also been linked to global temperature changes, Knight et al. 2005, Semenov et al. 2010 Semenov, V., Latif, M., Dommenget, D., Keenlyside, N., Strehz, A., Martin, T. und Park, W. (2010) The Impact of North Atlantic-Arctic Multidecadal Variability on Northern Hemisphere Surface Air Temperature Journal of Climate, 23 (21). pp. 5668-5677. DOI 10.1175/2010JCLI3347.1. [Noel Keenlyside, Norway]	The use of recent references is encouraged in the assessment.
11-503	11	19	1	19	1	add "Müller et al. (2012)" Müller, W. A., J. Baehr, H. Haak, J. H. Jungclaus, J. Kröger, D. Matei, D. Notz, H. Pohlmann, J.-S. von Storch, and J. Marotzke, 2012: Forecast skill of multi-year seasonal means in the decadal prediction system of the Max Planck Institute for Meteorology. Geophys. Res. Lett., doi:10.1029/2012GL053326 [Holger Pohlmann, Germany]	More targetted references have been used in the revised version.
11-504	11	19	1	19	1	Multi-year predictability of the AMV has been demonstrated, and this is expected to influence temperature and precipiation over land. However, multi-year predictability over land associated with the AMV has not yet been demonstrated [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	We agree with the comment. However, the sentence refers only to the link between the AMV and the temperature and precipitation over land, not to the remote predictability associated with the AMV.
11-505	11	19	1			add Matei et al.2012a [Daniela Matei, Germany]	More targetted references have been used in the revised version.
11-506	11	19	1			add Müller et al. 2012 (Müller, W., J. Baehr, H. Haak, J. Jungclaus, J. Kröger, D. Matei, D. Notz, H. Pohlmann, J.S. von Storch, J. Marotzke, 2012: Forecast skill of multi-year seasonal means in the decadal prediction system of the Max Planck Institute for Meteorology. Geophys. Res. Lett., 39, L22707, doi:10.1029/2012GL053326) [Daniela Matei, Germany]	More targetted references have been used in the revised version.
11-507	11	19	2	19	2	Also Smith et al 2010 [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Done.
11-508	11	19	2	19	4	The AMV has been connected to multi-decadal variability of Atlantic tropical cyclones and Asian monsoons (Dunstone et al., 2011; Goldenberg et al., 2001; Zhang and Delworth, 2006; Li and Bates, 2007; Li et al. 2008).Please refer to: Li, S., and G. Bates, 2007: Influence of the Atlantic Multidecadal Oscillation (AMO) on the winter climate of East China. Adv. Atmos. Sci., 24(1),126-135. Li, S., J. Perlwitz, X. Quan, and M. P. Hoerling, 2008: Modelling the influence of North Atlantic multidecadal warmth (AMO) on the Indian summer rainfall. Geophys. Res. Lett.,35, L05804, doi:10.1029/2007GL032901. [Shuanglin Li, China]	Done.
11-509	11	19	3	19	5	This sentence is out of place and advances a controversial view. First, non-initialized predictions are skillful over most of the globe, as seen in fig. 11.7, so it is perplexing why only AMV is explicitly mentioned. Second, the scientific community is split about whether AMV is dominated by forced or unforced variability. I am aware of a paper from GFDL that challenges the Booth et al. paper. I see no reason for ch11 to take sides on this evolving scientific question. [Timothy DelSole, United States of America]	The literature based on CMIP5 and ENSEMBLES decadal predictions and mentioned in the chapter shows that the multi-model prediction of the AMV in the non-initialized predictions is skilful when compared to the observations. This result is not trivial because the AMV is defined as the differencial warming of the North Atlantic SST with respect to the global oceans (note that the global-mean SST is removed from the North Atlantic averaged SSTs). This means that part of the differencial warming of the North Atlantic can be attributed to the prescribed variable forcing. This result is valid irrespective of Booth et al results, so the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							reference to Booth et al has been replaced by more appropriate references.
11-510	11	19	3	19	5	On the correlation of NoInit predictions of AMV -> If I read the graph correctly, the anomaly correlation skill score for the AMV is not significant for NoInit? Therefore, the figure does not fit with the statement that the "non-initialized AMV predictions is consitent with the view that........is due to external forcings". It is also clear that the NoInit runs do not capture the magnitude of the variability and look more like they are warming almost monatonically (figure 11.6) --> does this result in fact mean that the current models suggest that the external forcings can not account for the observed variability? [Jonathan Robson, United Kingdom]	The literature based on CMIP5 and ENSEMBLES decadal predictions and mentioned in the chapter shows that the multi-model prediction of the AMV in the non-initialized predictions is skilful when compared to the observations. This result is not trivial because the AMV is defined as the differencial warming of the North Atlantic SST with respect to the global oceans (note that the global-mean SST is removed from the North Atlantic averaged SSTs). This means that part of the differencial warming of the North Atlantic can be attributed to the prescribed variable forcing. Although the correlation of the multi-model ensemble mean is not significant at the 95% level, the correlation (and in a more reduced way, also the RMSSS) is positive in all the experiments available. This agreement provides enough confidence to attribute part of the recent AMV variations to the prescribed variable forcing, in spite of the underestimation of the observed variability by the multi-model ensemble mean.
11-511	11	19	3	19	5	Booth et al refers to variability in the North Atlantic, not global mean temperature [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The reference to Booth et al has been removed.
11-512	11	19	3	19	9	This section is confusing as it is presently written, and I suggest it is revised. Booth et al show that although models usually fail to produce the observed AMV amplitude when forced with aerosol, a sufficiently large indirect affect allows one model to do so. Fig 11.6 shows that although the CMIP5 forced ensemble has a good correlation with observations, it substantially underestimates the amplitude of the AMV. This might suggest that the forcing CANNOT explain the AMV, were it not for the result of Booth et al, which at least opens up the possibility. The initialized forecasts are much better at capturing what happened, as is clear also from the RMSE. It is a very interesting result - does initialization allow good short term predictions despite model inadequacies, or does the result suggest that some part of the variabilty was unforced and has to be initialized? [There are analogies to seasonal prediction here]. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	The section has been completely revised to take into account this and other comments about the role of the initialization in the prediction of the AMV.
11-513	11	19	4	19	4	The current phrasing would indirectly imply that the AMV is entirely explained by the external forcings. I would add "part of the recent variability is due to external forcings, especially the aerosols whose impact still needs to be clearly quantified though". I would add the Terray (2012)'s reference : Terray L, 2012: Evidence for multiple drivers of North Atlantic multi-decadal climate variability. Geophys. Res. Lett, 39, L197-12, doi:10.1029/2012 GL053046. [Christophe CASSOU, France]	The section has been completely revised to take into account this and other comments about the role of the initialization in the prediction of the AMV. The reference has been added.
11-514	11	19	4	19	5	A reference to Terray (2012) should be added here. This study also showed that although external forcings are the dominant driver of the tropical and subtropical part of the AMV, there is a significant contribution from the unforced component to the subpolar part of the AMV Terray, L., 2012: Evidence for multiple drivers of North Atlantic muli-decadal climate variability. Geophys. Res. Lett., 39, L19712. [RYM MSADEK, United States of America]	The reference has been added.
11-515	11	19	5	19	5	Otterå et al. ... 2010 was the first paper I know that showed such a link [Noel Keenlyside, Norway]	The reference has been added.
11-516	11	19	5	19	5	Should this be "Figure 11.6 shows that the CMIP5 [initialized] multi-model ensemble mean…" ? [William Merryfield, Canada]	The figure shows results for both the initialized and uninitialized ensemble.
11-517	11	19	5	19	6	"similar skill" to what? [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The sentence has been changed to "Figure 11.6 shows that the CMIP5 multi-model ensemble mean has skill on multi-annual time scales, the skill being

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							generally larger than for the single-model forecast systems".
11-518	11	19	7	19	9	The sentence begins by highlighting the improvement in AMV, then mentions that RMSE is reduced for global-mean temperature. Should it actually be RMSE is reduced for atlantic temperture? [Jonathan Robson, United Kingdom]	The sentence has been corrected to "In particular, the root mean square error is substantially reduced with the initialization for the AMV, which suggests that the external forcing as it is modelled in current systems does not explain all the AMV".
11-519	11	19	8	19	9	skill' and 'mean square error' implicitly refer to different metrics, but this is not clearly stated. What is the measure of skill in this sentence? [Timothy DelSole, United States of America]	The measure of skill is the correlation of the ensemble-mean predicition.
11-520	11	19	9			Given broad audience, consider replacing "reduced with " with "reduced (indicating improved skill) with" [Government of United States of America]	Done.
11-521	11	19	11	19	12	Important paper from Seager and probably many other: Seager, R., Y. Kushnir, M. Ting, M. Cane, N. Naik, and J. Miller (2008), Would advance knowledge of 1930s SSTs have allowed prediction of the dust bowl drought?, J Climate, 21(13), 3261-3281. [Noel Keenlyside, Norway]	Done.
11-522	11	19	17	19	18	Some skill is found in Keenlyside et al. 2008 (see fig 3e, and Suppl. Fig. 1 &2) [Noel Keenlyside, Norway]	The assessment is carried out preferably on multi-forecast system predictions instead of on single-model systems.
11-523	11	19	19	11	19	"the robust high correlation..." The ensemble-mean correlations shown for AMV in Fig 11.6 suggest that correlations of the non-initialized AMV predictions are not-significantly correlated and the initialized experiments appear to capture much more of the variance observed in the observations. This appears to be at odds with the statement made in this sentence. [Dan Hodson, United Kingdom]	The sentence has been changed to "The positive correlation of the non-initialized AMV predictions is consistent with the view that part of the recent variability is due to the external forcings".
11-524	11	19	19	19	21	"Although" occurs twice in sentence. [Government of Canada]	The second "although" has been removed.
11-525	11	19	19	19	21	Either the first or the second "although" in this sentence needs to be removed. [William Merryfield, Canada]	The second "although" has been removed.
11-526	11	19	19	19	21	"Although" is used twice in the same sentence [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	The second "although" has been removed.
11-527	11	19	19	19	22	There are two 'althoughs' in this single sentence. The sentence is difficult to understand. [Timothy DelSole, United States of America]	The second "although" has been removed.
11-528	11	19	25	19	29	The authors mention that DePreSys and ENSEMBLES give a better indication of forecast skill. Why are these not shown instead of the CMIP5 results? Was the CMIP5 design philosophy flawed? [Government of United States of America]	The sentence is misleading. In fact, as DePreSys and ENSEMBLES do not assume any future information of the volcanic aerosol at the start of the prediction, their skill estimates are closer to what could be obtained in an operational context where the decadal predictions will not include such forcing. Instead, the CMIP5 experimental setup includes the volcanic forcing until 2005, mimicking what is done in the historical simulations. The sentence has been changed to "As these forcings can not be specified in a real forecast setting, ENSEMBLES offers an estimate of the skill closer to what could be expected from a real-time forecast system such as the one described in Smith et al. (2013)"
11-529	11	19	26	19	26	"former" is ambiguous because CMIP5 was earlier in time. Replace by "CMIP5 multi-model ensemble" [Jouni Räisänen, Finland]	Done.
11-530	11	19	29	19	29	I suggest to add "although use of correct forcings allows a more powerful test of the ability of models to	Done.

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						reproduce past observations". Initialized forecasts are a powerful tool for model validation. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	
11-531	11	19	32	19	32	Figure 6. I cannot see the yellow line in the upper panel. It would be also useful to show here the uncertainty related to these scores computed on the basis of the spread of the initialised and non-initialised integrations. [Susanna Corti, Italy]	The figure has been redrawn and simplified by showing only results for the five-year start date frequency hindcasts. There is only one colour used. The uncertainty of the multi-model ensemble for each fore cast has been added by drawing the interquartilic range.
11-532	11	19	32			Figure 11.6: suggest to highlight in the caption the fact that all axes scales are different between SAT, AMV, IPO for a particular quantity. Assume it's not ideal to put them all on the same scale? [Thomas Stocker/ WGI TSU, Switzerland]	The range of values of the indices varies, which explains why we chose to use different vertical scales. This has been explained in the revised version.
11-533	11	19				Figure 11.6: The legend identifies yellow with the every 5-year subset forecasts and red with the every 1-year subset forecasts, whereas the caption lines 39-40 says the opposite. Based on the information in the figure, the legend appears correct. [William Merryfield, Canada]	The figure has been redrawn and simplified by showing only results for the five-year start date frequency hindcasts. There is only one colour used in the new version.
11-534	11	20	1	20	1	I have not found in the Chapter a discussion, or a mention, of the role of background trend of climate variables on skill scores. That is, are the near-term prediction systems also "skilful" when correlations/RMSEs are computed on detrended time series? As far as I know, the skill scores are generally lower in this case, indicating that the near-term prediction systems show more difficulties to capture the fluctuations than the background signal. [François Massonnet, Belgium]	The following sentence has been added "It has been shown that a large part of the skill corresponds to the correct representation of the long-term trend as the skill decreases substantially after an estimate of the long-term trend is removed (van Oldenborgh et al 2012; Corti et al 2012)".
11-535	11	20	1	20	42	If they are to be retained, there should be some discussion of the bottom two rows in Fig. 11.7. [Government of United States of America]	Only two rows, the ones showing the RMSE-based diagnostics, have been retained and described in the revised text.
11-536	11	20	3	20	4	Keenlyside et al. 2008 show a robust increase in skill over the North Atlantic [Noel Keenlyside, Norway]	Many single-model systems show the increase in skill in the North Atlantic. However, the spatial distribution of the skill improvement varies from one system to another. The assessment takes into account the robustness of those results by
11-537	11	20	3			and again Hanlon et al., 2012; and Matei et al. 2012 [Gabriele Hegerl, United Kingdom]	Done.
11-538	11	20	7			add Matei et al.2012a [Daniela Matei, Germany]	Done.
11-539	11	20	8		9	Here, skill of the North Atlantic of AMV is stated to be linked with variation of AMOC. Knight et al show this in terms of variability. For skill assessment this link,however, has to be shown. Better replace skill by e.g. variability or please replace reference. [Wolfgang Müller, Hamburg]	Skill has been replaced by variability.
11-540	11	20	9	20	10	Knight et al, 2005, is not a decadal prediction study. I do not believe it can be used to link skill in AMV with predictions of AMOC [Jonathan Robson, United Kingdom]	The sentence has been simplified to "The improvement in retrospective AMV predictions from initialization (García-Serrano et al., 2012; Hazeleger et al., 2012b; Smith et al., 2010; Wouters et al., 2012) suggest that internal variability was important to AMV in the past. However, the interpretation is complicated because the impact on the skill varies slightly with the forecast quality measure used."
11-541	11	20	11			Dunstone, et al, 2011, does not assess real-world retrospective hindcasts, but instead examines idealized prefect-model experiments. I don't think that it can be used as a reference here [Jonathan Robson, United Kingdom]	The sentence has been simplified to "The improvement in retrospective AMV predictions from initialization (García-Serrano et al., 2012; Hazeleger et al., 2012b; Smith et al., 2010; Wouters et al., 2012) suggest that internal variability was important to AMV

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							in the past. However, the interpretation is complicated because the impact on the skill varies slightly with the forecast quality measure used."
11-542	11	20	23	20	23	Should this read "reduces the positive impact of the initialization in the multi-model average." ? [William Merryfield, Canada]	The sentence has been removed to simplify the section.
11-543	11	20	25	20	25	sub-polar Atlantic, which was shown to be skillfull in Figures 11.6 Fig 11.6 talks about the AMV region (0:60N), rather than the sub-polar region ~ 50:65N. It may be the case that the majority of the variability in the AMV comes from this region - but Figures 11.6 does not show this. [Dan Hodson, United Kingdom]	The whole paragraph has been rewritten.
11-544	11	20	25	20	26	Perhaps the most clear demonstration of this, including a demonstration of the mechanisms is by Yeager et al. 2012 [Noel Keenlyside, Norway]	Added.
11-545	11	20	25	20	26	p.20: It would help the reader if this sentence is qualified in terms of what variability in the sub-polar Atlantic is being studied. -- Written dec 3, 2012 [Aneesh Subramanian, India]	There are multiple references in the paragraph that can illustrate the reader.
11-546	11	20	25	20	30	In the midst of discussing climate timescales the authors start discussing weather statistics (tropical storms). Even that text tends to be incoherent, in mentioning the subpolar Atlantic with tropical storms. [Government of United States of America]	There are multiple references in the paragraph that can illustrate the reader of what is the link between the subpolar Atlantic and the frequency of tropical storms.
11-547	11	20	25			This sentence is confusing, and says that initialisation of subpolar atlantic, which is shown to be skillfull, provides skill. Is it supposed to say that it may provide skill else where? Note that figure 11.6 shows AMV index, and not for the subpolar atlantic. [Jonathan Robson, United Kingdom]	The whole paragraph has been rewritten.
11-548	11	20	26			add Matei et al. 2012a and Yeager at al. 2012 (Yeager, S., A. Karspeck, G. Danabasoglu, J. Tribbia, and H. Teng, 2012: A Decadal Prediction Case Study: Late 20th Century North Atlantic Ocean Heat Content. J. Climate, in press, doi:10.1175/JCLI-D-11-00595.1.) [Daniela Matei, Germany]	Done.
11-549	11	20	32	20	32	Should this read "Sugiura et al. (2009) report skill using a single forecast system" ? [William Merryfield, Canada]	Done.
11-550	11	20	41			We recommend replacing "robust negative skill difference" we "loss of skill with initiation" [Government of United States of America]	Done.
11-551	11	20	46	20	58	There are a lot of questionable statistical analyses in this figure. How is the autocorrelation taken into account? Most procedures based on modifying degrees of freedom have not been shown to be valid. The Fisher Z-transform is used to test differences in correlations, but this test assumes the different correlations to be independent, which is not the case when comparing initialized and non-initialized forecats (since they reference the same set of observations). Similarly, the F test for comparing variances assumes the variances are independent, but this is not the case for MSE of initialized and non-initialized forecasts, since they reference the same set of observations. [Timothy DelSole, United States of America]	The reviewer is right in that there is a certain degree of uncertainty in the way the effective sample size is computed. However, no methodology appropriate for the decadal prediction problem is yet available. The methodology used is described in Doblas-Reyes et al (2013) and, given the uncertainties, intends to be more conservative than the typical formula described in the text book of von Storch and Zwiers. This is expected to compensate for considering the variances as independent when testing for significance.
11-552	11	20	46	20	58	It would be helpful to label each row in this figure - e.g "EM Corr", "Corr. Diff", "RMSS", "EM RMSS ratio" - just to orientate or anchor the reader. [Dan Hodson, United Kingdom]	The figure has been simplified and left with only two panels. In this case, the reader will easily find
11-553	11	20	47	20	47	We believe this should say "correlation of ensemble mean." What is said 'Ensemble-mean correlation" could easily be confused for 'ensemble mean of correlation" [Government of United States of America]	Done.
11-554	11	20				Figure 11.7: Should consider defining or including a reference for "root mean square skill score" (line 56), since it is not very commonly used, its range from -infinity to infinity is unusual, and as defined here apparently differs from other usages, e.g. as in e.g. Cohen and Fletcher, J Clim 2007 (http://dx.doi.org/10.1175/JCLI4241.1) which is bounded by -infinity and 1. [William Merryfield, Canada]	References to the RMSSS can be found in the forecast quality section.
11-555	11	21	9	21	11	I don't think there are any improvements over land from initialization (Fig 11.7). Smith et al 2010 and Dunstone	Done.

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						et al 2011 relate AMV to improved predictions of Atlantic tropical storms. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	
11-556	11	21	9	21	12	how are the two sentences related? [Annalisa Cherchi, Italy]	The first sentence has been removed.
11-557	11	21	9		17	This paragraph is a bit mixed up and some clarification are required: (1) Skill of AMV is stated to be related to skill of AMOC. Again Knight et al referes to variability and skill linkage need to be shown. (2) Focus of this section is AMOC, therefore overarching linkage to teleconnection respectively skill improvement over land is a bit misplaced here. Would be better placed where skill over land is actually discussed. Linking skill in North Atlantic with skill over land (European area) and associated teleconnections is also investigated in Müller et al (2012), though surely this is not settled yet, [Wolfgang Müller, Hamburg]	The first sentence has been removed.
11-558	11	21	12	21	17	Another study (Pohlmann, H., D. M. Smith, M. A. Balmaseda, N. S. Keenlyside, S. Masina, D. Matei, W. A. Muller and P. Rogel, Predictability of the mid-latitude Atlantic meridional overturning circulation in a multi-model system, Climate Dynamics, submitted (in revision)) shows a common AMOC signal from ocean analyses, and that this signal is predictable for a few years with initialized models - I think this should be mentioned here. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Done.
11-559	11	21	19	21	20	Ottera et al. 2010 also showed this [Noel Keenlyside, Norway]	Done.
11-560	11	21	19	21	21	About the statement "In spite of the positive role of the initialization, recent studies (Booth et al., 2012; Chang et al., 2011; Evan et al., 2009; Villarini and Vecchi, 2012b) suggest that the observed AMV over the 20th century was strongly influenced by changes in atmospheric (natural and anthropogenic) aerosol loading", it should be distinguished here the respective roles of anthropogenic and natural aerosols in influencing AMV. [Jianqi Sun, China]	Unfortunately we have to reduce the space and have little space left to deepen on these details.
11-561	11	21	19	21	29	Not sure that I suggest a change, but my comments above (p19, lines 3-9) are relevant to this discussion. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	This paragraph has been made consistent with the one in p 19.
11-562	11	21	19		29	This paragraph also mixes up too many issue,e.g aerosol loadings (multi-decadal) and AMOC prediction (annual to multi-year). I would suggest to attach the AMOC prediction to the previous section. The aerosol loading issue could be stated somewhere else, e.g. model uncertainties. [Wolfgang Müller, Hamburg]	The forcing uncertainty and errors in model response are defined in the introduction already includes the aerosol uncertainty.
11-563	11	21	23	11	23	I would add agian the the Terray (2012)'s reference : Terray L, 2012: Evidence for multiple drivers of North Atlantic multi-decadal climate variability. Geophys. Res. Lett, 39, L197-12, doi:10.1029/2012 GL053046. [Christophe CASSOU, France]	Done.
11-564	11	21	23	21	25	Paper from Pohlman et al. 2012 is relevant here: Pohlmann, H., D. M. Smith, M. A. Balmaseda, N. S. Keenlyside, S. Masina, D. Matei, W. A. Müller, P. Rogel, and E. D. da Costa, 2012: Predictability of the mid-latitude Atlantic meridional overturning circulation in a multi-model system, submitted [Noel Keenlyside, Norway]	Done.
11-565	11	21	23	21	29	This part of the paragraph must be rewritten. The assessment of the results by Matei et al. (2012) is unbalanced; in their reply to Vecchi et al. (2012a), Matei et al. (2012, Science) show that if all skill measures employed in the original paper are used, their prediction skill is better than climatology. This superiority is even stronger if the extended observational time series is used. Hence, there is some support for the assessment that prediction skill can be obtained for the current-length observations, although agreement is low. Finally, in an IPCC report there should be no call for specific research or observational efforts. [Jochem Marotzke, Germany]	The paragraph has been rewritten and reads more balanced now.
11-566	11	21	23			A reference to Terray (2012) and Zhang et al. (2012) could be added as a support of this statement Terray, L., 2012: Evidence for multiple drivers of North Atlantic muli-decadal climate variability. Geophys. Res. Lett., 39, L19712. Zhang R., T. Delworth, R. Sutton, D. L. R. Hodson, K. W. Dixon, I. M. Held, Y. Kushnir, J. Marshall, Y. Ming, R. Msadek, J. Robson, A. Rosati, M. Ting, G. Vecchi, 2012: Have aerosols caused the observed multidecadal variability?,submitted [RYM MSADEK, United States of America]	References have been added.

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11-567	11	21	24			change Matei et al. 2012 to Matei et al. 2012b [Daniela Matei, Germany]	corrected
11-568	11	21	25	21	29	These lines are heavily biased towards Vecchi et al (2012; cited in line 25). At minimum, the reply by Matei et al. (2012) should be cited here as well. [European Union]	The paragraph has been rewritten and reads more balanced now.
11-569	11	21	25	21	29	Further, the bias towards Vecchi et al (2012; cited in line 25) should not go as far as claiming 'the fact remains' without giving a single reference. [European Union]	The paragraph has been rewritten and reads more balanced now.
11-570	11	21	28	21	29	The reference to the additional observations is generic for all oceanic quantities, and should be placed in a more prominent location to highlight a fundamental limitation for the verification of all (integrated) oceanic quantities. [European Union]	It has been thought that the lack of AMOC observations is one of the clearest examples of the need to obtain longer observational samples.
11-571	11	21	28	21	29	With regard to buiding a sustained observing system for the AMOC reference could be made to Srokosz et al. (2012) Bull. Amer. Met. Soc. who discuss this need. [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	Done.
11-572	11	21	31	0	0	Please state why this has been the case. [Antje Weisheimer, United Kingdom]	That sentence has been removed.
11-573	11	21	31	21	43	The introduction of an ensemble and what it represents is introductory material and should be mentioned before any results which are based on ensembles are discussed. [European Union]	The reliability is not just a property of the ensembles used to make predictions, but of the predictions themselves. All probabilistic statements should be considered for reliability. This is an extremely relevant concept for the users of climate information that is new to the IPCC report readers. Hence, it has been considered that, even if a brief introduction to reliability can be found in the forecast quality section, a careful explanation of what Fig 11.8 means is necessary here.
11-574	11	21	31	21	43	We feel that the authors are giving too much prominence to "reliability" as a forecast attribute. Reliability has to be considered in the context of other skill measures to be value. A forecast of climatology is a perfectly reliable forecast, but has no intrinsic value as it lacks any sharpness. So reliability is not a stand alone measure with some absolute value. [Government of United States of America]	The multi-model ensemble of decadal prediction models allows to forecast not only an accurate multi-annual mean temperature, but also a range of values. This range as estimated from the CMIP5 multi-model ensemble is fairly accurate, although the reliability diagrams suggest that it's not perfect. In other words, the probability predictions are reliable or the uncertainty estimates of the mean predictions are accurate, which is the same as saying that the multi-model spread describes most of the uncertainty. The accurate range is relevant not only for the unusual tails of the multi-annual average values, but for the whole range of values around the mean prediction (typically the multi-model ensemble mean), which should also be reliable. Although the reviewer finds an excess of importance to the reliability of the predictions, we find that this is the first time such an evaluation of the simulations is performed. Although a climatological probability forecast is perfectly reliable, the predictions discussed in the chapter are skillful AND reliable.
11-575	11	21	31		43	This paragragh refers to a technical implementation of skill assessment (reliability and uncertainty), whereas previous and following paragraphs refer to what skill we actually have assessed. The readability may be improved if this paragraph is placed somewhere else e.g. 11.2.3.2 [Wolfgang Müller, Hamburg]	All probabilistic statements should be considered for reliability. This is an extremely relevant concept for the users of climate information that is new to the IPCC report readers. Hence, it has been considered that, even if a brief introduction to reliability can be found in the forecast quality section, a careful explanation of

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							what Fig 11.8 means is necessary here.
11-576	11	21	35			We recommend replacing "whether decadal" with "whether probabilistic decadal." [Government of United States of America]	The whole sentence has been simplified.
11-577	11	21	38	21	43	is the method been applied also to other kind of predictions? [Annalisa Cherchi, Italy]	They are standard measures of forecast quality in weather and seasonal forecasting.
11-578	11	21	45	21	54	I think that the introduction of reliability diagrams is material for the forecast quality subsection (11.2.3.2). [European Union]	All probabilistic statements should be considered for reliability. This is an extremely relevant concept for the users of climate information that is new to the IPCC report readers. Hence, it has been considered that, even if a brief introduction to reliability can be found in the forecast quality section, a careful explanation of what Fig 11.8 means is necessary here.
11-579	11	21	45	21	54	The discussion on attributes diagrams could be clearer. Suggested rewriting of this paragraph ([] indicate changes): Figure 11.8 illustrates the CMIP5 multi-model Init and NoInit attributes diagrams for predictions of the North Atlantic SSTs to be below the lower tercile. For perfect reliability the forecast probability and the frequency of occurrence should be equal, and the plotted points should lie on the diagonal (solid black line in the figure). [The] line joining the bullets (the reliability curve) [having] positive slope indicates that as the forecast probability of the event occurring increases, [so] does the verified chance of observing the event. The predictions therefore can be considered as [reliable to some degree]. However, if the slope of the curve is less than the diagonal, then the forecast system is overconfident[, whereas if] the reliability curve is mainly horizontal, then the frequency of occurrence of the event does not depend on the forecast probabilities [and resolution is low]. In this situation a user might make some very poor decision based on such uncalibrated probabilities. An ideal forecast should have high resolution whilst retaining reliability, [and] should be [] sharp[, i.e., able to distinguish from climatological predictions. [William Merryfield, Canada]	The whole paragraph has been rewritten.
11-580	11	21	45	21	54	As mentioned above, I think that the paper by Corti et al., GRL 2012, is the first probabilistic analysis of decadal forecasts. As such it and Fig 11.8 should get a much more prominent place in this assessment. Some of the explanations of the reliability diagram are perhaps too technical for the report and could be simplified. [Antje Weisheimer, United Kingdom]	The whole paragraph has been rewritten.
11-581	11	21	45			We recommend, at end of this line, inserting "global ocean and" [Government of United States of America]	Done.
11-582	11	21	46	21	46	First: is this for predictions of the North Atlantic area mean SST or have statistics been aggregated over individual grid boxes? Second: results for global SSTs are also included in the figure. [Jouni Räisänen, Finland]	The diagrams are constructed using predictions for each grid point over the corresponding area. They do not correspond to the attributes diagrams of the area-averaged SSTs.
11-583	11	21	48	21	49	This sentence is awkward and difficult to understand. [Timothy DelSole, United States of America]	The sentence has been reworded.
11-584	11	21	53	21	54	This sentence is not grammatically correct. [Timothy DelSole, United States of America]	The sentence has been modified.
11-585	11	21	56	21	57	To say that predictions are 'to some degree reliable" seems vague and not worth saying. [Timothy DelSole, United States of America]	The sentence has been modified to "CMIP5 multi-model surface temperature predictions are more reliable for the North Atlantic than when considered over the global oceans, and have a tendency to be overconfident particularly for the global oceans".
11-586	11	21	56	22	3	About reliability and BSS. [Susanna Corti, Italy]	Done.
11-587	11	21	56	22	3	I think that here would be useful to explain briefly that a forecast can have no skill, but it can be reliable. A good example of this is a system always forecasting climatological probability. This system will be by definition reliable (but not skilful). (This is the case of the upper panels in figure 8 where the BSS is very low but the system is reliable). To understand better the concept It would be useful to introduce the decomposition of BSS as well. A positive value of BSS indicates a forecast that is better than climatology. The BSS can be	The section tries to strike a balance between detail and concision. While some of these details have been included, the explanation of the BS decomposition might distract the reader from the main message.

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						decomposed as follows (Murphy 1973): BSS=BSS_rel+BSS_res-1, where BSS_rel and BSS_res are the reliability and the resolution components of the score. The reliability measures how close the forecast probabilities are to the observed frequencies. The resolution measures how much the forecast probabilities differ from the climatological probability of the event. By definition when the forecast is always the climatological probability, the system is perfectly reliable (BSSrel = 1), but has no resolution (BSSres = 0) and thus no skill (BSS = 0). The values of BSS_rel and BSS_res can be drawn in the upper corners of figure 8 to complement the information. [Susanna Corti, Italy]	
11-588	11	22	2	22	2	The large uncertainty in the reliability diagrams is associated to the limited number of verification points. [Susanna Corti, Italy]	Done.
11-589	11	22	2			Is 'makes' the right word here? [Timothy DelSole, United States of America]	The sentence has been rephrased.
11-590	11	22	3			Although the elements of Fig. 11.8 are cataloged no discussion of what these figures tell us about the topic at hand is provided. What specifically are we to glean from Fig. 11.8? [Government of United States of America]	This figure shows the first reliability assessment of a multi-model climate prediction and can be considered an illustration of the degree of validation necessary to assign trustworthiness, a highly desirable feature for the users, to the information provided by the CMIP5 climate models.
11-591	11	22	19	22	25	L19-25, It is really hard to see prediction skill in precipitation in the presentation. The figure is very noisy. It would seem more sense to construct some indices for different regions, and use a presentation as in Fig. 11.6. [Noel Keenlyside, Norway]	The figure is more readable in the revised version after a spatial smoothing has been applied. The use of spatially-aggregated indices has been discarded due to the heterogeneity of the precipitation field.
11-592	11	22	19			"Figure 9" would be "Figure 11.9" [Yoshimitsu Chikamoto, United States of America]	Done.
11-593	11	22	19			The skill map also has numerous negative values. This whole discussion seems pretty dismissive of negative values and does not even raise the question of whether the skill map has field significance. In my opinion, this discussion would not pass peer-review standards due to lack of discussion of field significance. [Timothy DelSole, United States of America]	Field significance is useful only when trying to interpret small features in a map. Instead, decadal prediction skill of precipitation tries to make use of the knowledge of the precipitation trends and the impact of known teleconnections. The reader is also made aware of the lack of statistical significance with high confidence level.
11-594	11	22	19			It is unclear whether the authors mean that there are several regions with skill or there only a few regions with skill. It would be useful if the intent were clarified. [Government of United States of America]	The sentence has been changed to "The skill for land precipitation (Figure 9) is much lower than the skill for temperature. Several regions, especially in the Northern Hemisphere, display statistically significantly positive values."
11-595	11	22	20	22	22	I'm surprised even that there is much skill associated with the forced signal. The detection of anthropogenic influence on precipitation is marginal and has only been achieved in zonal mean data, so I would not expect a large forced signal at the grid point level, except perhaps in the Arctic. See section 10.3.2.2. [Nathan Gillett, Canada]	The figure attempts to show what the skill of the state-of-the-art experiments is, along with statistical significance estimates. There is no intention to claim that there is much skill for precipitation.
11-596	11	22	20	22	22	One should be very careful attributing skill of precipitation forecasts. [Antje Weisheimer, United Kingdom]	The skill is what is shown in Fig 11.9. However, the skill obtained is not statistically significantly different from zero with confidence above 95%.
11-597	11	22	28	22	45	It would be helpful to label each row in this figure - e.g "EM Corr", "Corr. Diff", "RMSS", "EM RMSS ratio" - just to orientate or anchor the reader. [Dan Hodson, United Kingdom]	The figure has been simplified and left with only two panels. In this case, the reader will easily find
11-598	11	22	55	22	60	The discussion of the 1 year vs 5 year initialization frequency should be an individual paragraph, and it also needs references for 'clearly more robust', and 'does not change substantially', or an indication that this is unpublished - but generally known. [European Union]	This discussion has been removed. Results from hindcasts with five-year intervals between start dates are the only ones shown and discussed in the revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-599	11	22	55	23	5	The entire paragraph is about the fundamentals of the experiment design, and should come much earlier. [European Union]	The paragraph is a more in depth discussion of the results shown in Figs 11.6-9 and would be difficult to understand without the previous results.
11-600	11	22	57	22	58	It is not clear to me that increased start dates (e.g. annual vs every 5 years) increses the number of statistically significant results. For example, with annual start dates, the time-mean realisations of 2-5 year means are not independent from each other any further which has impotant implications for the significance. [Antje Weisheimer, United Kingdom]	The references show that the increased number of start dates changes the results, and hence their significance. The effective sample size of the time series used also changes because the sample used is different. To avoid speculating, this part of the sentence has been removed.
11-601	11	23	2	23	2	It's not at all obvious to the eye that "spatial variability is substantially reduced" in the right column of Fig. 11.10a as compared to the left column. Can this statement be justified? [William Merryfield, Canada]	This figure has been removed.
11-602	11	23	7	23	7	Caption of figure 10a. Here there is no description of the panels in the four row in the figure. [Susanna Corti, Italy]	The caption has been corrected.
11-603	11	23	7	23	23	It would be helpful to label each row in this figure - e.g "EM Corr", "Corr. Diff", "RMSS", "EM RMSS ratio" - just to orientate or anchor the reader. [Dan Hodson, United Kingdom]	The figures have now labels and titles.
11-604	11	23	25	23	41	The statement "These results confirm that there is substantial skill in multi-annual predictions of temperature and non-trivial skill for land precipitation. Most of the skill is due to the slowly varying changes in atmospheric composition, both natural and anthropogenic." implies that the annual and subdecadal prediction challenge is no longer an issue. It is also a bit misleading since most of the skill seems to be over the ocean rather than on land where people live. The runs initialized in 2012 (Figure 11.10b) seem to show strong five-year warming trends that likely resemble the concentration-driven experiments for these models for the same five years. An additional set of maps to Figure 11.10b but for runs initialized in 2000 or 1990 would be insightful. [Government of United States of America]	This discussion has been reduced to reduce the text length. Figures 11.10b and c have also been removed. Finally, the fact that there is substantial skill for multi-annual predictions of temperature does not imply that the prediction challenge is no longer an issue because what matters is improving the usefulness of the predictions, and the variables that can be useful go beyond temperature (precipitation, wind, radiation, etc).
11-605	11	23	28	23	28	Please specify which measure of forecast quality. [Government of United States of America]	This sentence has been removed to reduce the text length.
11-606	11	23	29	23	29	How can the initialisation degrade forecast quality compared to an uninitialised forecast? This must reflect failings in the initialisation scheme, musn't it? [Nathan Gillett, Canada]	Yes, there might be some issues with the initialization. The CMIP5 exercise included several forecast systems that did not have much experience on initializing the different components.
11-607	11	23	29	23	41	This paragraph discusses plans to develop a quasi-operational intiative. This topic seems out of place for a review. If this initiative has developed to the point of producing publishable papers, then those papers should be cited. If not, then why mention it? Why give special recognition to this particular initiative and not mentioned other intiatives? Why show 'an example' of a preliminary initiative? I believe this whole section and figure should be deleted because it is inappropriate. [Timothy DelSole, United States of America]	The figures have been eliminated, although the reference to the paper that describes the near-real time decadal prediction initiative has been retained given the importance of such initiative. There are not, to the best of our knowledge, any other international initiatives with similar objectives. The global-mean temperature from the near-real time predictions have been included in a new version of Figure 11.12, where several projections and the 30-year predictions are also plotted
11-608	11	23	33	23	34	This sentence has to be rewritten “The forecast is experimental.....” [Aneesh Subramanian, India]	Done.
11-609	11	23	35	23	36	the forecasts used within the decadal exchange exercise have been started near the end of 2011! (and not 2012) [Daniela Matei, Germany]	This figure is a placeholder for the set of predictions started in 2012. However, the figure has been finally removed.
11-610	11	23	36	23	36	I suspect these initialized predictions were started near the end of 2011, or if "2012" and current Figs. 11.10b,c are placeholders that should be indicated. [William Merryfield, Canada]	This figure is a placeholder for the set of predictions started in 2012. However, the figure has been finally removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-611	11	23	38	23	40	Does this reflect correction of model biases? Or is this more subtle? Explain. [Nathan Gillett, Canada]	Biases have been removed, the warming in the Atlantic is part of the internal variability correctly predicted by the initialized systems.
11-612	11	23	38	23	40	Are these differences simply due to the persistence of the initial state (e.g., the uninitialized simulations exaggerating the global sea surface warming this far) or some more complicated mechanism? [Jouni Räisänen, Finland]	This information is still not available in the literature.
11-613	11	23	38			Initialization doesn't "cause" anything. The wording should be changed. Initialization can lead to a prediction of substantial warming. [Government of United States of America]	Done.
11-614	11	23	39			The discussion mentions "cooling in the Pacific", but this is relative to the unitialized prediction. We feel that it would be much clearer to discuss some of the details of warming first and then the comparison is not cooling, but rather a lower level of warming (if that is the case). [Government of United States of America]	This figure has been removed, as well as the corresponding discussion.
11-615	11	23	40	23	41	The rise in global temperature is probably slower in the intialised forecasts (see Fig. 11.6). [Noel Keenlyside, Norway]	This has been commented in the revised text.
11-616	11	23	43	23	52	I would present here (for the sake of brevity) only one figure (11.12) with 2 panels: a) the multi-model of figure 11.10b, b) the multi-model of figure 11.10c [Susanna Corti, Italy]	This figure has been removed to reduce the text length.
11-617	11	23	44	23	44	As for line 36, I suspect these initialized predictions were started near the end of 2011, or if "2012" and current Figs. 11.10b,c are placeholders that should be indicated. [William Merryfield, Canada]	This figure is a placeholder for the set of predictions started in 2012. However, the figure has been finally removed.
11-618	11	23	44	23	46	I understand that these are predictions for the period 2013-2017. So I do not understand the reference to the “observed” temperature anomalies in the caption. [Susanna Corti, Italy]	This figure has been removed.
11-619	11	23	45	23	45	degrees centigrade? [Ian Watterson, Australia]	This figure has been removed.
11-620	11	23	50	23	50	As for line 36, I suspect these initialized predictions were started near the end of 2011, or if "2012" and current Figs. 11.10b,c are placeholders that should be indicated. [William Merryfield, Canada]	This figure is a placeholder for the set of predictions started in 2012. However, the figure has been finally removed.
11-621	11	23	54	23	55	This sentence is wrong: the ENSEMBLES and CMIP5 experiments do not provide estimates of current skill. This is because of the point made in lines 17 to 21 of page 11-24. For the past few years we have had full coverage of data from Argo floats, which are expected to improve predictions as stated on page 11-24. So current skill, which can benefit from the established Argo data coverage, is likely higher than indicated by the ENSEMBLES and CMIP5 experiments, which used initial dates from the period 1960-2005. The problem, of course, is that we do not have the Argo data for long enough to estimate just what is the level of current skill. [Adrian Simmons, United Kingdom]	We agree with the comment. However, this sentence has been removed to reduce the text length.
11-622	11	23	56	24	3	This part has a confusing jump. It begins by saying that understanding is being found for 2 events; in the North Atlantic in the mid-1990s, and in the Pacific in the 1960s. It then goes on to talk about the enchancement of northward heat transport related to the NAO played a key role. The way this is written makes it sound like the NAO was important in the Pacific aswell. Recomend explicitly saying, 'For the North Atlantic mid-1990s warming event, it has been shown.....' [Jonathan Robson, United Kingdom]	We agree with the comment. However, this discussion has been removed for problems with the space allocated to the chapter.
11-623	11	23				Comment on Fig.10b and 10c. I am firmly opposed to have these 2 figures in the chapter. An assessment report should not be a climate bulletin and should not provide an operational forecast, all the more that it is an experimental initiative (which I fully support as such) . The mechanisms leading to the fact that the 2012 predictions are colder from the projections are still under debate. Is it really due to the ocean initialization? Is it due to the fact that the prescribed aerosol forcing in the model (RCP45) from 2006 to 2012 is not consistent with the actual observed one over that period? The message about the added-value of the initialization would be very different in that case. Is it due to a small volcanic background that would lead to a colder ocean state in 2012? In addition, considering the skill of the forecasts as honestly and nicely described throughout the chapter , I would not dare to provide an operational forecast. I suggest either to remove these two figures or to replace the 2012 forecast by the 2005 forecast that all the groups have performed following the CMIP5	These figures have been removed precisely to avoid the issues that the reviewer mentions. However, the global-mean temperature from the near-real time predictions have been included in Figure 11.12, where several projections and the 30-year predictions are also plotted. The reference to those predictions has been however included, which doesn't seem to be in contradiction with what the reviewer claims. There is a new discussion of the recent hiatus in global-mean temperature now included in Chapter 9 that

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						protocol. It will be much more interesting, much more constructive and more scientifically solid to see if the models do forecast the "observed plateau" when initialized in 2005. Indeed both projections and predictions would have shared the same external forcings while the 2012 observed ocean state used for initial conditions has not. A verification map coud be added in that case because data are available over the 2005-2011 period. In other word, I think that it is extremely dangerous to provide Fig.10b and 10c in the report, even if I participated to Doug Smith' initiative from which the figures have been taken. [Christophe CASSOU, France]	discusses, among other things, the prediction of the hiatus as mentioned by the reviewer.
11-624	11	24	1	24	1	played the key role … in the increase of the North Atlantic SSTs (the previous sentence also mentions the North Pacific)? [Jouni Räisänen, Finland]	This discussion has been removed for problems with the space allocated to the chapter.
11-625	11	24	3			Note that Robson et al, 2012b, also shows some prelimenry evidence that the succesful prediction of the subpolar change allowed predictions of a shift in the Surface climate in the Atlantic region, which is similar to that observed. [Jonathan Robson, United Kingdom]	This discussion has been removed for problems with the space allocated to the chapter.
11-626	11	24	4	24	8	Keenlyside et al. 2008, also show improvements in the Pacific [Noel Keenlyside, Norway]	This discussion has been removed for problems with the space allocated to the chapter.
11-627	11	24	10	24	38	Here you lack a discussion on how to capture teleconnections to ocean variability. Recent work for example indicates that resolving stratosphere-troposphere interaction could be important for capturing the NH winter response to AMV (Omrani et al. 2012) Also, I don't see any discussion on the benefits for example for fisheries, the North Atlantic being one region where marine ecosystems are strongly coupled to multi-decadal oceanic variations. Omrani, N.-E., N. S. Keenlyside, J. Bader and E. Manzini, 2012: Stratosphere key for wintertime atmospheric response to warm Atlantic decadal conditions, submitted to Clim. Dyn [Noel Keenlyside, Norway]	The discussion is limited by the availability of related material. In particular, the reference mentioned does not seem to be published by the IPCC deadline.
11-628	11	24	12	24	13	Again, assessments of predictability based on "idealized" models could be all wrong due to model biases. Such measures need to judge in the context of other validations, e.g., how does the total variance simulated by the idealized model compare with the observed variance. [Government of United States of America]	Done.
11-629	11	24	12	24	15	A lack of understanding of internal vs external contributions is also relevant here, and has been discussed above for AMV and PDV. [Noel Keenlyside, Norway]	The improvement should be carried out in a more general context. The Solomon et al reference has been added to discuss this issue.
11-630	11	24	12	24	15	In this section the limited availability of data is highlighted as a main hurdle, which is of course true. It may be good though to make an explicit seperation between lack of data for a particular initial or validation time, and the lack of a long period of observations. i.e. need to emphasize that there is a lack of 'events' to test against, as well as poor data for each event. This means that it is incredible hard to validate the skill and reliability of large decadal change events, and of course means that improved observations, i.e. Argo etc for say the next decade, is not going to hugely improve our ability to validate hindcasts if no marked decadal changed events take place. [Jonathan Robson, United Kingdom]	Done.
11-631	11	24	12	24	15	p.24: References or elaboration on these points would help the interpretation of these points better. -- Written dec 3, 2012 [Aneesh Subramanian, India]	All references available have been added.
11-632	11	24	12	24	38	Are there any studies which suggest that modelling and initialisation of factors such as vegetation could be sources of predictive skill? Perhaps it could be important for P-E. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Not to our knwoledge.
11-633	11	24	17	24	18	I suggest "It is expected ... Argo floats has given a recent step change", since ARGO has been in the water for some time now. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Done.
11-634	11	24	17	24	21	Has the impact of the Argo and altimetry data been assessed in perfect model forecasts? It seems to me that this could be done even if there isn't much real data against which to vertify forecasts. [Nathan Gillett, Canada]	Dunstone and Smith (2010) have done so. The reference has been added.
11-635	11	24	17	24	21	It might be worth mentioning the prospect of Argo floats that descend deeper than the current ones, as	We could not find publications addressing this issue.

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						implementation of these could be expected to bring further improvement to decadal predictions. [Adrian Simmons, United Kingdom]	
11-636	11	24	20	24	26	The details of the satellite data need to be corrected. Altimetry did not first appear in 1992. SeaSat was launched in 1978, though it only lasted 105 days. GEOSAT was launched in 1985 and lasted 5 years. GFO was launched in 1998 and lasted about 10 years. Of course none of these satellites provided data of the same caliber as TOPEX/Poseidon (which was launched in 1992) and the Jason follow-on series. SMOS was launched in 2009 and CryoSAT-2 in 2010. [Government of United States of America]	Done.
11-637	11	24	25	24	26	Both SMOS and Cryosat have been in orbit for some time - the word "planned" should be removed from both of them. Perhaps a caveat should be added that both these instruments will give only relatively short datasets. [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Done.
11-638	11	24	25			SMOS was indeed planned, but it has in fact been in orbit for more than three years - it was launched on 2 November 2009. [Adrian Simmons, United Kingdom]	Done.
11-639	11	24	26			ESA's Climate Change Initiative plans to produce a sea-ice thickness dataset from 1993 onwards based on radar altimeter data; this could be mentioned as well as Cryosat-2. [Adrian Simmons, United Kingdom]	A reference would be needed to contrast the information.
11-640	11	24	26			And Cryosat-2, like SMOS, is not just planned, it was launched on 8 April 2010. It is a bit disturbing that this paragraph is so out-of-date. [Adrian Simmons, United Kingdom]	Done.
11-641	11	24	28	24	30	p.24: Caveats or reasons for why using 4DVAR or ensemble based assimilation schemes for cannot be practically achieved in the present day can be mentioned here. -- Written dec 3, 2012 [Aneesh Subramanian, India]	This would require a space that is not available in the current version of the chapter.
11-642	11	24	28	24	31	There is nothing inherently coupled about 4DVar or EnKF. Nor have they proven to be particularly skilled in a coupled setting. If it makes sense to do "coupled assimilation" (and not everyone agrees that it does), then simple methods can be coupled just as easily as "sophisticated" methods. [Government of United States of America]	The sentence has been corrected and "sophisticated" has been removed.
11-643	11	24	30			In addition to Zhang et al. 2007a, it would be appropriate to include Keppenne et al. 2005: Keppenne, C. L., Rienecker, M. M., Kurkowski, N. P., and Adamec, D. A.: Ensemble Kalman filter assimilation of temperature and altimeter data with bias correction and application to seasonal prediction, Nonlin. Processes Geophys., 12, 491-503, doi:10.5194/npg-12-491-2005, 2005. [Government of United States of America]	Done.
11-644	11	24	33	24	38	Maybe cite Palmer & Weisheimer, GFD (2011) on how to reduce biases in climate models [Antje Weisheimer, United Kingdom]	Done.
11-645	11	24	36	24	38	This is a research recommendation - not allowed in IPCC assessments, I think. [Nathan Gillett, Canada]	This stateemnt helps to explain the relative importance of reasons underpinning current limitations. It is therefore a useful component of our assessment.
11-646	11	24	40			Section 11.3. This section would benefit from more assessment of model projections in the context of observed changes. This is done already in parts of the section, but in some subsections there is a tendency to just write 'The models do this' without really making an assessment about what the likely change is in the real world. For example 11.3.2.2, on projected changes in the free atmospheric temperature, just reports the pattern of free tropospheric temperature trends simulated by the models. There is no reference here to section 9.4.1.3.2 where it is concluded that most CMIP3 and CMIP5 models overestimate the warming trend in the tropical troposphere over the satellite period. If the authors are discussing a variable for which simulated and observed trends disagree this should be discussed and considered in the assessment of future changes. Another example is the ES, ln 20, where a prediction of Arctic sea ice extent decrease is made of -28% for September for 2016-2035 relative to 1986-2005. But I believe that the observed decrease in Arctic sea ice extent in September has already substantially exceeded this. This needs to be reported next to the projected changes (observations of ice changes are discussed withinin the chapter, but this needs to be taken up to the ES). More generally, I think it is more useful to make statements about future changes in the real climate system, even if the confidence level is lower and perhaps expert judgement is involved, rather than just to	This is a useful comment, which we have generally tried to follow in a number (but not all) relevant locations. Revisions to section 11.3 have been made to take greater account of evidence from other chapters about the reliability of models for specific projections. One area where we have not followed the reviewer is the section on free tropospheric temperature trends specifically mentioned by the reviewer. Early in the revision process, it was decided to significantly shorten this discussion in chapter 11 (and to drop the corresponding figure that was in the SOD). The discussion of the more theoretical papers on atmospheric moistening is now in section 7.2.4, and other aspects relevant to tropospheric

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						report what the models project. [Nathan Gillett, Canada]	temperature trends in section 10.3.1.2.1 and 12.4.3.2. We now provide the reader with a pointer towards these sections. Statements regarding the future of Arctic Sea Ice have been revised in coordination with other relevant chapters.
11-647	11	24	52	24	55	It would be more useful with shorter time periods for the near term changes. The same apply to the reference period and the reference period should be closer to current time. The interannual variability will be larger, but both over the reference period and 2016-2035 there is an important trend. [Gunnar Myhre, Norway]	What is most useful depends on the user. As the reviewer notes, shorter time periods are more strongly influenced by interannual variability, which obscures the signal of change. There is no perfect choice here. The decision to use a twenty year period and to use a reference period of 1986-2005 was taken after careful discussion at the first Lead Author meeting.
11-648	11	24	53		53	The period chosen is too short (16 years), which is not compliant. Under these conditions, the findings are marred doubts! [Ibouraïma YABI, Benin]	The period chosen is twenty years long. Note that some information is provided for successive ten and twenty year periods, e.g. in Fig 11.32.
11-649	11	25	1			Another question that could be added concerns the consistency of current observed trends and near-term projections. [Clare Goodess, United Kingdom]	Comparisons with observations are discussed in Chapters 9 and 10.
11-650	11	25	2	25	8	Four instances of 'natural variability' should be 'internal variability'. [Nathan Gillett, Canada]	Accepted and amended
11-651	11	25	7	25	9	As written it sounds as though there is one date at which climate change emerges from internal variability. Of course this depends on the variable, location, averaging region and averaging period. Overall, I think this makes the idea of a date at which the signal emerges from the noise less clearly defined than it initially appears. [Nathan Gillett, Canada]	Accepted. Text here has been shortened and the discussion of emergence in 11.3.2.1.1 has been revised.
11-652	11	25	7	25	9	This is one question that might be relevant for some aspects of adaptation planning, but certainty not all, and needs qualification. In fact the statement is close to saying that projected changes not potentially detectable against noise are of no interest. This is absolutely not true. If we have confidence in a projected change, this information can be used in adaptation planning even for times where this change may not yet be detectable against noise. Such a trend will be changing the odds of wet and dry years, or hot and cold ones, and such information is actionable in adaptation planning. [Government of Australia]	Accepted. Text here has been shortened and the discussion of emergence in 11.3.2.1.1 has been revised.
11-653	11	25	7	25	9	What does "clearly" mean? Emergence of the signal above noise is also dependent on what one is using for the climatological base period. A signal in 2100 may be clearly emergent relative to present climate, but not so much clear relative to a contemporary climate. [Government of United States of America]	Accepted. Text here has been shortened and the discussion of emergence in 11.3.2.1.1 has been revised.
11-654	11	25	7	25	9	This is one question that might be relevant for some aspects of adapation planning, but certainty not all, and needs qualification. In fact the statement is close to saying that projected changes not potentially detectable against noise are of no interest. This is absolutely not true. If we have confidence in a projected change, this information can be used in adapation planning even for times where this change may not yet be detectable against noise. Such a trend will be changing the odds of wet and dry years, or hot and cold ones, and such information is actionable in adaptation planning. [Penny Whetton, Australia]	Accepted. Text here has been shortened and the discussion of emergence has in 11.3.2.1.1 has been revised.
11-655	11	25	11	25	12	But the impacts may be more sensitive (e.g., when consider population/land-use, other changes associated with the emissions scenario. [Clare Goodess, United Kingdom]	Agreed. Impacts are primarily addressed by WG2 but a note has been added here.
11-656	11	25	11	25	13	The caption provides no units, and also change "brackets" to "parentheses". [Stephanie Downes, Australia]	*It is unclear what this comment is referring to. Are the page and line references wrong?*
11-657	11	25	11	25	19	I find this paragraph a bit confusing. On the one hand, the statement is that near-term projections are insensitive to forcing scenario (hence the focus on RCP4.5), but on the other hand the statement is that near-term projections are sensitive to forcing scenario (noteably for anthropogenic aerosols). At the very least I suggest a reference supporting the latter statement. [Fyfe John, Canada]	The paragraph does clearly distinguish between comparative insensitivity to greenhouse gas scenario, but sensitivity to aerosol scenario. A reference has been added as suggested.
11-658	11	25	30			suggest to refer to Chapter 1 here, in particular Section 1.4.3 introducing the AR5 Uncertainty Guidance Note	Accepted. Reference to Chapter 1 (Section 1.4) has

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						etc. [Thomas Stocker/ WGI TSU, Switzerland]	been inserted.
11-659	11	25	32	25	56	Authors in this section have described very well uncertainties associated with projections in this chapter; [Government of United Republic of Tanzania]	Thank you
11-660	11	25	32	26	16	Discussions on underlying 3-type uncertainty is also extensively covered in section 12.2.2. Cross-referencing to this section may be beneficial. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. References to the relevant discussions in Chapters 1 and 12 have been inserted.
11-661	11	25	32	26	46	In fact, in addition to the three sources of uncertainty, the uncertainty of the projections also depends on variables, geographic location, and forcing intensity (Deser et al. 2012; Hu et al. 2012). Deser, C., A. Phillips, V. Bourdette, and H. Teng: 2012: Uncertainty in climate change projections: the role of internal variability. Clim. Dyn., 38(3-4), 527-546. DOI: 10.1007/s00382-010-0977-x. Hu, Z.-Z., A. Kumar, B. Jha, and B. Huang, 2012: An analysis of forced and internal variability in a warmer climate in CCSM3. J. Climate, 25 (7), 2356-2373. DOI: http://dx.doi.org/10.1175/JCLI-D-11-00323.1. [Zeng-Zhen Hu, United States of America]	It is already stated on page 26 of the SoD, lines 19-20, that the relative importance of the three sources of uncertainty "depends on the variables of interest, the space and time scales involved, and the lead time of the projection"
11-662	11	25	34	25	34	Add AMV to IPO. AMV is as important as IPO for which the predictability is weak anyway. [Christophe CASSOU, France]	Noted, but the IPO is only mentioned as an example not because it is more important than AMV. In fact the IPO example has now been replaced by ENSO, which is a clearer example of internal variability. The relative importance of internal processes and forced responses is still uncertain for the observed AMV and IPO.
11-663	11	25	34	25	35	This line seems to me contradictory since projections evacuate the problem of internal variability. Maybe the phrase should state "predicted." [Ramon de Elia, Canada]	Disagree. Projections do not exlcude internal variability. Internal variability is simulated by the climate models which are used to generate projections.
11-664	11	25	37			Is it possible to say anything about the extent to which land-use changes may or may not be a source of uncertainty in near-term projections? [Clare Goodess, United Kingdom]	There is very little literature on this question so a detailed assessment is not possible. Some brief discussion is included in 11.3.6.1.
11-665	11	25	44			please add references to Chapters 3 Ocean Obs and 4 Cryosphere Obs when mentioning internal variability and how it can be estimated from observations, in addition to Chapter 2 [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. References to Chapters 3 and 4 have been inserted..
11-666	11	25	54	11	55	This sentence seems to be a bit misleading in the context of the intent of the development of the RCPs to move away from the socio-economic scenario focus of the SRES scenarios. Being clear on the differences between the SRES and RCP approaches will be an important part of communicating the outcomes of the AR5. This is generally an easy to read section of the chapter, so it would be good if it was absolutely clear in this respect. [Government of Australia]	Accepted. Text has been clarified.
11-667	11	26	1	26	46	What could be the physcal implication to the climate system due to the decreasing ozone and incresing Carbon dioxide. Assuming that ozone recoveres to its natural composition? [Government of United Republic of Tanzania]	This issue is addressed in 11.3.2.4.
11-668	11	26	18	26	32	The paragraph doesn't say how the internal variability component of the uncertainty is calculated. Also, Deser et al, 2012, have shown that there can be considerable internal variability in one model when run under a large ensemble. The CMIP5 models do not have such a large ensemble, so does this mean that the internal variability component in figure 11.11 is underestimated? [Jonathan Robson, United Kingdom]	Accepted. A note has been added to indicate how internal variability was estimated. We are explicit that this is just an estimate. Internal variability could in reality be larger or smaller, but it is very unlikely such a result would change the key points 1-3.
11-669	11	26	18	26	46	Although there is some attempt here to explain the difference between uncertainty and model or scenario spread, it still seems to me that there is an assumption that the RCPs and the CMIP5 models are representative of the uncertainy we would assess using all the information from the multi-model ensemble, perturbed physics ensembles, observations etc. Probably this is less of a problem for the model response	Accepted. Text and figure caption have been amended; in particular, the figure now shows "model spread" and "RCP scenario spread".

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						uncertainty than it is for the RCPs, which are simply scenarios with no likelihood attached to them. I would suggest the wording and figure captions are carefully looked at and 'uncertainty' replaced by model or RCP spread where appropriate. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	
11-670	11	26	40	26	40	In the context of growth of signal-to-noise with time following paper is very relevant : Hu, Z.-Z., et al., 2012: An analysis of forced and internal variability in a warmer climate in CCSM3. J. Climate, 25, 2356-2373. [Government of United States of America]	Noted. However, this paper does not change the basic high level points that are being made here.
11-671	11	26	42	26	46	It might be worth noting that the maximum signal to noise ratio could be at shorter lead times for initialized predictions if they are successful at predicting internal variability. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Yes but this raises questions of how the signal is defined, which there is not space to discuss here. Initialised predictions are discussed in 11.2.
11-672	11	26	44	26	46	Suggest slight rewording: “The latter feature arises because over the first few decades the signal grows when scenario uncertainty is small, but subsequently the contribution from scenario uncertainty grows more rapidly than does the signal, so the signal-to-noise ratio falls.” [Ed Hawkins, United Kingdom]	Rejected. Disagree that the suggested rewording is clearer.
11-673	11	27	1			I like the read a lot, most of it is already in very good shape. One general question: Many of the figures have no references. Are they produced just for this report? For our SREX chapter we had only used published figures. Are the figures here also published or have the terms changed? If there are references, please provide them. [Boris Orlowsky, Switzerland]	Where there are new figures they use published methdologies but may be updated using new data (e.g. from CMIP5).
11-674	11	27	6			Section 11.3.2.1.1 Global mean surface air temperature: we suggest to refer here to Chapter 12, Table 12.2, which includes CMIP5-based results for mean and multi-model range for the 2016-2035 period. It seems important to discuss and explain the differences between the purely CMIP5 based and the ASK-scaled projections for the near-term. The same comment will be made to Chapter 12. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. A reference to Table 12.2 has been added. Discussion of comparison between CMIP5 and ASK was already included in the SoD, and has been updated.
11-675	11	27	12	27	17	what about for the large overestimation of the models in the year 2000-2010? any known reason for it? [Annalisa Cherchi, Italy]	The comparison between models and observations over the last two decades is discussed in a new Box (Box 9.2) in Chapter 9. This Box is now referenced.
11-676	11	27	19		28	ASK projects the greenhouse gas response forward separately from the response to other forcings, woth mentioning (also because otherwise its not clear why a ghg runw ould be needed) [Gabriele Hegerl, United Kingdom]	Rejected. Unfortunately there is not space to provide more detail, which is of course available in the references. Other comments suggested too much space was devoted to ASK.
11-677	11	27	21	27	23	ASK could be described in a bit more detail here. It would be good to say that this method scales the resopnses to GHGs and aerosols separately, based on fits of each to observed temperature evolution. [Nathan Gillett, Canada]	Rejected. Unfortunately there is not space to provide more detail, which is of course available in the references. Other comments suggested too much space was devoted to ASK.
11-678	11	27	21	27	56	Since the ASK approach is mentioned and used only here and for Fig. 11.12, I find that its description takes an unproportional amount of space. [Boris Orlowsky, Switzerland]	Rejected. The ASK results play an important role in the overall assessment presented in 11.3.6.3 so it is important that they are properly discussed.
11-679	11	27	21			How do these weightings jibe with findings like those from Pierce et al (2009, PNAS; Pierce, D. W., T. P. Barnett, B. D. Santer, and P. J. Gleckler, 2009: Selecting global climate models for regional climate change studies. Proceedings of the National Academy of Sciences, doi:10.1073/pnas.0900094106) and Santer et al (2009, PNAS, doi:10.1073/pnas.0901736106), which showed that such weighting is not as informative as one might intuit? [Government of United States of America]	Pierce et al use weighting based on climatology and variability, which is not as informative for constraining projections as past changes in temperature (as used in ASK). Santer et al focus on changes in water vapour and do not seek to determine whether models under or overestimate the observed response.
11-680	11	27	26	27	28	An additional source of uncertainty here is that ASK assumes that if a model should be scaled up by a particular factor for the historical period, then it should be scaled up by the same factor for the future. This isn't completely true, especitally for the case where the forcing increases more slowly or declines in the future. Kettleborough et al. (2007) investigate this source of uncertainty. Admittedly this is less of an issue for the near-term projections considered here than for longer term projections. Kettleborough, J. A., B. B. B. Booth, P. A. Stott, and M. R. Allen (2007), Estimates of uncertainty in predictions of global mean surface temperature, J.	Accepted. This is one example of possible non-linearities in the response to forcings, and the general issue of non-linearities is now mentioned.

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						Clim., 20, 843–855. [Nathan Gillett, Canada]	
11-681	11	27	30	27	42	The heavy emphasis on the ASK results here and in the summary seem overdone given the long list of caveats here. There is much more work to be done on the problem of connecting historical skill to the probabilities of various projection-ensemble members. [Government of United States of America]	Noted. However, the main alternative of simply relying on the raw CMIP5 results is inadequate, in that it makes no direct use of information about the consistency of models and the real world. Furthermore the ASK results are consistent with other evidence concerning the Transient Climate Response (discussed in Chapter 10). The new Box 9.2 is also relevant. We agree that there is much more work to be done here.
11-682	11	27	30	27	42	This implication is very important, suggesting overestimation of longer-term global mean surface air temperature. Needs to reflect in Chapter 12. [Akio Kitoh, Japan]	These findings have been discussed with the Chapter 12 author team. However, the implications for longer term projections are not necessarily straightforward because different uncertainties may become more important in the long term (e.g. carbon cycle feedbacks).
11-683	11	27	31	27	31	There is no "Stott, 2012" in the References. Should it be "Stott et al., 2012"? Also, the "Stott and G. Jones, 2012" should be "Stott and Jones, 2012" (delete the "G.") [Xiaolan Wang, Canada]	References have been corrected and updated. The correct reference is Stott et al, ERL, 2013
11-684	11	27	38	27	39	It might be useful to refer back to Figure 11.1a where this appears to be shown. [Fyfe John, Canada]	Fig 11.1a is included as a semi-schematic illustration and should not be used as evidence of science results.
11-685	11	27	38	27	40	Something similar is conveyed by Figure 11.1. [Ramon de Elia, Canada]	Fig 11.1a is included as a semi-schematic illustration and should not be used as evidence of science results.
11-686	11	27	40	27	40	Add Smith et al (2012) which provides consistent multi-model results, to the Meehl and Teng (2012) reference. [Christophe CASSOU, France]	Reference to both papers has now moved to 11.3.6.3
11-687	11	27	41	27	42	Could you give a few details of this statistical method? Compared to ASK this is really short. [Boris Orlowsky, Switzerland]	Accepted. A short description has been added.
11-688	11	27	42	27	42	Should not the "CMIP5 multi-model mean" be replaced by "CMIP5 multi-model median"? It is the median that is shown in Fig. 11.12(b). [Xiaolan Wang, Canada]	Accepted. Text has been amended.
11-689	11	27	44	27	44	Do you use AE or BE spelling (emphasized/emphasised)? [Boris Orlowsky, Switzerland]	The WGI style guide says that the language used in IPCC Reports is UK English. However, this sentence has now been deleted.
11-690	11	27	44	27	49	I was a bit surprised to have to read all the way down to section 11.3.6. to find some discussion of the fact that the observations are at the lower end of the model spread and the other estimates presented in fig 11.12, I would have thought that some comment on this feature should be made at this point of the chapter. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. This point was already noted on lines 36-38 (page 27 of the SoD), but note that a new Chapter 9 Box (Box 9.2) now discusses the comparison between model simulations and observations over the last 1-2 decades in detail. This new box is now referred to here.
11-691	11	28	8	28	10	Suggest replace the sentence "An estimate of the projected … (dahsed black lines from Stott, 2012" with "The dashed black lines show an estimated of the projected 5-95% range for decadal mean global mean surface air temperature for the period 2016-2040 derived using the ASK methodology applied to several CMIP5 GCMs (from Stott et al., 2012)." Note that the "Stott, 2012" should be "Stott et al., 2012", or add "Stott, 2012" in the References. [Xiaolan Wang, Canada]	Accepted. Caption has been amended.

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11-692	11	28	14	28	15	The caveats given in Box 11.2 should be mentioned here '(see Box 11.2: Temperature, for the ability of climate models to simulate observed regional trends)'. [Government of United States of America]	Accepted. A reference to Box 11.2 has been inserted.
11-693	11	28	14	28	15	How can these patterns of near-term surface warming be consistent with observations, we're not there yet? [Fyfe John, Canada]	Accepted. Text has been clarified.
11-694	11	28	14		15	The geographical pattern of near-term surface warming simulated by the CMIP5 models (Figure 11.13) is consistent with previous IPCC reports and observational trends in a number of key aspects. [Government of United States of America]	*It is not clear what is the comment here.*
11-695	11	28	17	28	21	Boer et al. (2011) demonstrate that the enhanced warming of land compared to oceans results in part from different local feedbacks, and in part from an anomalous transport of heat from the ocean regions to the land. Boer et al. (2011) also provides a good review of other mechanisms proposed in the literature. Boer, G. J. (2011). The ratio of land to ocean temperature change under global warming. Climate dynamics, 37(11), 2253-2270. [Nathan Gillett, Canada]	Accepted. Reference has been added.
11-696	11	28	17			Lambert and Chiang 2007 doesn't state that ocean heat uptake allows more rapid land warming, which could be an interpretation of what is written here. Instead, we found that ocean heat uptake retards land warming such that a constant ratio of land-sea surface temperature chnage is maintained. In other words, in most situations, the land is chained to the ocean, rather than being free to warm independently of it. [Francis Hugo Lambert, United Kingdom]	Accepted. Text has been amended.
11-697	11	28	23	28	33	It is probably worth noting here that polar amplification is much larger in the N. Hemisphere, if it isn't solely a NH phenomena. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text has been amended.
11-698	11	28	23	28	33	Make reference to the polar amplification box in Chapter 5. [Thierry Fichefet, Belgium]	Accepted. Reference to Chapter 5 box has been added.
11-699	11	28	24	28	25	Do CMIP5 models now represent well the polar amplification? [Akio Kitoh, Japan]	This is an issue for Chapters 9 and 10, but the literature may not yet be available to make this assessment.
11-700	11	28	30	28	30	"and increases in cloud cover and water vapour". Some references should be cited to support this mechanism, just as what has been done for the other amplification mechanisms. [Gan Zhang, United States]	Accepted. Two references have been added.
11-701	11	28	42	28	42	remove the) from change) [Boris Orlowsky, Switzerland]	Accepted. Text has been amended.
11-702	11	28	57	28	59	The spatial resolution description is inconsistent with what is described in the caption for Figure 11.14. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text has been amended.
11-703	11	28	58	28	59	The figure caption says something about 2.5 deg, not 5 deg as in the text, and the use of control simulations to derive the noise. Could you make the text more consistent with the caption? [Boris Orlowsky, Switzerland]	Accepted. Text has been amended.
11-704	11	28	58		59	5° latitude x5° longitude' [Government of United States of America]	Accepted. Text has been amended.
11-705	11	28	58		59	Figure caption says '2.5° latitude x 2.5° longitude', and it indeed looks like 2.5°x2.5°. [Government of United States of America]	Accepted. Text has been amended.
11-706	11	29	1	29	3	The sentence in parentheses "(Note that choosing…..leading to later ToE) is important, as it demonstrates that the conclusions with respect to emergence time are sensitive to threshold assumptions, some of which are (seemingly?) arbitrary. Suggest (a) adding some of the details (e.g., specific numbers) of this sensitivity analysis to the figure (11.14) and (b) rather than just stating "higher signal to noise threshold results in later ToE", provide some more details on how much higher, how much later, etc. Alternatively, consider providing more detailed rationale for why the particular assumptions used in the analysis that you chose to present are the appropriate ones. [Government of Canada]	There is not space to include a detailed discussion of sensitivity but the text has been amended to emphasize that the specific threshold of interest is highly dependent on the user, and thus the greatest value of Fig. 11.14 is in the information it provides about the regional variations and model related uncertainties in estimates of ToE.
11-707	11	29	1	29	18	It would be nice to reconcile the ToE analysis with the hatching shown on the spatial maps as they are related. [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	They are related but the hatching provides discrete information (hatched or not hatched) whereas ToE provides a continuous measure.

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11-708	11	29	1		17	I think the analysis is too general. If possible, bring them to the level of climatic regions (equatorial, tropical, temperate and polar) for better explained. [Ibouraïma YABI, Benin]	Unfortunately there is not space to provide this level of regional detail in Chapter 11. Further information about regional changes is provided in Chapter 14 and will be provided in the Working Group II report.
11-709	11	29	2	29	2	Do you mean "resolution" by "smaller spatial scales"? The statement is redundant with the sentence starting on Line 7. [Boris Orlowsky, Switzerland]	Accepted. Text has been amended.
11-710	11	29	11	29	11	I'd prefer Region 2 as an example, which shows a clearer signal, although I generally find this comparison difficult (at least in my printout). [Boris Orlowsky, Switzerland]	Accepted. Text has been amended.
11-711	11	29	16	29	17	I think for most extremes the signal to noise ratio is lower than for changes in the mean climate. What cases are referred to here? Precipitation extremes? I think 'many cases' is overstating this - I would say 'some cases' at most. [Nathan Gillett, Canada]	Accepted. Text has been amended.
11-712	11	29	16	29	17	"become apparent sooner", this surprises me, perhaps an example/reference would be useful. [Fyfe John, Canada]	This is consistent with Malstein et al 2011 and Hawkins and Sutton 2012. However, this sentence has now been rephrased.
11-713	11	29	21	29	22	Is this statement about it being very likely that future Arctic warming is greater than global mean warming for the next few decades true even considering that the Arctic has warmed strongly in recent decades? I think this statement is made based on model projections, but it should also account for the warming that has already been observed. For example if the anomaly observed in the Arctic over the base period were already as large as the predicted anomaly for 2016-2035, then this would presumably not be true. I'm not saying that the statement is necessarily wrong, but it does need to account for observed temperature changes. [Nathan Gillett, Canada]	Noted. It is a robust result from climate models that Arctic warming is greater than global mean warming over periods of several decades. Sentence has been modified to clarify the point.
11-714	11	29	24	29	24	boreal summer? [Boris Orlowsky, Switzerland]	This sentence has now been rephrased.
11-715	11	29	25	29	25	Change "in these" to "in this season and in these" [Fyfe John, Canada]	This sentence has now been rephrased.
11-716	11	29	35			Hawkins & Sutton 2011 should be Hawkins & Sutton 2012 – which is not in the reference list [Ed Hawkins, United Kingdom]	Accepted. Reference corrected.
11-717	11	29	37			Section 11.3.2.2. This section also needs to consider the consistency of simulated changes with observations, and should in particular consider section 9.4.1.3.2. [Nathan Gillett, Canada]	The consistency of simulated and observed temperature changes in the free atmosphere is discussed in chapter 10 (detection and attribution)
11-718	11	29	40			Should 'IPCC AR4' be 'CMIP3 models'? [Nathan Gillett, Canada]	Yes, text has been amended.
11-719	11	29	43	29	43	This phrasing is not quite right. From what I see in Figure 11.15 I'd say something like "extends nearly into the tropopause in the high southern latitudes in JJA and high northern latitudes in DJF". In the figure I'd be tempted to plot the climatological tropopause. [Fyfe John, Canada]	The figure and the sentence addressed by the comment has been dropped in order to shorten the chapter. More detailed discussions of temperature changes in the free atmosphere are provided in chapters 10 and 12.
11-720	11	29	43	29	45	The two hemispheres responses seem rather symmetric in the stratosphere. Startospheric cooling in the high southern latitudes in JJA is not significant. Insignificant stratospheric cooling appears also in the high northern latitudes in DJF or the annual mean. [Akio Kitoh, Japan]	The figure and the sentence addressed by the comment has been dropped in order to shorten the chapter. More detailed discussions of temperature changes in the free atmosphere are provided in chapters 10 and 12.
11-721	11	29	44			Recommend "not evident" --> "not as evident" [Government of United States of America]	The figure and the sentence addressed by the comment has been dropped in order to shorten the chapter. More detailed discussions of temperature changes in the free atmosphere are provided in chapters 10 and 12.
11-722	11	29	53	29	57	Could the SREX also be cited at this point? [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised

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11-723	11	29	53	29	57	Would it be appropriate to also cite the SREX here, since precipitation extremes are explicitly mentioned? [European Union]	Accepted - text revised
11-724	11	29	56	29	56	"O'Gorman" is misspelled throughout the chapter. [J. Graham Cogley, Canada]	Accepted - text revised
11-725	11	30	2	30	4	The 7%/K increase only follows from Clausius-Clapeyron under the assumption of constant relative humidity. This is really an assumption, rather than something with a clear theoretical explanation. The cited refs here are reasonable, although the assumption of constant relative humidity under climate change was first put forward by Arrhenius in 1896. Insert 'and the assumption of constant relative humidity' after 'Clausius-Clapeyron equation'. [Nathan Gillett, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-726	11	30	4	30	4	A fine point here, but this is not a consequence of the CC equation, it's a consequence of the physics that the CC equation represents, i.e. that saturation water vapor pressure changes approximately exponentially with temperature. [Fyfe John, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-727	11	30	4	30	5	Should there be some discussion here of observed changes in mean precipitation? [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6. Observed changes in mean precipitation are presented in Chapter 2.
11-728	11	30	6			Allen and Ingram (2002) should be cited again here. I think they were the first to put forward the idea of the precip being controlled by the tropospheric energy budget. [Nathan Gillett, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-729	11	30	8	30	8	Comma needed after "absorption". [J. Graham Cogley, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-730	11	30	10			Allen and Ingram (2002) should be cited again here - they pointed out that precip should be more sensitive to shortwave forcings than to greenhouse gas changes. [Nathan Gillett, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-731	11	30	11	30	12	I don't quite understand the last part of this sentence regarding the role of "longwave radiation". Does this refer to the absorption and emission of outgoing longwave radiation by aerosols, or what.? On this point, I bring to your attention this paper that is in-review in GRL that deals with solar and aerosol impacts on precipitation: Fyfe et al. (2012), Biogeochemical carbon coupling influences global precipitation in geoengineering experiments. [Fyfe John, Canada]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-732	11	30	12	30	12	the reference "Alessandri et al(2012)" could be included. It shows the role of sulphate aerosols in the strenghtening of the hydrological cycle, even considering a mitigation scenario. It also compute water and energy budgets for land and ocean. The details of the reference are: "Alessandri A, Fogli PG, Vichi M, Zeng N (2012) Strengthening of the hydrological cycle in future scenarios: atmospheric energy and water balance perspective. Earth Syst Dynam 3 199-212" [Annalisa Cherchi, Italy]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6.
11-733	11	30	13			Maybe worth also pointing back to Ch10, which discusses attributable changes in the watercycle. Overall, I like the watercycle discussion also the one in the paragraph starting in l 28 is very clear [Gabriele Hegerl, United Kingdom]	Taken into account - this paragraph was removed. Processess underlying precipitation changes will be discussed in Chapter 7, section 7.6. Observed changes in mean precipitation are presented in Chapter 2.
11-734	11	30	16	30	16	What do you mean by largest? Global, zonal, continental? [Boris Orlowsky, Switzerland]	We mean zonal means. Text revised
11-735	11	30	17	30	18	Rewording needed. There is no climatological maximum in precipitation in high latitudes. [Jouni Räisänen, Finland]	Accepted - text revised
11-736	11	30	20	30	26	Information on agreement on little change is important. How is this issue treated in AR5? [Akio Kitoh, Japan]	This isse is addressed in Section 11.2.3. and is also mentioned in Ch 12.

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11-737	11	30	21	30	21	"small in some sense, relative to internal variability, then …". [J. Graham Cogley, Canada]	Current wording is ok
11-738	11	30	24	30	26	This is interesting, but is it possible to express how much more widespread is the consensus, e.g. is the spread 10% smaller, 50% smaller, or what? [Fyfe John, Canada]	Estrimates are provided by Power et al. Values are not quoted here because they deopend on the criteria used to define "small".
11-739	11	30	28	30	39	I wonder if there is quatitative measure of the validity of the wet-get-wetter pattern? [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account - Text revised. Processess underlying precipitation changes is discussed in Chapter 7, section 7.6
11-740	11	30	29	30	29	Italicize P and E throughout the text where they are algebraic symbols, and replace the hyphen in "precipitation- evaporation" with an en-dash. [J. Graham Cogley, Canada]	Editorial - copyedit to be completed prior to publication
11-741	11	30	30	30	31	I find that there substantial deviations from this pattern, some dry regions of today are projected to become wetter. E.g. over East Africa, West Asia, NE Brazil in boreal winter, see Fig. 11.16. Could you differentiate the statement? [Boris Orlowsky, Switzerland]	Accepted - text revised
11-742	11	30	52	30	53	In which direction does the guide work? How is it projected to change and which changes in precipitation follow? [Boris Orlowsky, Switzerland]	Muller and O'Gorman (2011) show that "Within the energetic framework, it is dry static energy transport by the mean circulation that plays a key role in determining the pattern of precipitation change. Shortwave and longwave radiative contributions are also important, but tend to partially offset each other. Cloud and water vapour radiative feedbacks locally dampen the precipitation response over ocean, such that changes in the diabatic cooling are only a guide to the precipitation response for sufficiently large length scales". The sentence was rewritten but the discussion on processes underlying precipitation changes is in Chapter 7. The reference to this Chapter is included in the revised text of the section.
11-743	11	30		32		The first paragraph in p30 through the Figure 11.16 and 11.17 in p32 should form a section as 'Changes in precipitation' for readability and more consistent structure. Accordingly, the section 11.3.2.3.1. 'Changes in evaporation, runo-off, soil moisture, and specific humidity' will be 11.3.2.3.2. [Government of United States of America]	Accepted - text revised
11-744	11	30		32		The first paragraph in p30 through the Figure 11.16 and 11.17 in p32 should form a section as 'Changes in precipitation' for readability and more consistent structure. Accordingly, the section 11.3.2.3.1. 'Changes in evaporation, runo-off, soil moisture, and specific humidity' will be 11.3.2.3.2. [Government of United States of America]	Accepted - text revised
11-745	11	30				About Projected changes in precipitation. I think that it would be important here to insert a statement about the skill (and deficiencies) of the climate models in simulating the present climate precipitation patterns. It could be a reference to another chapter in this report. [Susanna Corti, Italy]	Accepted - text revised
11-746	11	31	2			A 'result of the AR4' sounds like new science in the AR4 - of course this was known in the literature before, and this literature was assessed in the AR4. Rephrase to make this clear. [Nathan Gillett, Canada]	Accepted - text revised
11-747	11	31	6			I think there is also a Mahlstein paper out quantifying emergence for precipitation [Gabriele Hegerl, United Kingdom]	Accepted - text revised. The reference Mahlstein I., R. W. Portmann, J. S. Daniel, S. Solomon and R. Knutti (2012). Perceptible changes in regional precipitation

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							in a future climate. Geophysical Research Letters, 39, L05701 will be added
11-748	11	31	7	31	7	Replace "O(1000 km)" with "of the order of 1000 km", to improve accessibiity for non-mathematical readers. [J. Graham Cogley, Canada]	Agreed. Change made.
11-749	11	31	11			Are uncertainties associated with land-use change an issue? [Clare Goodess, United Kingdom]	Accepted - text revised
11-750	11	31	12	31	12	"for projected changes in seasonal mean precipitation". [J. Graham Cogley, Canada]	Accepted - text revised
11-751	11	31	15	31	19	This tends to repeat P30 L20-26. [J. Graham Cogley, Canada]	Accepted - text revised
11-752	11	31	15	31	19	Redundant with P30 lines 20 ff. [Boris Orlowsky, Switzerland]	Accepted - text revised
11-753	11	31	27	31	27	Change "has the potential to exert" to "may have". I do not think you can "exert" an impact. [J. Graham Cogley, Canada]	Accepted - text revised
11-754	11	31	28	31	28	"on global mean precipitation, see above)." is changed to "...on global mean precipitation, see above)(Yue et al.,2011; Bollasina et al., 2011)." Yue X., H. Liao, H. Wang, S. Li, and J. Tang, Role of sea surface temperature responses in simulation of the climatic effect of mineral dust aerosol, Atmos. Chem. phys., 11, 6049-6062, doi:10.5194/acp-11-6049-2011, 2011. Bollasina, M. A., Y. Ming, V. Ramaswamy, Anthropogenic Aerosols and the Weakening of the South Asian Summer Monsoon, Science, 334(6055): 502-505, 2011. [Jianqi Sun, China]	Taken into account - text revised: both references will be added
11-755	11	31	34	31	34	The map (Fig. 11-16, page 11-107) of precipitation changes given in percent is not very meaningful, with the only regions standing out being those (Sahara, Arabia) where even the 50% changes are 50% of practically zero. It would be better to give the absolute values where the 10-20% internal variability (parts of South America, southern Africa) may make a difference for vegetation, and also for the changes, left-hand panel, to improve contrast and reduce the masking effect of the stippling. Or else add panels of absolute v alues. Similarly for Fig. 11-18, page 11-109. [Robert Kandel, France]	Rejected -units will be keep in % but it will redrawn with 3-monts season (DJF, MAM, JJA, SON). Figure 11.18 will be replaced by a new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%).
11-756	11	31	34	31	35	See comment on P30 lines 30-31. Please allow for some regional deviations from this pattern. [Boris Orlowsky, Switzerland]	Accepted - text revised
11-757	11	31	39			This important statement demands a reference (more than one, ideally). Please add references. [Government of United States of America]	Accepted - text revised
11-758	11	31	46	31	46	"The Sahara and Arabia exhibit …". [J. Graham Cogley, Canada]	'the' inserted
11-759	11	31	49	31	49	"almost everywhere less": this does not describe Figure 11.17 accurately. The median changes are outside the grey envelope polewards of 45S and 40N, and are outside or 'borderline' in several zones between those latitudes. [J. Graham Cogley, Canada]	Taken into account - text revised. SOD Fig. 11.17 (new Fig. 11.15) was replaced by a new one with two pannels (P and E-P) and box plots will be removed for clarity
11-760	11	31	50	31	50	suggest replace "internal variability" with "internal variability (see hatchings in Figure 11.16)" [Xiaolan Wang, Canada]	Accepted - text revised
11-761	11	31	54			Should 'likely' be in italics? [Clare Goodess, United Kingdom]	Editorial - the sentence/assessment changed
11-762	11	31	56	31	56	This paper is relevant here: Fyfe at al. (2012), Human influence on extratropical Southern Hemisphere summer precipitation, GRL. It shows the impact of GHG, aerosols and ozone on precipitation in CMIP5 models. By the way, circulation changes and aerosol forcings do not belong on the same level, the latter to some extent	Accepted - text revised. The reference Fyfe, J.; Gillet, N. and Marshall J., 2012: Human influence on extratropical Southern Hemisphere summer precipitation, GRL will be added

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						forces the former. [Fyfe John, Canada]	
11-763	11	32	10	32	10	The unit in Figure 11.17 seems to be per cent, not mm/day. [Jouni Räisänen, Finland]	Accepted - text revised
11-764	11	32	16	32	17	repetition from page 29, l53ff -- suggest to not repeat the references to AR4 and the IPCC Technical Paper again here [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised
11-765	11	32	17	32	17	"required to balance": I doubt the wisdom of this generalization. The muted response mentioned at L20 violates it, and in any case it does not account for recharge of depleted aquifers and possible sequestration of snow on the Antarctic Ice Sheet. It is true that both evaporation and precipitation are expected to increase, but that idea could be phrased more accurately. [J. Graham Cogley, Canada]	Accepted - text revised
11-766	11	32	18		22	Precisely, the mistake is deriving the formulation of evaporation in climate models from a bulk aerodynamic approximation which involves variables like the local wind speed and the local low-altitude specific humidity gradient which cannot easily and convingly be linked to the gridscale mean variables carried in the computation. This is where the choice of parameterized formulation erroneously introduces a spurious dependence upon mid-level humidity and Clausies-Clapeyron. As demonstrated by numerous hydrological field studies, this "model" approach to "predicting" evaporation or evapotranspiration ignores the spatial variability of all hydrological variables (even over the relatively smooth ocean). As stated in the Assessment Report (e. g. Chap.7,page 5, lines 30-32 and sections 7.6.2, 7.6.3), an effective approach to estimating evaporation at the surface of the Earth surface must take into account the energy energy budget constraint on the process i. e. (H = LE) . Over continents, this method determines hourly water fluxes, irrespective of near-surface specific humidity gradients which adjust from one minute to the next to solar irradiance fluctuation. Over the oceans, the energy budget constraint is extended over time but is quite real nevertheless. Consider for example the 1-2 weeks time interval needed to replenish the heat content off the upper ocean after the passage of a hurricane. [Government of France]	Noted. No further response required.
11-767	11	32	19			Is this result based on assuming constant relative humidity? [Government of United States of America]	Accepted - Yes, details on processess underlying precipitation changes are discussed in Chapter 7, section 7.6.
11-768	11	32	21	32	21	Uhm, I don't really understand what is being said here, and I'm particularily confused by the statement that the CMIP3 models show a systematic decrease in wind stress. I would of the thought the opposite to be true. [Fyfe John, Canada]	Taken into account - references will be included about this statement
11-769	11	32	22	32	29	The paper I mentioned above is especially relevant here: Fyfe et al. (2012), Biogeochemical carbon coupling influences global precipitation in geoengineering experiments, GRL. [Fyfe John, Canada]	Taken into account - reference will be added
11-770	11	32	24	32	24	Subscript 2. [J. Graham Cogley, Canada]	Corrected
11-771	11	32	24	32	24	typo CO2, use subscript [Boris Orlowsky, Switzerland]	Corrected
11-772	11	32	24	32	24	"... effects of CO2 may involve ..." The number "2" should appear in the subscript form. [Gan Zhang, United States]	Corrected
11-773	11	32	25	32	25	transpiration not evapotranspiration [European Union]	Accepted - text revised
11-774	11	32	26	32	26	in this case the term evapotranspiration is correct [European Union]	Noted
11-775	11	32	26	32	26	Please also see: Bounoua, L., and Co-authors, 2010: Quantifying the Negative Feedback of Vegetation to Greenhouse Warming. A Modeling Approach, Geophys. Res. Lett, L23701, doi:10.1029/2010GL045338. [Government of United States of America]	Noted
11-776	11	32	27	32	27	"a vegetation model that allows". [J. Graham Cogley, Canada]	Corrected
11-777	11	32	27	32	27	typo: inclusion of a vegetation modelmodels that [European Union]	Corrected
11-778	11	32	27	32	27	typo modelmodels [Boris Orlowsky, Switzerland]	Corrected
11-779	11	32	27	32	27	Correct typo “modelsmodels” [Aneesh Subramanian, India]	Corrected

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11-780	11	32	27	32	27	Check the phrase "the inclusion of a vegetation modelmodels". Maybe it meant "the inclusion of vegetation models"? [Xiaolan Wang, Canada]	Corrected
11-781	11	32	27	32	27	"... vegetation modelmodels that ..." "modelmodels" is likely to be a typo. [Gan Zhang, United States]	Corrected
11-782	11	32	31	31	45	This is all rather a general discussion of soil moisture changes. Is it possible to be more specific about changes/issues associated with the near-term timescale? [Clare Goodess, United Kingdom]	Taken into account - text revised
11-783	11	32	34	32	34	Suggest to include the following references: Koster et al., 2004, Science: Regions of Strong Coupling Between Soil Moisture and Precipitation; Hirschi et al., 2011, Nature Geosci.: Observational evidence for soil-moisture impact on hot extremes in southeastern Europe; Mueller and Seneviratne, 2012, PNAS: Hot days induced by precipitation deficits at the global scale. [Boris Orlowsky, Switzerland]	Accepted - text revised
11-784	11	32	36	32	36	typo "limitations" [Annalisa Cherchi, Italy]	Corrected
11-785	11	32	36	32	36	"limitations". "effects on". [J. Graham Cogley, Canada]	Corrected
11-786	11	32	36	32	36	typo: limitations [European Union]	Corrected
11-787	11	32	36	32	36	typo limiations [Boris Orlowsky, Switzerland]	Corrected
11-788	11	32	38			Should include the classic reference Koster, R.D. and Coauthors, 2004: Regions of strong coupling between soil moisture and precipitation. Science, 305, 1138–1140. [Government of United States of America]	Accepted - reference included
11-789	11	32	39	32	39	Replace "Meehl et al. (2007b)" with "(Meehl et al., 2007b)". [Xiaolan Wang, Canada]	Corrected
11-790	11	32	41	32	42	Replace "mostly not consistent or statistically significant" with "mostly not consistent nor statistically significant" or "mostly not consistent or statistically insignificant" [Xiaolan Wang, Canada]	Corrected
11-791	11	32	41	32	45	Wording here needs improvement. [Fyfe John, Canada]	Done.
11-792	11	32	42	32	42	Delete "Future". [J. Graham Cogley, Canada]	Accepted - text revised
11-793	11	32	44	32	45	Change "high temperature values" to "extreme temperatures". "means". [J. Graham Cogley, Canada]	Accepted - text revised
11-794	11	32	47	32	47	Delete "from". [J. Graham Cogley, Canada]	Corrected.
11-795	11	32	47	32	47	rephrase; Changes in runoff from are coupled to changes in precipitation [European Union]	Accepted - text revised
11-796	11	32	47	32	47	remove "from" [Boris Orlowsky, Switzerland]	Corrected.
11-797	11	32	47	32	47	Replace "Change in runoff from" with "Changes in runoff form". [Xiaolan Wang, Canada]	Corrected.
11-798	11	32	47		56	To be specified: the extent to which change in human activity (damming, land use change…) is included in runoff change projections [European Union]	taken into account - it will be considered the possibility of specifying this issue
11-799	11	32	49	32	49	typo: reductionannual mean reductions in southern Europe and increases [European Union]	Corrected.
11-800	11	32	49	32	49	typo reductionannual [Boris Orlowsky, Switzerland]	Corrected.
11-801	11	32	49	32	49	Fix typo “reductionannual” [Aneesh Subramanian, India]	Corrected.
11-802	11	32	49	32	49	Replace "reductionannual" with "annual". [Xiaolan Wang, Canada]	Corrected.
11-803	11	32	49	32	49	"... indicating reductionannual mean reductions ..." Probably there is typo and the meaning is also unclear. [Gan Zhang, United States]	Accepted - text revised
11-804	11	32	49	32	50	Delete "reduction" and "effect". [J. Graham Cogley, Canada]	Accepted - text revised
11-805	11	32	49			… reductionannual (typo) [Government of France]	Accepted - text revised
11-806	11	32	50	32	50	typo: effecteffects [European Union]	Corrected.
11-807	11	32	50	32	50	typo effecteffects [Boris Orlowsky, Switzerland]	Corrected.

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11-808	11	32	50	32	50	"... physiological effecteffects ..." This is probably another typo. [Gan Zhang, United States]	Corrected.
11-809	11	32	51	32	52	For when are these specific changes applicable? [Clare Goodess, United Kingdom]	Corrected.
11-810	11	32	52	32	52	11 plus/minus 6. Is this the 95% range or what? [Fyfe John, Canada]	Corrected.
11-811	11	32		33		This particular subsection has a fair number of spelling and grammatical errors, [Fyfe John, Canada]	Corrected.
11-812	11	33	1	33	1	Do not hyphenate "runoff" (see also L24). [J. Graham Cogley, Canada]	Corrected.
11-813	11	33	1	33	20	the presentation of changes in specific humidity are useful, but changes in relative humidity are only briefly mentioned and not shown. I think it would be worthwhile to have a figure shoing both relative humidity and vapor pressure deficit projections. or if this is too much, i would at least put VPD in. the reason is that many impacts on plants and animals are directly related to changes in VPD, much more than say changes in specific humidity [David Lobell, United States of America]	Corrected.
11-814	11	33	3	33	3	typo: internal variability in these quantitiesChanges [European Union]	Corrected.
11-815	11	33	3	33	3	typo quantitiesChanges [Boris Orlowsky, Switzerland]	Corrected.
11-816	11	33	3	33	3	fix typo “quantitiesChanges” [Aneesh Subramanian, India]	Corrected.
11-817	11	33	3	33	3	Replace "quatitiesChanges" wih "quatities. Changes" [Xiaolan Wang, Canada]	Corrected.
11-818	11	33	3	33	3	"... quantitiesChanges ..." should be "... quantities. Changes ..." [Gan Zhang, United States]	Corrected.
11-819	11	33	3			… quantities.Change (typo) [Government of France]	Corrected.
11-820	11	33	4	33	4	to me, NW Africa shows increases as well in Fig. 11.18 and I don't see the largest changes systematically over N high latitudes. [Boris Orlowsky, Switzerland]	Accepted - text revised
11-821	11	33	4	33	4	"except in north-western Africa"? North-western Africa also show positive changes in Figure 11.18. Maybe it meant to say except in the Middle East (there is very small areas of small negative changes)? [Xiaolan Wang, Canada]	Accepted - text revised
11-822	11	33	4			changes in evaporation over land are mostly positive, except in north-western Africa' [Government of United States of America]	Accepted - text revised
11-823	11	33	4			It looks positive over north-western Africa as well in the figure 11.18 top left panel. [Government of United States of America]	Taken into account - Figure redrawn
11-824	11	33	6	33	7	I find it very hard to see any stippling on the evaporation panel. [Clare Goodess, United Kingdom]	Figure 11.18 was be replaced by a new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%).(New Fig.11.16)The number of CMIP5 models used is indicated in the upper-right corner of each panel
11-825	11	33	6			Projected changes are larger than the estimated standard deviation of internal variability only at high latitudes and over the tropical oceans. In the figure 11.18 top left panel (evaporation), the tropical oceans are not stippled. Is this because only < 90% of the models agree on the sign, although the multi-model mean projections differ significantly (as stated in the text) from the control (according to the definition for the stippling in Figure 11.13 caption)? [Government of United States of America]	Accepted - text revised
11-826	11	33	6			The sentence beginning "Projected changes" only appears to apply to specific humidity; this needs to be made clearer. [Government of United States of America]	Taken into account - maritime continent will be defined clearly in the text

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11-827	11	33	9	33	9	The maritime continent is not in northern mid-to-high latiudes, and in any case many readers will not catch the geographical allusion. [J. Graham Cogley, Canada]	Taken into account - maritime continent will be defined clearly in the text
11-828	11	33	9	33	9	What do you mean by Maritime continent? [Boris Orlowsky, Switzerland]	Taken into account - It´s the region of Southeast Asia which comprises many islands, peninsulas and shallow seas (10S–20N and 908–150E). See for example R. Neale and J. Slingo, 2003: The Maritime Continent and Its Role in the Global Climate: A GCM Study. , J.Climate 16, 834-848.
11-829	11	33	9	33	9	Reverse order ("in other regions of northern mid-to-high latitudes and the Maritime continent") to avoid claiming that the Maritime Continent is in the mid-to-high latitudes. [Jouni Räisänen, Finland]	Accepted - text revised
11-830	11	33	12	33	13	As written this statement is not true. Projected changes in specific humidity are larger in the tropics and smaller near the poles (see e.g. Willett et al., 2007, Figure 2d). It is only when expressed in percentage terms (as in Figure 11.18) that the projected changes are larger at the high latitudes. The saturation vapour pressure varies approximately exponentially with temperature according to the C-C equation, so for a given temperature change the change in saturation vapour pressure (and hence specific humidity, if RH remains constant) will be much larger in the warm tropics than at the cold high latitudes. If expressed in percentage change, then again assuming constant RH, it is true that the change will be larger where the temperature change is larger, since deviations from exponential behaviour are small. But this doesn't come across in the text as written. Suggested re-phrasing 'and when expressed in percentage terms are largest at northern high latitudes, as expected based on the larger warming projected here, the Clausius-Clapeyron equation, and an assumption of constant relative humidity.' Willett, K. M., Gillett, N. P., Jones, P. D., & Thorne, P. W. (2007). Attribution of observed surface humidity changes to human influence. Nature, 449(7163), 710-712. [Nathan Gillett, Canada]	Accepted - text revised
11-831	11	33	12	33	20	Here, in Figure 11.18, relative humidity figure, rather than specific humidity figure, is more illustrative to show, because we can easily imagine specific humidity changes from surface temperature changes. Please consider exchange a specific humidity map with a relative humidity map in Figure 11.18. [Akio Kitoh, Japan]	Taken into account - This figure was replaced by a new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%). The number of CMIP5 models used is indicated in the upper-right corner of each panel. New Fig.11.16
11-832	11	33	12	33	20	A cross-reference to section 2.5.5 could be given here, though see comment 109. [Adrian Simmons, United Kingdom]	The paragraph was rewritten. It doesn't seem that comment 109 refers to the Water Cycle section. Section 2.5.5 does not exist in the SOD
11-833	11	33	12			Annual humidity is not a particularly useful metric of change. Please include some discussion of seasonalities of these changes. [Government of United States of America]	Taken into account - text revised
11-834	11	33	23			One wonders what the cause(s) of such "projected" evaporation increase may be. What is the evidence for the "projected" changes in cloudiness, aerosol loading or downward LW radiation that will increase the net surface budget and allow faster evaporation (see comment 11.4)? [Government of France]	Evaporation increase over land in the northern high latitudes is consistent with the increase in precipitation and the overall warming that would increase potential evaporation.
11-835	11	33	30	33	31	Control conditions: what period?; typo Figure11.13 [Boris Orlowsky, Switzerland]	Accepted - Figure caption was changed according to the new version of this figure: CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f)

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							absolute change in relative humidity (%). The number of CMIP5 models used wil be indicated in the upper-right corner of each panel. (New Fig.11.16)
11-836	11	33	35	33	57	Here as well it would be important to have a reference to the chapter and section in this report where the actual ability of climate models in reproducing the 20th century atmospheric circulation is assessed. The ability of climate models of reproducing the main features of the atmospheric circulation is very much relevant for evaluating the "reliability" of future projections. [Susanna Corti, Italy]	Refernce to biases in circulation included.
11-837	11	33	35	34	11	There are two new CMIP5 papers that are quite relevant here. 1. Gillett and Fyfe (2012), Attribution of observed sea level pressure trends to greenhouse gas, aerosol and ozone changes, Nature Climate Change (in review), and 2. Gillett and Fyfe (2012), Annular mode changes in the CMIP5 simulations, GRL (in review). Both are discussed quite extensively in Chapters 12 and 14. The first paper is also relevant to the 11.3.2.4.3 subsection. [Fyfe John, Canada]	Manuscripts are cited.
11-838	11	33	36	33	38	Section 11.3.2.4.1: the subsection starts with a sentence comparing the near-term to the long-term projection before saying what the changes in the near-term are. In addition, the comparison would better be placed in Chapter 12, where the long-term assessment is being made. We suggest to delete the comparative part and to focus on the near term changes themselves. [Thomas Stocker/ WGI TSU, Switzerland]	The section has been restructured to focus on the near term response and reasons for limited confidence in this.
11-839	11	33	48			But it's not clear that the models should be expected to capture observed multi-decadal variations in the NAO - these may be mainly internal variability. See e.g. section 10.3.3.2. [Nathan Gillett, Canada]	OK, this sentence has been removed.
11-840	11	33	54			Which models is this based on - are these models generally realistic in their variability? [Gabriele Hegerl, United Kingdom]	There is no obvious reason to suspect that the variability in these models is particularly ill represented; CCSM is one of the models.
11-841	11	33	55			Replace 'natural variability' with 'internal variability'. [Nathan Gillett, Canada]	Accepted.
11-842	11	34	4			We couldn't find discussion of "significant changes in solar forcing over the next few decades" in section 11.2.2.2. Perhaps 11.3.6.2.2 was meant. Please verify the reference and correct, if necessary. [Government of United States of America]	Yes, 11.3.6.2.2 seems right.
11-843	11	34	7			Does the Haarsma study look specifically at this near-term period? [Clare Goodess, United Kingdom]	Paragraph is removed.
11-844	11	34	14	34	29	Again, see Gillett and Fyfe (2012), Annular mode changes in the CMIP5 simulations. Also very relevant here is Fyfe and Swart (2012), Observed and simulated changes in the Southern Hemisphere surface westerly wind-stress. GRL. [Fyfe John, Canada]	References included.
11-845	11	34	14			Section 11.3.2.4.2. Before discussing the role of ozone recovery, this section should start by discussing the expected influence of GHG increases on future SAM trends. The annual mean changes shown in Figure 11.19 look like the positive phase of the SAM, and in the annual mean the CMIP5 models robustly simulate an ongoing increase in the SAM. Without highlighting this fact, the current discussion is focused exclusively on DJF, with no discussion of projected SAM changes in the other seasons. I suggest first describing the annual mean response, as reflected, for example, in the wind trends shown in Fig 11.19, which are projected to be a shift towards the positive phase of the SAM. Then go on to point out that in DJF the trend in coming decades is likely to be small owing to the competing effects of ozone recovery and GHG increases. [Nathan Gillett, Canada]	The role of GHGs leading to a poleward shift in the SH extra-tropical circulation in the annual-mean leads the section, with a reference to Chapters 10 & 12 and Figure 11.19.
11-846	11	34	17	34	17	Follow "driven by increasing greenhouse gases.", add "including the related dynamical processes". Li et al. (2010) suggest that the inclusion of dynamical responses can explain the observed SAM changed more resonably. Please refer to: Li, S., J. Perlwitz, M. P. Hoerling, and X. Chen, 2010: Opposite annular responses of Northern and Southern Hemisphere to Indian Ocean warming. J. Climate, 23(13),3720-3738. [Jianqi Sun, China]	The details of the mechanisms behind observed SAM changes belongs in Chapters 9 and 10.
11-847	11	34	32	34	52	As for the previous section, the discussion here should start by summarising the mean projected changes in Hadley Cell width in thea annual mean, as shown in Fig 11.20. This shows that on average the CMIP5 models project a poleward shift in both hemispheres in the near future. In particular this shows that all the CMIP5	The role of GHGs leading to a poleward shift in the Hadley circulation in the annual-mean leads the section, with a reference to Chapters 10 & 12 and

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						models simulate a poleward shift in the southern boundardy of the Hadley Cell between 2016-2035 and 1986-2005.Then go on to discuss the role of ozone recovery in counteracting some of the influence of GHG increases. [Nathan Gillett, Canada]	Figure 11.20.
11-848	11	34	38	34	41	"...The poleward expansion of the Hadley Circulation driven by the response of the atmosphere to increasing GHGs (Butler et al., 2012; Chen and Held, 2007; Kang and Polvani, 2011; Korty and Schneider, 2008; Lorenz and DeWeaver, 2007; Lu et al., 2007)...". One new CMIP5 study by Hu et al. (2012) should be included here. [Reference: Hu, Y., L. Tao, and J. Liu, 2012: Poleward expansion of the Hadley circulation in CMIP5 simulations. Adv. Atmos. Sci., in press.] [Dabang Jiang, China]	Reference added
11-849	11	34	38	34	41	"...The poleward expansion of the Hadley Circulation driven by the response of the atmosphere to increasing GHGs (Butler et al., 2012; Chen and Held, 2007; Kang and Polvani, 2011; Korty and Schneider, 2008; Lorenz and DeWeaver, 2007; Lu et al., 2007)...". The new CMIP5 study should be added here. Hu, Y., L. Tao, and J. Liu, 2012: Poleward expansion of the Hadley circulation in CMIP5 simulations. Adv. Atmos. Sci., accepted. [Jianqi Sun, China]	Reference added
11-850	11	34	41	34	42	I think this effect is mainly restricted to the Southern Hemisphere in DJF. [Nathan Gillett, Canada]	Seasonal dependence now explicit.
11-851	11	34	48	34	50	The text here implies that an influence of ozone depletion on the location of the northern latitude boundary of the Hadley Cell has been demonstrated, but I don't think this is the case. Min and Son (2012) did not find an effect of ozone depletion on the northern boundary of the Hadley Cell over the historical period. Min, S.K. and Son, S.-W., Multi-model attribution of the Southern Hemisphere Hadley Cell widening: CMIP3 and CMIP5 models, J. Geophys. Res., submitted. [Nathan Gillett, Canada]	The discussion now focuses on the southern edge of the Hadley cell.
11-852	11	34				Generally, I miss a discussion or at least forward reference (to ch14?) of changes in NAM and SAM [Gabriele Hegerl, United Kingdom]	Refefence to CH14 now made.
11-853	11	35	1	35	32	This discussion of circulation changes is oriented towards issues that are mostly hydroclimatically relevant. Please discuss some of the temperature (at least) implications also. [Government of United States of America]	Within the space constraints, we could not add discussion as there was a need to cut the size of the chapter considerably.
11-854	11	35	2	35	2	Is the due to the fact that aerosol effects are more specific regional as compared to GHG driven climate effects? Provide an explanation for this observation [European Union]	The sensitivity is complex, depending on the spatial and vertical distribution of the aerosols, their optical properties, their indirect impacts, etc.
11-855	11	35	3	35	5	“…Meanwhile, the strength and structure of the Walker circulation are impacted by internal climate variations, such as the El Niño/Southern Oscillation (e.g., Battisti and Sarachik, 1995), the PDO (e.g., Zhang et al., 1997)…”. The AAO has also influence on the Walker circulation, which should be included. Thus the above sentence is changed to as “…Meanwhile, the strength and structure of the Walker circulation are impacted by internal climate variations, such as the El Niño/Southern Oscillation (e.g., Battisti and Sarachik, 1995), the Antarctic Oscillation (AAO) (Sun et al., 2009), the PDO (e.g., Zhang et al., 1997)…” Reference: Sun, J. Q., H. J. Wang, W. Yuan, 2009: A possible mechanism for the co-variability of the boreal spring Antarctic Oscillation and the Yangtze River valley summer rainfall, International Journal of Climatology, 29, 1276-1284, doi:10.1002/joc.1773. [Hong Liao, China]	Sentence has been deleted.
11-856	11	35	3	35	5	“…Meanwhile, the strength and structure of the Walker circulation are impacted by internal climate variations, such as the El Niño/Southern Oscillation (e.g., Battisti and Sarachik, 1995), the PDO (e.g., Zhang et al., 1997)…”. Besides the ENSO and PDO, Sun et al. (2009) found that the AAO has also influence on the Walker circulation, which should be added here. Sun, J. Q., H. J. Wang, W. Yuan, 2009: A possible mechanism for the co-variability of the boreal spring Antarctic Oscillation and the Yangtze River valley summer rainfall, International Journal of Climatology, 29, 1276-1284, doi: 10.1002/ joc.1773. [Jianqi Sun, China]	Sentence has been deleted.
11-857	11	35	18	35	18	"... (see Section 14.3.10) …” There is no such section in chapter 14. [Jianqi Sun, China]	Reference corrected.
11-858	11	35	18	35	19	"In response to projected increases in GHGs the (see Section 14.3.10) there is an expectation for a reduction	Paragraph has been deleted.

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						in the strength of the monsoonal circulations, yet an increase in monsoon rainfall." The East Asian summer monsoon circulation and precipitation are both projected to be increased. So I suggest that the above sentence is changed to as "In response to projected increases in GHGs the East Asian summer monsoon circulation and precipitation are both increased (Sun and Ding, 2010; Chen and Sun, 2009), while the Indian monsoon circulation is decreased although the monsoon rainfall is increased". References: Sun, Y., Y. H. Ding, 2010: A projection of future changes in summer precipitation and monsoon in East Asia, Science in China: Earth Sciences, 53, 284-300. Chen, H. P., J. Q. Sun, 2009: How the "Best" Models project the future precipitation change in China, Adv. Atmos. Sci., 26(4), 773-782. [Hong Liao, China]	
11-859	11	35	21	35	26	I think all these studies apply to absorbing aerosols and the South Asian monsoon - if so, state this. [Nathan Gillett, Canada]	Paragraph was deleted.
11-860	11	35	21	35	32	This paragraph has some inaccuracies, as some studies were done with AGCMs only, some included only absorbing aerosols, some did not include aerosols indirect effects, some had prescribed aerosols or their radiative forcing. These differences, due to the progressive development of aerosols treatment in models, are in large part responsible for the disagreement among the modeling studies. To support the conclusions from the modeling studies, It should also be mentioned that the observed precipitation change over India during the last half-century is a decrease. [Massimo Bollasina, Italy]	Paragraph was deleted.
11-861	11	35	26	35	26	Has this to do with the size distribution of aerosols corresponding to the finding of Junkermann et al 2011 The climate penalty of clean fossil fuel combustion. Atm Chem Phys 11, 12917-12924 who claims that more small particles are emitted if filters are used during fossil fuel combustion? [European Union]	Paragraph was deleted.
11-862	11	35	26	35	29	"…Further, internal climate variations associated such as ENSO, AO, AAO, TBO, IOD and AMO (see Section 14.2.5) can influence monsoon rainfall and circulation (Ashok et al., 2004; Gadgil et al., 2004; Li et al., 2008; Meehl and Arblaster, 2011; Meehl and Arblaster, 2012; Nolte et al., 2008; Yuan et al., 2008; Zhang and Delworth, 2006; Wang and He, 2012; Sun et al., 2009; Gong et al., 2011)…". [References: (1) Wang, H. J. and S. P. He, 2012: Weakening relationship between East Asian winter monsoon and ENSO after mid-1970s. Chinese Science Bulletin, DOI: 10.1007/s11434-012-5285-x. (2) Sun, J. Q., H. J. Wang, W. Yuan, 2009: A possible mechanism for the co-variability of the boreal spring Antarctic Oscillation and the Yangtze River valley summer rainfall, International Journal of Climatology, 29, 1276-1284. (3) Gong, D. Y., J. Yang, S. J. Kim, Y. Gao, D. Guo, T. Zhou, M. Hu, 2011: Spring Arctic Oscillation-East Asian summer monsoon connection through circulation changes over the western North Pacific. Climate Dynamics, 2011, 37: 2199-2216.] [Dabang Jiang, China]	Paragraph was deleted.
11-863	11	35	29			Should 'likely' be in italics? [Clare Goodess, United Kingdom]	Paragraph has been deleted.
11-864	11	35	35			Figure 11.19: what are the white areas? Is this what currently is referred to as "grey shading", i.e.., regions where the multi-model average anomalies are smaller than 2sigma of the multi-model estimate of internal variability from the control runs? Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Figure has been remade.
11-865	11	35	42			Figure 11.20: caption refers to "open circles" for the multi-model average, but there are no open circles…? The multi-model mean seems to be missing. Please add. [Thomas Stocker/ WGI TSU, Switzerland]	Figure has been removed.
11-866	11	35	44	35	44	typo Concentratio [Boris Orlowsky, Switzerland]	Corrected.
11-867	11	35	52	35	52	"Vecchi and Soden, 2007" is duplicated here. Maybe the second one should be replaced with "Vecchi et al., 2006"? [Xiaolan Wang, Canada]	Figure removed.
11-868	11	36	14		55	This misses some recent work analyzing decadal predictions for extremes, e.g. some Met Office work (I think Meade et al.) and again (sorry) Hanlon et al. 2012a. Another point well worth mentioning might be that there is also evidence from attribution work that some of the predicted increases in hot extremes might be overestimated at least over some regions (ch10 has a figure) [Gabriele Hegerl, United Kingdom]	References to those two publications have now been added.
11-869	11	36	18	36	18	Orlowsky and Seneviratne is from 2012. Which paper by Sillmann do you mean? One of the two parts submitted in 2012, not 2011? [Boris Orlowsky, Switzerland]	References have been corrected and updated.

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11-870	11	36	18	36	18	The reference Sillmann et al. 2011 is incorrect in this context, as it deals with the influence of atmospheric blocking on extreme cold temperatures in Europe. A more general overview on projected changes in a CMIP3 model is given in Sillmann and Roeckner 2008 (Climatic Change) and in an ensemble of CMIP5 models in Sillmann et al. 2012 [Jana Sillmann, Canada]	References have been corrected and updated. Here Sillmann et al. 2013 is referenced, Sillmann et al. 2011 is referenced later.
11-871	11	36	19	36	19	the reference should be Fischer and Schär 2009 (in Schär is “a” with two dots). please correct here and in all other occurrences. [Jana Sillmann, Canada]	Reference corrected. Thanks ;-)
11-872	11	36	20	36	20	"...Marengo et al., 2009; Meehl et al., 2009a)...". The review paper of Wang et al. (2012) on the extreme climate researches over East Asia should be added here. Thus the above sentence is changed to as "...Marengo et al., 2009; Meehl et al., 2009a; Wang et al., 2012)". Reference: Wang, H. J., J. Q. Sun, H. P. Chen, Y. L. Zhu, Y. Zhang, D. B. Jiang, X. M. Lang, K. Fan, E. T. Yu, and S. Yang, 2012: Extreme Climate in China: Facts, Simulation and Projection, Meteorologische Zeitschrift, doi: 10.1127/0941-2948/2012/0330. [Hong Liao, China]	Reference included
11-873	11	36	24	36	31	There are now some studies assessing the skill for predicting extremes in near-term climate predictions, showing significant skill resulting from the radiatively forced signal. Two such studies are: (1) Eade, R., E. Hamilton, D. M. Smith, R. J. Graham and A. A. Scaife, Forecasting the number of extreme daily events out to a decade ahead, 2012, J. Geophys. Res., 117, D21110, doi:10.1029/2012JD018015 (2) Hanlon, H. M., G. C. Hegerl, S. F. B. Tett and D. M. Smith, Can a decadal forecasting system predict temperature extreme indices?, J. Climate, accepted [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	References to these publications have now been added.
11-874	11	36	29	36	29	Delete "J." from "Sillmann J. et al., 2012". [Xiaolan Wang, Canada]	Corrected
11-875	11	36	38	36	38	remove "J." after Sillmann [Boris Orlowsky, Switzerland]	Corrected
11-876	11	36	38	36	38	Delete "J." from "Sillmann J. et al., 2012". [Xiaolan Wang, Canada]	Corrected
11-877	11	36	40	36	40	I know what ENSEMBLES refers to, but will most other readers? I rather doubt it. [Fyfe John, Canada]	Text improved to include more details.
11-878	11	36	43	36	43	typo Figure 11.1b23 [Boris Orlowsky, Switzerland]	Corrected
11-879	11	36	43	36	43	Replace "Figure 11.1b23a-b" with "Figure 11.23a-b". [Xiaolan Wang, Canada]	Corrected
11-880	11	36	46	36	47	"This difference between changes in mean and extremes can be explained by increases in interannual and/or synoptic variability, or increases in diurnal temperature range" More than an explanation it seems to me that only shows coherence of results. [Ramon de Elia, Canada]	Some of the quoted references give more detailed explanation, space is too short to provide this in the text.
11-881	11	36	57	36	57	It seems to me that in figure 11.23d 10th percentile of Tmin will be more useful. [Ramon de Elia, Canada]	This might depend on the impact considered. In general, in chapter 11 we deal with impacts from a climate perspective. Extremes from an impact perspective (e.g. heating degree days during for winter, etc) are addressed by WG2.
11-882	11	36	57	36	58	The extreme temperature change shown in 11.23d does not show the largest change in the N-NE part of Europe - the largest values seem to be over north Africa. I suggest deleting 'with high intensity in the N-NE part of Europe', since the focus here is on how extremes change. Also delete 'This pattern tends to persist to the end of the century'. [Nathan Gillett, Canada]	This sentence was meant to address Fig.23c (and not Fig.23d). Reference to figure was change correspondingly, and description of figure was improved, partly following comments #884 and #885. It also makes sense to compare the near-term to the longer-term (centennial) changes, because this indicates that these change patterns are not dominated by natural variability.
11-883	11	36	57	36	58	Better: "with the largest change in the N-NE part" [Jouni Räisänen, Finland]	Suggestion followed.
11-884	11	37	2	37	2	compare Figures 11.23e and 11.23f [Boris Orlowsky, Switzerland]	Suggestion followed.
11-885	11	37	2	37	3	"which is indicative of reductions in INTRASEASONAL variability"? [Ramon de Elia, Canada]	From the figure itself we cannot conclude whether this

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							is intraseasonal or interannual variability (both would yield similar effects). However, the studies quoted in the following sentence confirm that synoptic (i.e. intraseasonal) variability changes are relevant.
11-886	11	37	18	38	2	When discussing extreme rainfall more effort should be made to define the indices being assessed. Rare, very heavy, rainfall extremes are of much greater interest in impact terms. [Government of Australia]	In response to this comment we have changed Fig.SOD11.22 and now show another index relevant to much heavier extremes. However, a majority of the published studies consider more intermediate precipitation events.
11-887	11	37	18	38	2	When discussing extreme rainfall more effrot should be made to define the indices being assessed. Rare, very heavy, rainfall extremes are of much greater interest in impact terms. [Penny Whetton, Australia]	In response to this comment we have changed Fig.SOD11.22 and now show another index relevant to much heavier extremes. However, a majority of the published studies consider more intermediate precipitation events.
11-888	11	37	18			What about changes in 'dryness'? E.g., Sheffield and Wood, 2008, Projected changes in drought occurrence under future global warming from multi-model, multi-scenario, IPCC AR4 simulations. Climate Dynamics 31, 79-105. The estimated year of detecting various increases in drought for the Mediterranean falls within the near-term period. e.g., 2018 for increased area of drought. [Clare Goodess, United Kingdom]	We have added additional panels to Figs. 11.16-18 (SOD numbers) and provide substantially enhanced information on the water cycle. In addition to a general discussion of the revised figure, we now have a detailed paragraph in section 11.3.2.3.2, where the drought issues is discussed. Thie mentions, among several additional studies, also the Sheffield and Wood (2008) paper. However, we are unable to provide an assessment for near-term drought changes in the near term, as this cannot be justified based on the comparatively few studies that address this time horizon.
11-889	11	37	20	37	20	"likely to be strongest". [J. Graham Cogley, Canada]	This text has been removed as a result of shortenings.
11-890	11	37	20			Should 'likely' be in italics? [Clare Goodess, United Kingdom]	This text has been removed as a result of shortenings.
11-891	11	37	22			Why is statistical downscaling specifically mentioned here? [Clare Goodess, United Kingdom]	This text has been removed as a result of shortenings.
11-892	11	37	23	37	23	insert "Figures 11.23e,f," before "assessment" [Boris Orlowsky, Switzerland]	Suggestion followed
11-893	11	37	25	37	25	"than for temperature extremes". [J. Graham Cogley, Canada]	This text has been removed as a result of shortenings.
11-894	11	37	27	37	29	Studies on East Asia should be added here, and the suggested sentence is "Since AR4, a larger number of additional studies have been published using global and regional climate models (Fowler et al., 2007; Gutowski et al., 2007; Hanel and Buishand, 2011; Heinrich and Gobiet, 2011; Im et al., 2008; Meehl et al., 2012c; O'gorman and Schneider, 2009; Sun et al., 2007; Xu et al., 2009; Sun et al., 2010).". [References: (1) Xu, Y., C.H. Xu, X.J. Gao, Y. Luo, 2009: Projected changes in temperature and precipitation extremes over the Yangtze River Basin of China in the 21st century. Quat. Int. 208, 44–52. (2) Sun Y., S. Solomon, A. G. Dai, R. W. Portmann, 2007: How often will it rain? J. Clim., 20, 4801-4818. (3) Sun, J., H. Wang, W. Yuan, and H. Chen, 2010: Spatial-temporal features of intense snowfall events in China and their possible change, J. Geophys. Res., 115, D16110, doi:10.1029/2009JD013541.] [Dabang Jiang, China]	In the next version, we now reference the papers of Sun et a. (2007) and Xu et al. (2009)
11-895	11	37	27	37	29	Some related references on East Asia should be added here. Thus this sentence is changed to as "Since AR4, a larger number of additional studies have been published using global and regional climate models (Fowler et al., 2007; Gutowski et al., 2007; Hanel and Buishand, 2011; Heinrich and Gobiet, 2011; Im et al., 2008; Meehl et al., 2012c; O'gorman and Schneider, 2009; Sun et al., 2007; Xu et al., 2009; Sun et al., 2010)." References: Xu, Y., C.H. Xu, X.J. Gao, Y. Luo, 2009: Projected changes in temperature and precipitation extremes over the Yangtze River Basin of China in the 21st century. Quat. Int. 208, 44–52. Sun Y., S. Solomon, A. G. Dai, R. W. Portmann, 2007: How Often Will It Rain? J. Clim., 20, 4801-4818.	In the next version, we now reference the papers of Sun et a. (2007) and Xu et al. (2009)

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						Sun, J., H. Wang, W. Yuan, and H. Chen, 2010: Spatial-temporal features of intense snowfall events in China and their possible change, J. Geophys. Res., 115, D16110, doi:10.1029/2009JD013541. [Shuanglin Li, China]	
11-896	11	37	31	37	31	"Sillmann et al., 2011, 2012". See also P36 L38. [J. Graham Cogley, Canada]	Reference is now to Sillmann et al. (2013)
11-897	11	37	31	37	31	Do you mean Sillmann et al. 2012? Also remove the J. after Sillmann [Boris Orlowsky, Switzerland]	Reference is now to Sillmann et al. (2013)
11-898	11	37	31	37	31	The reference Sillmann et al. 2011 is incorrect in this context, as it deals with the influence of atmospheric blocking on extreme cold temperatures in Europe. [Jana Sillmann, Canada]	Reference is now to Sillmann et al. (2013)
11-899	11	37	31	37	31	Delete "J." from "Sillmann J. et al., 2012". [Xiaolan Wang, Canada]	Suggestion followed.
11-900	11	37	34	37	34	"difficulties in representing". [J. Graham Cogley, Canada]	Suggestion followed.
11-901	11	37	41	37	42	"increases in the higher latitudes and decreases in the subtropics" is only three words longer, and much more readable. [J. Graham Cogley, Canada]	Suggestion followed.
11-902	11	37	44	37	46	A caveat here is that the 95th percentile is not very extreme. If there are differences between the changes in extremes and the mean, these would likely increase in magnitude (though not necessarily in statistical significance) towards the absolute tail of the distribution. [Jouni Räisänen, Finland]	This is a valid point. Text changed to: Figure SOD11.23e-h also shows that mid- and high-latitude projections for changes in DJF extremes and means are qualitatively very similar in the near term, at least for the event size considered
11-903	11	37	45	37	46	There are large areas where the mean and extremes show changes of the opposite signs, e.g., eastern Europe in JJA, or Mediterranean in DJF (Figure 11.23e-h). [Xiaolan Wang, Canada]	This is a valid point. The agreement is for mid- and high latitudes, but not the subtropics. Text changed correspondingly.
11-904	11	37	46	37	46	This holds for DJF but not for JJA. Please rephrase. [Boris Orlowsky, Switzerland]	Suggestion followed.
11-905	11	37	48	37	54	What are the implications of these findings for model results? [Emma Daniels, Netherlands]	One sentence has been cut due to shortenings, one sentence of interpretation has been added. These are relevant results, as hydrostatic models (i.e. most GCMs and RCMs have great difficulties for short-term events.
11-906	11	37	48	37	54	Do these studies specifically look at this near-term period? [Clare Goodess, United Kingdom]	Some of these studies consider past obserations, others are more theoretical. In general, this is an issue that will probably become relevant in the near term. Note that the text has been substantially revised and now includes some additional references.
11-907	11	37	50	37	52	"…Lenderink et al. (2011) show that extreme precipitation exhibits a stronger increase per degree surface dewpoint temperature than expected based on the Clausius-Clapeyron equation…" Two more close related papers should be added here. These two paper found and stronger response of the extreme precipitation to surface temperature. Thus the above sentence is changed to as "…Lenderink et al. (2011), Liu et al. (2009); Sun and Ao (2012) show that extreme precipitation exhibits a stronger increase per degree surface dewpoint temperature/surface air temperature than expected based on the Clausius-Clapeyron equation…" Reference: Liu, S. C., C. B. Fu, C. J. Shiu, J. P. Chen, F. Wu, 2009: Temperature dependence of global precipitation extremes, Geophys. Res. Lett., 36, L17702, doi:10.1029/2009GL040218. Sun, J. Q., Ao J., 2012: Changes in precipitation and extreme precipitation in a warming environment in China. Chinese Science Bulletin, doi: 10.1007/s11434-012-5542-z [Shuanglin Li, China]	We have now included the study of Liu et al in the references, which is a very useful study in this context. We have not incluede Sun and Ao (2012), it appears it is not available in the citation index.
11-908	11	37	50	37	52	"…Lenderink et al. (2011) show that extreme precipitation exhibits a stronger increase per degree surface dewpoint temperature than expected based on the Clausius-Clapeyron equation…" Tow more close related papers should be added here. These two paper found and stronger response of the extreme precipitation to surface temperature. Thus the above sentence is changed to as "…Lenderink et al. (2011), Liu et al. (2009);	We have now included the study of Liu et al in the references, which is a very useful study in this context. We have not incluede Sun and Ao (2012), it appears it is not available in the citation index.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Sun and Ao (2012) show that extreme precipitation exhibits a stronger increase per degree surface dewpoint temperature/surface air temperature than expected based on the Clausius-Clapeyron equation…" Reference: Liu, S. C., C. B. Fu, C. J. Shiu, J. P. Chen, F. Wu, 2009: Temperature dependence of global precipitation extremes, Geophys. Res. Lett., 36, L17702, doi:10.1029/2009GL040218. Sun, J. Q., Ao J., 2012: Changes in precipitation and extreme precipitation in a warming environment in China. Chinese Science Bulletin, doi: 10.1007/s11434-012-5542-z [Hong Liao, China]	
11-909	11	37	50		54	Is this a robust finding or is there just one study? It surprises me.. [Gabriele Hegerl, United Kingdom]	There have been a significant number of additional studies that support this finding. See the revised text, some of the additional references are listed.
11-910	11	37	52	37	52	"Over a wide range". [J. Graham Cogley, Canada]	Paragraph has been rewritten, targeted sentence has been deleted.
11-911	11	37	53	37	54	As written this explanation is unclear. If there are fixed limitations to the water supply or convective activity, presumably these would lead to a weaker increase per degree C for extreme precipitation - the opposite effect to that described here. Is the meaning here that the limitations on moisture supply and convection become less important at higher temperatures? Explain. [Nathan Gillett, Canada]	Paragraph has been rewritten and is now much clearer.
11-912	11	37	56	38	2	Are these aspects likely to be particularly relevant for this near-term period? [Clare Goodess, United Kingdom]	There are several observational studies that document such changes for the past decades (see chapter 9), so this is likely relevant for the near-term. Note that the paragraph starting on line 48 (SOD) has been completely rewritten, and the one started on line 56 has been deleted.
11-913	11	37	57	37	57	It is usual, at least in glaciology, to treat "snowline" as one word. In any case, it should be followed by "altitude". [J. Graham Cogley, Canada]	Paragraph has been deleted.
11-914	11	37	57	37	57	How do you increase a snow line? [Fyfe John, Canada]	Paragraph has been deleted (besides that, we meant increase in the altitude of the snowline)
11-915	11	37	57			Replace 'snow line' with 'snow line height'. [Nathan Gillett, Canada]	Paragraph has been deleted.
11-916	11	37				Section 11.3.2.5.2 Heavy precipitation events: the skill for heavy precipitation has been assessed in decadal climate predictions, showing some potential skill over Europe from the radiatively forced signal, in Eade, R., E. Hamilton, D. M. Smith, R. J. Graham and A. A. Scaife, Forecasting the number of extreme daily events out to a decade ahead, 2012, J. Geophys. Res., 117, D21110, doi:10.1029/2012JD018015 [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	References to this publication has now been added to section 11.2.3.
11-917	11	38	1	38	1	"implications for runoff (". [J. Graham Cogley, Canada]	Paragraph has been deleted.
11-918	11	38	4	38	52	Section 11.3.2.5.3 is especially long-winded and confusing. Considering the scientific and public interest in this topic, it would be useful to tighten the text significantly. [Government of United Kingdom of Great Britain & Northern Ireland]	Description of individual results has been moved to a table.
11-919	11	38	10	38	10	Reference to box 14.3 should be 14.2 [Fabrice Chauvin, France]	Changed reference. Now a section in Chapter 14.
11-920	11	38	12	38	44	We have made a statistical downscaling of TC activity CMIP5 projections (Grinsted et al. in review). We find a quite large sensitivity to projected warming patterns which would imply that the elevated activity should be evident also in the near-term. Please include this work into this discussion. See Grinsted, Moore, Jevrejeva, (PNAS in review), Projected Atlantic hurricane surge threat from rising temperatures. [Aslak Grinsted, Denmark]	Reference discusses projected coastal impacts of TCs (storm surge), which is outside of the scope of the current discussion that focusses on large-scale measures of tropical cyclone activity (frequency, intensity, etc).
11-921	11	38	12	38	44	This paragraph carries on and on, without a very compelling narrative. [Fyfe John, Canada]	Description of individual results has been moved to a table.

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11-922	11	38	12	38	44	A high resolutin AGCM result might be included here. Sugi and Yoshimura (2012) conducted a 228-year long, three-member ensemble simulation using a 60 km gird size AGCM from 1872 to 2099, and found a clear decreasing trend of global tropical cyclone frequensy throughout the simulation, with a statistically significant 6% global TC frequency decrease even at the near-term (2016-2035 versus 1986-2005). Sugi, M., and J. Yoshimura, 2012: Decreasing trend of tropical cyclone frequency in 228-year high-resolution AGCM simulations. Geophys. Res. Let. 39, L19805, doi:10.1029/2012GL053360. [Akio Kitoh, Japan]	Reference added.
11-923	11	38	12	38	44	Is it possible to say something about changes in the spatial distribution of TCs? [Xiaolan Wang, Canada]	Changes in position noted where available in literature (e.g., in the North West Pacific). In general, confidence in projections of spatial structure are lower than on overall frequency and intensity.
11-924	11	38	15	38	15	Orlowsky et al, 2010, JCLI: Future climates from bias-bootstrapped weather analogues: an application to the Yangtze river basin. confirm the reduced TC activity but find relative increased intensity of the typhoons with landfall from a statistical resampling analysis. [Boris Orlowsky, Switzerland]	Reference added.
11-925	11	38	22	38	22	"fifty" instead of "fifity" [Fabrice Chauvin, France]	Statement removed.
11-926	11	38	23	38	27	Another study(Dunstone, N. J., D. M. Smith, L. Hermanson and R. Eade, Aerosol forcing of Atlantic tropical storms, Nature Geoscience, submitted) also shows increases in north Atlantic tropical cyclones as aerosols are reduced. This study also raises the possibility that multi-decadal variations in Atlantic tropical cyclone frequency may have been largely controlled by aerosols, and that aerosols may have suppressed the frequency of Atlantic tropical cyclones throughout the whole period since 1860 (implying that the average of the historical observations might not represent the natural level of variability) [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Manuscript not accepted in time for IPCC-AR5 cutoff.
11-927	11	38	28	38	28	"due both to" instead of "duebothto" [Fabrice Chauvin, France]	Corrected
11-928	11	38	28	38	28	typo duebothto [Boris Orlowsky, Switzerland]	Corrected
11-929	11	38	28	38	28	Replace "duebothto" with "due both to", i.e., add in spaces to separate the three words. [Xiaolan Wang, Canada]	Corrected
11-930	11	38	28	38	28	"... Duebothto ..." seems to be an incorrect form. [Gan Zhang, United States]	Corrected
11-931	11	38	28			… duebothto (typo) [Government of France]	Corrected
11-932	11	38	33	38	33	"temperature" instead of temperautre" [Fabrice Chauvin, France]	Corrected
11-933	11	38	42	38	43	"limited confidence" -- if meant to be a formal uncertainty statement this needs to be expressed using the reserved terms when presenting confidence assessments, i.e., very low, low or medium, whatever is appropriate. Otherwise please avoid using the term confidence. [Thomas Stocker/ WGI TSU, Switzerland]	Changed to "low confidence"
11-934	11	38	42	38	44	We suggest to add this conclusion to the SPM. It is very relevant for policy makers, since it addresses the near term en deals with one of the most important extemes in terms of impacts. [Government of Netherlands]	Considered, but we did not consider it warranted to be included in SPM given raft of other issues of higher priority and tight restrictions on length of SPM
11-935	11	38	43	38	44	After a long (and somewhat confusing) discussion of tropical cyclone frequency changes, this sentence about intensity almost seems dismissive. Increased near-term TC intensity could have a large impact on the collective global society, so this sentence should be explained. [Government of United States of America]	The projections of intensity change have been rephrased. The confidence in the intensity projections is "low" because although there is consistency in sign across studies, there are a very limited number of studies, thetwo studies that focus on the same basin use different intensity metrics used (which does not allow for quantitative comparison of studies), and there is limited understanding of the role of intenal variability.
11-936	11	38	44	38	50	"…Modes of climate variability … (Vecchi and Wittenberg, 2010)". More large-scale factors for the vairbaility of	Reference to second manuscript added.

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						the tropical cyclone should be included here. So the above sentence is changed to as "Modes of climate variability that in the past have led to variations in the intensity, frequency and structure of tropical cyclones across the globe – such as the El Niño Southern Oscillation, the North Pacific Oscillation, the Antarctic Oscillation, are very likely continue to exist through the mid-21st century (Callaghan and Power, 2011; Collins et al., 2010; Vecchi and Wittenberg, 2010; Wang et al., 2007; Wang and Fan, 2007)." References: Wang, H. J., K. Fan, 2007: Relationship between the Antarctic oscillation in thewestern North Pacific typhoon frequency, Chinese Science Bulletin, 52, 561-565. Wang, H. J., J. Q. Sun, K. Fan, 2007: Relationships between the North Pacific Oscillation and, Science in China (D)-Earth Science, 50, 1409-1416. [Shuanglin Li, China]	
11-937	11	38	44	38	50	"…Modes of climate variability …(Vecchi and Wittenberg, 2010)". More large-scale factors for the vairbaility of the tropical cyclone should be included here. So the above sentence is changed to as "Modes of climate variability that in the past have led to variations in the intensity, frequency and structure of tropical cyclones across the globe – such as the El Niño Southern Oscillation, the North Pacific Oscillation, the Antarctic Oscillation, are very likely continue to exist through the mid-21st century (Callaghan and Power, 2011; Collins et al., 2010; Vecchi and Wittenberg, 2010; Wang et al., 2007; Wamg and Fan, 2007)." References: Wang, H. J., K. Fan, 2007: Relationship between the Antarctic oscillation in thewestern North Pacific typhoon frequency, Chinese Science Bulletin, 52, 561-565. Wang, H. J., J. Q. Sun, K. Fan, 2007: Relationships between the North Pacific Oscillation and, Science in China (D)-Earth Science, 50, 1409-1416. [Hong Liao, China]	Reference to second manuscript added.
11-938	11	38	49	38	49	Where is it suggested in the literature that ENSO might cease? This notion seems over-the-top to me, but perhaps I'm missing something. [Fyfe John, Canada]	Rephresed to indicate that the modes of variability will continue to influence TCs.
11-939	11	38	49	38	49	missing "to" between "likely" and "continue"? [Boris Orlowsky, Switzerland]	Added
11-940	11	38	50	38	52	Although natural variability is dominant, there is a scheme to assess potential changes in regional tropical cyclone intensities and associated precipitation extremes due to background warming environment, as documented in Tsutsui (2012). See the previous comment for Box 14.2. A case study for a typical tropical cyclone that makes landfall in Japan has revealed that its intensity, measured by a central pressure drop at sea level, and peak precipitation are projected to increase by 6.5% and 9.3%, respectively, under a globally 1-K warmed environment relative to the present, which corresponds to changes from 1990 to 2040. Error ranges reflecting uncertainties of upper-air warming structure have also been provided. [Junichi Tsutsui, Japan]	Study focuses on basis for expected changes in intensity and potential intensity in a warming climate, not near-term TC projections, so more relevant to Chapter 14.
11-941	11	39	6			Is this referring to globally-averaged depth- averaged temperatures or SSTs? I imagine it's true for both, but the text should be specific. [Nathan Gillett, Canada]	Specified that both SST and depth-averaged temperature.
11-942	11	39	10	39	10	add "However the ocean heat uptake might explain the discrepancy between observations and simulations in the first decade of the 21st century (Hansen et al. 2011, Meehl et al. 2011)." Hansen, J., M. Sato, P. Kharecha, and K. von Schuckmann, 2011: Earth's energy imbalance and implications. Atmos. Chem. Phys., 11, 13421–13449, doi:10.5194/acp-11-13421-2011; Meehl, G. A., J. M. Arblaster, J. T. Fasullo, A. Hu, and K. E. Trenberth, 2011: Model based evidence of deep-ocean heat uptake during surface-temperature hiatus periods, Nature Climate Change, 1, 360-364, DOI: 10.1038/NCLIMATE1229 [Holger Pohlmann, Germany]	The topic of the potential mechanisms behind discrepancies between recent observations and models is addressed in Chapter 9.
11-943	11	39	12	39	14	Are volcanic eruptions really more of a 'key uncertainty' for the future evolution of ocean temperatures, than they are for the future evolution of land temperatures? I don't think that ocean temperatures have a particularly high sensitivity to volcanic forcing - I think this is just a signal-to-noise issue, whereby ocean temperatures are less noisy than land temperatures and therefore the impact of volcanic forcing in historical observations and simulations is clearer. Also the oceanic temperature response is spread over a longer period than the land average temperature response. I would guess that the climate sensitivity, rate of ocean heat uptake, and future tropospheric aerosol evolution are larger sources of uncertainty for near-term projections of ocean heat content than is the possible eruption of large volcanoes. Exaplin more clearly if and why volcanoes are a	It is not argued that they are more or less a source of uncertainty for the ocean than land, but that they are one for the ocean.

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						larger source of uncertainty for ocean temperature than for near-surface air temperature. [Nathan Gillett, Canada]	
11-944	11	39	16	39	16	1815, not 1851 [Jouni Räisänen, Finland]	Corrected.
11-945	11	39	20	39	20	Really don't like the use of the word 'surprises' here. It is not an appropriate or scientifically robust phrase and suggests that there are drivers that are not considered. [Government of United Kingdom of Great Britain & Northern Ireland]	Removed reference to "surprises".
11-946	11	39	27	39	28	Perhaps say here why a subsurface warming maximum is projected for the Arctic. This is due to inflow of warm waters from the North Atlantic, isn't it? This is mentioned later in the chapter (end of 11.3.3.2), but include this here where this feature is first mentioned. [Nathan Gillett, Canada]	Moved statement up to this section (it was in Salinity section and didn't make sense there).
11-947	11	39	30	39	36	It might be worth noting that future changes in aerosol emissions could cause significant regional ocean temperature changes especially in the north Atlantic (Booth et al 2012) [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Noted.
11-948	11	39	39	39	40	What does the phrase 'These differences between models tend to be displaced from regions of high internal variability' mean? That the differences between models are largest where internal variability is low? Explain. [Nathan Gillett, Canada]	Statement has been rephrased.
11-949	11	39	42	39	45	This is false. There's been a fair bit of work on anthropogenic forcing of Southern Ocean temperature change. Take for example, Sigmond et al. (2012), Drivers of past and future Southern Ocean change: Stratospheric ozone versus greenhouse gas impacts, GRL. This is just one of number of recent studies that I've personally been involved in, and there are quite a number of other papers that I have not been involved in. This is a gap in this assessment, not in the literature. [Fyfe John, Canada]	Discussion of Southern Ocean temperature changes - along with a few others - has been removed due to length constraints.
11-950	11	39	42			As written it sounds like stratospheric ozone changes are not anthropogenic. Rewrite 'stratospheric ozone and other anthropogenic forcing'. [Nathan Gillett, Canada]	Rewritten.
11-951	11	39				It is not clear why all the results are based on CMIP3 (Figs 24-27) instead of CMIP5. [Government of United States of America]	Figures have been remade with CMIP5. A reduced set of figures were made, because the use of native grids on ocean data in CMIP5 complicates generating some of the figures.
11-952	11	40	39	40	39	"..e.g. Durack and Wijffels, 2010)" it would be fair to cite Durack et al., 2012 (Science) here [Paul Durack, United States]	Reference added
11-953	11	40	45	40	45	It would be fair to cite Durack et al., 2012 (Science) and Pierce et al., 2012 (Geophys Res Lett) here [Paul Durack, United States]	Reference added
11-954	11	40	47	40	47	It would be fair to cite Durack et al., 2012 (Science) here [Paul Durack, United States]	Reference added
11-955	11	40	52			The phrase 'stabiliize deep ocean convection' sounds like it means make the rate of convection more constant, which I don't thin is the intended meaning. Replace with 'reduce deep ocean convection'. [Nathan Gillett, Canada]	Rephrased as suggested.
11-956	11	40	56			This discussion of salinity changes reads as a list of changes in metrics; is there any chance that a brief overview of the implications of these changes could be included here? (Following the lead of most of the previous subsections,) what does it all mean in the grand scheme? [Government of United States of America]	Discussion of salinity changes has been sharpenned.
11-957	11	41	1	41	32	There has been a lot of work on circulation changes in the Southern Ocean, but none of this work is reflected in this subsection. Here's a gap is this assessment, not the literature. [Fyfe John, Canada]	Length constraints keep us from being able to add discussion - we have shortenned multiple sections.
11-958	11	41	2	41	2	typo; in response to [European Union]	Corrected
11-959	11	41	2	41	2	anthropogenic or better atmospheric GHG [European Union]	Changed to "increases in atmospheric GHG"
11-960	11	41	8			"more rapid recovery" from what? Please clarify what is meant here. [Government of United States of America]	Rephrased to indicate that the ralative amplitude of weakening and recovery was comparing the two

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							models.
11-961	11	41	19	41	19	This statement needs a reference, even if studies from AR4 are cited (e.g., Baehr et al., 2007 J Clim, or 2008 Climatic Change; or Roberts and Palmer, 2012 Clim Dyn). [European Union]	References added
11-962	11	41	21	41	22	The Chapter 12 assessment is that an abrupt transition or collapse of the AMOC is very unlikely in the 21st century [Jouni Räisänen, Finland]	statements removed
11-963	11	41	22	41	22	there is a cross reference to section 11.3.6 but I can't see the relevance of this [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	statements removed
11-964	11	41	24	41	32	As demonstrated by the two references, the equatorial oceanic circulation in the Indo-Pacific depends on the Walker circulation. However, the meridional circulation may also play a role in the oceanic circulation changes described by DiNezio et al., 2009a. As suggested in their Fig. 7b, the meridional components of wind stress are generally greater than the zonal components at Central and East equatorial Pacific. This wind stress pattern may have several impacts: 1) driving the near-surface water eastward where the Coriolis force is not negligible via the Ekman transport; 2) causing the sea water convergence near the equator which further forces the downward motion as seen in their Fig. 8b; 3) contributing to the heat budget via trasport (their Fig. 11 b) and evaporation. In the term of the atmospheric circulation, the meridional circulation is an important contributor to the ITCZ, where the Hadley circulation and Walker circulation are coupled. This may indicate that the identified cloud cover changes are not solely contributed by the Walker circulation changes. Given these facts, it may be somewhat partial to leave the meridional circulation completely unmentioned in the discusssion. [Gan Zhang, United States]	We are unaware of references discussing the role of meridional stress changes in near-term projections of equatorial circulation.
11-965	11	41	34			Section 11.3.4.: please ensure to keep overlap between what is presented here on the Cryosphere and what is being assessed in Chapter 12 at a minimum. There are some subsections (e.g., as done in Section 11.3.4.1 on Sea Ice) here that extend the assessment to the end of the 21st century. This should clearly be avoided and the mid- to long-term assessment should be provided in the relevant sections of Chapter 12. [Thomas Stocker/ WGI TSU, Switzerland]	we have eliminated reference to end of century results in this section
11-966	11	41	37	41	37	I did not find any discussion about changes in the Arctic Ocean in this section. [Thierry Fichefet, Belgium]	we now mention Arctic Ocean warming and refer to where it's shown in Fig. 11.27
11-967	11	41	37	41	37	How is near surface permafrost defined? [Sharon Smith, Canada]	this is now noted to be defined in the glossary
11-968	11	41	43	41	44	It should be ensured that aspects that are now considered as part of AR5, which were not by AR4, are appropriately highlighted. Perhaps this could be included in a list or table somewhere if it is not already? [European Union]	the quantitative values and ranges for changes in sea ice extent, snow cover and near surface permafrost are all from the CMIP5 models, and are now noted as such
11-969	11	41	45	41	45	What is meant by geographical coverage of near surface permafrost? [Sharon Smith, Canada]	"geographical coverage" wording now deleted
11-970	11	41	50	42	35	Section 11.3.4.1: large parts of this section on Sea ice focus on end of the century projections. This should be removed and, if appropriate, included in the Chapter 12 assessment. [Thomas Stocker/ WGI TSU, Switzerland]	we have eliminated reference to end of century results in this section
11-971	11	41	54	41	55	Does this refer to RCP8.5? Not clear which scenario is being referred to. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	use of SRES scenarios now noted
11-972	11	41	54	41	55	Which scenario or scenarios does this result refer to? Is this also RCP8.5? [European Union]	use of SRES scenarios now noted
11-973	11	41	55	42	1	This study has been updated with CMIP5 models (Wang and Overland, 2012). [Thierry Fichefet, Belgium]	agreed, reference and result added
11-974	11	41	55	42	1	For which scenario are the Wang and Overland results presented? [François Massonnet, Belgium]	RCP8.5, now noted
11-975	11	41				Section 11.3.4.1 Sea Ice: should there be some discussion of the recent body of literature suggesting that a declining Arctic sea ice pack might drive changes in atmospheric circulation? See Liu, J.A. Curry, H. Wang, M. Song, and R.M. Horton. 2012. Impact of declining Arctic sea ice on winter snowfall. Proceedings of the National Academy of Sciences of the United States of America, http://dx.doi.org/10.1073/pnas.1114910109,	While there are interesting possibilities of Arctic sea ice loss driving atmospheric circulation changes, this is a process discussion that would be more appropriate for ch 10, and not a near term prediction

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						and references therein [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	of sea ice which is the subject of this section
11-976	11	42	17	42	17	Additionally, Arctic sea ice area loss is greater for CMIP3 models that have higher oceanic heat transport into the Arctic. See: Mahlstein I, Knutti R (2010) Ocean heat transport as a cause for model uncertainty in projected Arctic warming. J Clim 24(5):1451-1460. doi:10.1175/2010JCLI3713.1 and Daniel L. R. Hodson , Sarah P. E. Keeley, Alex West, Jeff Ridley, Ed Hawkins and Helene T. Hewitt (2012) Identifying uncertainties in Arctic climate change projections. 10.1007/s00382-012-1512-z [Dan Hodson, United Kingdom]	agreed, references added
11-977	11	42	19	42	19	Make also reference to Chapter 9 here. [Thierry Fichefet, Belgium]	agreed, reference now made to 9.4.3
11-978	11	42	19	42	21	The statement here that the CMIP5 models exhibit a stronger sensitivity of Arctic sea ice extent to temperature than the obs is not supported by the referenced section 12.4.6.1. The cited figure 12.32 (should be 12.30) shows that the sensitivity of the CMIP5 models is on average higher than the CMIP3 models, but it doesn't compare with observations. Was the inteded meaning 'is more sensitive and comparable to observed trends'? Even this isn't necessarily supported by section 12.4.6.1 which cautions that the role of internal variabilility needs to be properly taken into account when comparing the sensitivity per degree warming of models with observations, and doesn't directly compare them. [Nathan Gillett, Canada]	discussion now revised to refer to 9.4.3 with less emphasis on ch 12, and ice change per degree of warming has been deleted
11-979	11	42	19	42	21	The sentence suggests that the Arctic sea ice extent is more sensitive to warming in CMIP5 models than observed. This is inconsistent with section 12.4.6.1. [Jouni Räisänen, Finland]	discussion now revised to refer to 9.4.3 with less emphasis on ch 12, and ice change per degree of warming has been deleted
11-980	11	42	23			To increase the rate of decrease of Arctic sea ice, the amount of soot in the Arctic would have to increase in the future. But in the RCPs, global black carbon emissions decrease through the 21st century (with the exception of a short-term temporary increase in RCP2.6) - see Figure 8.2. Therefore if the effect of black carbon on snow is missing from the models, this would lead to reduced warming and reduced sea ice decline in the future. This wouldn't contribute to ice-free late summer conditions in the Arctic in the models. Explain. [Nathan Gillett, Canada]	this text now deleted
11-981	11	42	28	42	35	The point regarding timescales for a summer ice-free Arctic i.e. how soon it may occur, does not appear to be adequately represented in the SPM or TS. [European Union]	The statement about a nearly ice-free September will be in the TS and SPM.
11-982	11	42	37	42	39	Chapter 9 also discusses this issue. [Thierry Fichefet, Belgium]	we now make reference to ch 9
11-983	11	42	37	42	47	I would break this paragraph into two paragraphs at line 42, since the first part is concerned with Southern Ocean sea ice only, how models reproduce the observed increase, while the second discusses both hemispheres sea ice and their changes in the future. [François Massonnet, Belgium]	agreed, change made
11-984	11	42	37	42	47	p.42: Explaining the spatial pattern of antarctic sea ice change would be important for this section. -- Written dec 4, 2012 [Aneesh Subramanian, India]	we have now deleted Fig. 11.28, so any discussion of geographical changes would have been related to that figure; the reason it was deleted was that the changes for near-term are not significant in the Antarctic and thus regional changes would also be insignificant
11-985	11	42	41	42	41	"ocen" --> ocean [François Massonnet, Belgium]	agreed, change made
11-986	11	42	42	42	42	Change "Decreases" in "Changes" as the sign "-" is already shown [François Massonnet, Belgium]	agreed, change made
11-987	11	42	42	42	45	These percentages are for ice extent, not ice area (see difference in Chapter 4, page 8, lines 46-50). [Thierry Fichefet, Belgium]	agreed, change made
11-988	11	42	45	42	45	Change "Reductions" in "Changes" as the sign "-" is already shown [François Massonnet, Belgium]	agreed, change made
11-989	11	42	49	43	14	This subsection on snow cover is focused almost completely on snow cover above the Arctic Circle. Snow cover is highly relevant from there to well into the mid-latitudes (climatologically, as well as ecologically and societally), and thus this discussion needs to expand beyond the Arctic Circle. Also, snow water contents are of considerable hydrological and thus hydroclimatic and energy-balance significance so that discussion of the implications of these projections, or of projections of water contents, should be included here. [Government of	there is only one paper that addresses snow only north of 70N; the rest of the results are for all snow covered areas as noted

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						United States of America]	
11-990	11	42	51			Chapter 4, on the cryosphere, uses SCE (snow cover extent) not SCA (snow cover area). It would be helpful if acronyms could be used consistently across different chapters. [Adrian Simmons, United Kingdom]	agreed, we now show snow cover extent
11-991	11	43	2	43	2	(Committee, 2010) is a strange reference. I suppose the ACIA report is meant? [Gerhard Krinner, France]	agreed, change made
11-992	11	43	2	43	2	"...northern spring (March-April-May) (Committee, 2010)". The extreme snowfall has a large contribution to the snow cover. However the extreme snowfall has a different variation with the snow cover, which should be addressed here. So I suggest add the following sentence after the above sentence. "...northern spring (March-April-May) (Committee, 2010). The projected change of snow cover over some regions is inconsistent with that of the extreme snowfall, the major contributor to the snow cover formation. For instance, the snow cover is projected to be decreased over northern China by the mid-21 century (Shi et al., 2011), while the extreme snowfall events over the region is projected to be increased (Sun et al., 2010)". References: Shi, Y., X. J. Gao, J. Wu, and F. Giorgi, 2011: Changes in snow cover over China in the 21st century as simulated by a high resolution regional climate model, Environ. Res. Lett., 6(4), 045401, doi:10.1088/1748-9326/6/4/045401. Sun, J., H. Wang, W. Yuan, and H. Chen, 2010: Spatial-tem1poral features of intense snowfall events in China and their possible change, J. Geophys. Res., 115, D16110, doi:10.1029/2009JD013541. [Hong Liao, China]	extremes are covered in section 11.3.2.5.2; we have added the reference Shi et al (2011) as well as Ji and Kang (2012) for China for projected near-term seasonal changes in snow
11-993	11	43	3	43	3	Increases in snow fall driving increases in snow amount? There is something circular here. [Fyfe John, Canada]	agreed, change in wording made
11-994	11	43	4	43	5	"…Additionally, the reduction of Arctic sea ice also provides an increased moisture source for snowfall (Liu et al., 2012)…" One more closely related paper should be added here. Thus the above sentence is changed to as "…Additionally, the reduction of Arctic sea ice also provides an increased moisture source for snowfall (Liu et al., 2012; Ma et al., 2012)…". [Reference: Ma, J. H., H. J. Wang, Y. Zhang, 2012: Will boreal winter precipitation over China increase in the future? The AGCM simulation under summer 'ice-free Arctic' conditions, Chinese Science Bulletin, 57, 921-926.] [Dabang Jiang, China]	reference added
11-995	11	43	4	43	5	"…Additionally, the reduction of Arctic sea ice also provides an increased moisture source for snowfall (Liu et al., 2012)…" One more closely related paper should be added here. Thus the above sentence is changed to as "…Additionally, the reduction of Arctic sea ice also provides an increased moisture source for snowfall (Liu et al., 2012; Ma et al., 2012)…" Reference: Ma, J. H., H. J. Wang, Y. Zhang, 2012: Will boreal winter precipitation over China increase in the future? The AGCM simulation under summer 'ice-free Arctic' conditions, Chinese Science Bulletin, 57(8), 921-926. [Shuanglin Li, China]	same comment as 11-994, and reference is added
11-996	11	43	16			Section 11.3.4.3 Avoid using the term near-surface permafrost and refer only to increasing thaw depth which is really what is being calculated in these studies. Near-surface permafrost extent/area is confusing terminiology often used by the climate modelling community but rarely by the permafrost science community. This term is not defined, i.e. what depth is being referred to? This is misleading terminology and is often interpreted as complete loss of permafrost. Normally the models on which these statements are based are considering thawing in the upper 2-3 m of the ground and are therefore considering an increase in thaw depth over time rather than a decrease in permafrost extent. In the permafrost chapter of the SWIPA report, use of this terminology was avoided when refering to the results of these modelling studies. Instead, statements such as "models project that the upper 2 to 3 m of permafrost will thaw over X% of the area currently under by permafrost by XXXX" were used in the SWIPA report. It is strongly suggested that similar terminology be utilized here. If the annual thaw exceeds annual freezing over a given area then we can refer to the area over which permafrost is in a degrading state. This would be preferable to the terminology utilized in this section. Ref: Callaghan, T.V., Johansson, M., Anisimov, O., Christiansen, H.H., Instanes, A., Romanovsky, V., and Smith, S. 2011. Chapter 5, Changing permafrost and its impacts. In Snow, Water, Ice and Permafrost in the Arctic (SWIPA). Arctic Monitoring and Assessment Program (AMAP), Oslo, Norway. [Sharon Smith, Canada]	we now define what we mean by "near surface frozen ground" in the Glossary, and is coordinated with the same definition applied in ch. 9 and 12; we also add the SWIPA report (Callaghan et al 2011)

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11-997	11	43	16			Section 11.3.4.3 cont'd - The statement that there is a reduction in annual mean permafrost area of 18% is probably incorrect and is not referring to the reduction in the area underlain by permafrost but rather this poorly defined near-surface permafrost. It is not clear what annual mean permafrost area means - permafrost is defined as frozen ground that exists for at least 2 years and an area is either underlain by permafrost in a given year or it is not - how do you get a mean? [Sharon Smith, Canada]	we have clarified that all discussions here relate to near surface permafrost
11-998	11	43	16			Section 11.3.4.3 cont'd - It would be better to refer (in line 19) to ACIA (2005) rather than Committee (2010) which seems to be some on line encyclopedia which provides a link to the ACIA report. The original/actual report should be cited (by citing Committee 2010 you are implying that this is a more recent report than it actually is). Also, the authors should consider the permafrost chapter of the recent SWIPA report (Callaghan et al. 2011) and the predictions made with respect to future permafrost conditions including ground temperature and active layer thickness. Ref: Callaghan, T.V., Johansson, M., Anisimov, O., Christiansen, H.H., Instanes, A., Romanovsky, V., and Smith, S. 2011. Chapter 5, Changing permafrost and its impacts. In Snow, Water, Ice and Permafrost in the Arctic (SWIPA). Arctic Monitoring and Assessment Program (AMAP), Oslo, Norway. [Sharon Smith, Canada]	we now refer to the SWIPA report (Callaghan et al 2011), not "committee"
11-999	11	43	20	43	22	reference here should be given to the relevant Chapter 9 section where the model evaluation of permafrost is covered (Section 9.4.4.1 and Figure 9.25) [Thomas Stocker/ WGI TSU, Switzerland]	agreed, reference to 9.4.4.1 now added
11-1000	11	43	20	43	23	"…current climate models represent permafrost more accurately". This may be a bit too optimistic given the conclusions of Koven et al., Analysis of permafrost thermal dynamics and response to climate change in the CMIP5 Earth System Models, J. Climate, in press (currently cited in Ch. 12 as "Koven and Riley, submitted"). From the abstract of Koven et al: "Models show a wide range of current permafrost areas, active layer statistics (...), and ability to accurately model the coupling between soil and air temperatures at high latitudes." [Gerhard Krinner, France]	agreed, caveats added and reference updated
11-1001	11	43	23	43	25	What is the reference for this CMIP5 RCP4.5 permafrost result? Is this a surface frost index estimate or diagnosed directly from the CMIP5 modelled soil temperatures? [Government of United States of America]	these results are diagnosed directly from the CMIP5 models, and is for near surface permafrost as defined in the Glossary
11-1002	11	43	27	43	39	This section is far from being exhaustive and is redundant with Section 12.5.5. I suggest to remove it and make reference to Section 12.5.5. [Thierry Fichefet, Belgium]	this section now deleted
11-1003	11	43	29	43	30	"occurred in the late 2000s" should be changed to something like "have occurred over the past decade" in view of the record minimum ice extent in September 2012. [Adrian Simmons, United Kingdom]	this section now deleted
11-1004	11	43	29	43	49	The discussion of near-term abrupt change is weak. The reference seems to be to a single model but then the text is just general statements, not really speculative, but not relating to any simulations. The text on "continued near term loss of sea-ice extent" does not relate to abrupt change and repeats earlier material. [Government of United States of America]	this section now deleted
11-1005	11	43	31	43	34	Reads as if there is a non sequitur here - 'and this raises the question' doesn't obviously follow from the previous phrase ' but in winter…..' [Government of United Kingdom of Great Britain & Northern Ireland]	this section now deleted
11-1006	11	43	38	43	38	Change "predictions" to "projections". [Fyfe John, Canada]	this section now deleted
11-1007	11	43	38			The first reference appears misplaced in this sentence about the uncertainty in simulated sea ice due to clouds. It seems like an appropriate reference for this sentence would be Eisenman et al. (2007, dpi:10.1029/2007GL029914) (which used GCM output and a simpler model to suggest that cloud simulation errors have a large impact on simulated sea ice, and further suggested that small adjustments to the albedo might be masking this effect in the simulated modern ice cover), rather than the included reference to DeWeaver et al. (2008) (which was a Comment on the former paper showing that CCSM3 was less sensitive to albedo than the model used in the former paper). [Ian Eisenman, United States of America]	this section now deleted
11-1008	11	43	46	43	49	This short paragraph mentions snow cover in conjunction with possible sudden abrupt changes. It is hard to imagine any sudden abrupt changes linked to continental snow cover, so it might be good to try to avoid any such link in the reader's mind. To a lesser extent, this might also apply to permafrost. [Gerhard Krinner, France]	this section now deleted

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11-1009	11	44	1			Section 11.3.5: In many places throughout the section results from the SRES scenarios from the TAR were compared with the CLE or MFR variants of the SRES developed for precursor emissions and the new RCPs. What I think is missing is some sense of how different the various emission scenarios are. Ideally I would suggest an additional figure such as Figure 7 of van Vureen et al. (2011) showing the range of global total emission from the scenarios in the literature along with the RCP and SRES scenarios. Alternatively, perhaps a Figure 11.30a that shows regional changes in SO2 and NOx using the same colour scale and regions as Figure 11.30? Or, perhaps less informative, a quick summary within the text of SO2 and NOx global emission totals (or percentage changes) to 2030 in a subset of the scenarios showing the range? [David Plummer, Canada]	Good point, but this is slated to be in Chapter 1, Annex II and should be referred to in this chapter (not enough room for all the figures here). See also chapter 8. We have expanded some discussion in 3.5 of the RCP ranges and changes relative to previous (CLE/MFR, SRES)
11-1010	11	44	1			Section 11.3.5: please make sure to carefully coordinate updates of numbers between this section and the Annex II of the WGI Contribution. [Thomas Stocker/ WGI TSU, Switzerland]	Yes, we will insure they are identical.
11-1011	11	44	3	44	5	Do not agree here: specifically the atmospheric concentration of CO2 (but also of CH4 and N2O) will largely depend on the biosphere feedback to climate change. To say natural emissions is not correct here and also the following statements should be more clear on this. [European Union]	What was intended was that anthropogenic emissions also include those forced climate change, but we will try to fix. Natural emissions can change on their own also (see paleo Chapter 5). We have added "biosphere feedback" to the opening discussion.
11-1012	11	44	4	44	13	"IPCC emission scenarios" -- suggest to change to "scenarios used in IPCC Assessment reports": The newly developed RCPs are (i) not emission scenarios but concentration pathways and (ii) were not developed by the IPCC. For the SRES scenarios the statement would be/have been correct, but it's not clear they are also meant to be referred here. [Thomas Stocker/ WGI TSU, Switzerland]	Yes, thanks, that awkward expression is no longer used.
11-1013	11	44	25	44	28	This point about RCPs not covering the range of pollution emissions found in the literature, and that they tend to Maximum Feasible Reductions scenarios is highly policy relevant. It is noted clearly at TS-37, though the point may be usefully raised in the text as well. This needs a more precise description of what is the exact difference in the literature and the policies assumed in the RCPs including details on when (which year) maximum feasible reductions are reached and how far below these the literature is. [European Union]	A revised comparison with some numerical examples is now included, although a thorough evaluation of the RCPS is beyond WGI.
11-1014	11	44	25	44	28	We have recently presented alternative RCP-like scenarios with varying assumptions on emission factors. This new set of emission scenarios is more representative than the original RCPs in terms of the possible range of emissions of aerosols and short-lived reactive gases. Using chemistry transport model simulations we estimate the implications for future air quality and quantify the effects on surface concentrations of ozone, sulphate and black carbon (Chuwah, Van Noije, Van Vuuren, Hazeleger, et al., Implications of alternative assumptions regarding future air pollution control in RCP-like scenarios, Atmospheric Environment, submitted). Assuming the paper will be published before the AR5 deadline, it would be worthwhile to refer to it in this paragraph (see comments no. 14 and 17) [Twan van Noije, Netherlands]	Thanks. Unfortunately this paper was not accepted by the deadline so we cannot cite it.
11-1015	11	44	27	44	28	That the emissions in the RCPs are 'all tending towards the MFR scenarios.' I feel is a bit too strong of a statement. Total emission of NOx (including biomass burning) in the RCPs changes from ~125.4 in 2000 to between 103.4 (RCP26) and 136.1 (RCP85) Tg-NO2/yr at 2030. In Dentener et al. (2005) the year 2000 base emissions of 120.5 Tg-NO2/yr increased to 137.2 Tg-NO2/yr for the CLE scenario but decreased to 72.8 Tg-NO2/yr in the MFR scenario. The RCP26 has the lowest NOx emissions at 2030, a decrease of 18% from 2000 values, but the older MFR scenario projected a decrease of 40% between 2000 and 2030. While I agree with the general statement, supported by van Vuuren et al. (2011), that the full range of possible emissions in the literature is not represented by the RCPs (a problem that gets even worse in the second half of the 21st century) it seems an overstatement to suggest they all tend towards an MFR-like scenario. (continued in next cell) [David Plummer, Canada]	Thanks, yes, we agree and have revised this discussion to reflect more what you say here. Quantitative comparisons are now made, and vary across the different pollutants.
11-1016	11	44	27	44	28	In Figure 11.30 one can get some sense that the situation is not quite as simple as given by the global emissions totals as there is significant regional variability to the response of ozone, with changes over North America and Europe in the RCP scenarios spanning the range between CLE and MFR, though over East Asia and South Asia the ozone changes from the RCPs are far removed from MFR and overlap with CLE. [David Plummer, Canada]	Agreed and statement has been revised. Regional shift in pollutants is now mentioned.
11-1017	11	44	28	44	32	Are these factors assessed anywhere else in AR5? [European Union]	The impact of changes in natural emissions and

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							deposition through altered land-use, production of food or biofuels (Chapter 6), technology and urbanization, on atmospheric composition and air quality are not assessed here. Projected CO2 abundances are discussed in Chapters 6 and 12.
11-1018	11	44	41	44	43	The sentence beginning 'The RCP-prescribed emissions...' sits by itself and only gains relevance in the discussion within the following section, 11.3.5.1.1. I would suggest moving that statement down to section 11.3.5.1.1 to preface the discussion of updated relationships between emission and atmospheric abundance derived from revised estimates of atmospheric lifetime. Here I have assumed that the statement is referring to the updated abundances of long-lived GHGs such as CH4 and N2O as suggested by the Dlugokencky and Prather references. Moving this rather sweeping statement into where the results are presented helps give perspective to the statement. In fact, I might suggest changing 'do not reflect current best understanding' to 'no longer reflects current best understanding' since, given the long time line behind the production of the RCP scenarios, these updated lifetime estimates were not available when the RCPs were originally constructed. (continued below) [David Plummer, Canada]	Thanks, good idea. We have revised to adopt this suggestion and redone this discussion to 11.3.5. The 'no longer' is not really accurate. It is a structural issue of using a parametric MAGICC model to do biogeochemistry that has been out of date since it was based on the TAR.
11-1019	11	44	41	44	43	I think one also needs to clarify the exact implications, since the statement is worded rather broadly. What is actually being commented on is the exact relationship between the specific emissions of the long-lived reactive gases and the atmospheric abundances. While it is clearly advantageous to have that relationship as robustly defined as possible, variations in future emissions that fall well within uncertainty could easily bring the prescribed future evolution of atmospheric abundance back in line with the specified emissions using the current best understanding of lifetimes. For models that specify the concentrations directly, the actual emissions used to derive the time-evolving tropospheric concentration is rather academic. The situation is more tenuous for models that specify the emissions directly, since one would like to ensure as much consistency as possible between models that specify concentrations or fluxes as boundary conditions. (continued below) [David Plummer, Canada]	Yes, these are good points. The problem is deeper seated than just what the CMIP5's ran with - it relates to the understanding of just what GHG abundances can be expected for what emissions - this is a basic government problem since mitigation is based on emissions, not on abundances. The sections in 11.3.5 and 11.3.5.1 have been revised to try to explain more clearly.
11-1020	11	44	41	44	43	Although to be pragmatic, models have such a range of atmospheric lifetimes for these long-lived gases that scatter in a multi-model ensemble would likely easily cover off the bias between the actual RCP-specified evolution of concentration and the current best understanding shown in Figure 11.29. The statement as it stands, particularly by itself in a paragraph presenting a general overview, seems to suggest there is some fundamental flaw in the specification of the long-lived reactive gases in the RCPs . There may be inaccuracies, but these do not compromise the use of the RCPs to force climate models. [David Plummer, Canada]	We tend to disagree: it is not the range in CMIP5 model lifetimes that matters since we know these models have large flaws in chemistry or stratospheric circulation; but the rather the lifetime and uncertainty is based on observations and observation-constrained models. We stand by the statement, but note that it and the section have been revised per your earlier comments.
11-1021	11	44	42	44		Note sure that this means "The RCP-prescribed emissions, abundances and 42 radiative forcing used in the CMIP5 model ensembles do not reflect current best understanding of natural 43 and anthropogenic emissions,", but certaintly the RCP emissions were, indeed, representative of our best understanding of emissions. [Steven Smith, United States of America]	Yes and No, see above. The *understanding of anthropogenic emissions for the RCPs is no better than the AFOLU reporting accuracy. And certainly must be updated based on BGC and chemistry models for what we know about the budgets. This is clearly referenced here with peer-reviewed literature. The RCPs did not even agree on anthrop sources until "harmonized" - correct?*
11-1022	11	44		52		Section 11.3.5: there was some repetition and lengthiness that could (and I think should) be simplified and clarified. It seems to me that a summary of Arlene Fiore's comprehensive chemical modeling comparison study maybe was added on to the CMIP5 results without synthesizing information from the two studies in a more coherent way. [Government of United States of America]	Agreed, we have revised to synthesize more, and cut length.
11-1023	11	44		52		Section 3.5: Some parts of this section are very dense and difficult to follow. We feel that this section would benefit from an overall summary. [Government of United States of America]	See above #1022.
11-1024	11	44		52		Section 11.3.5: there was some repetition and lengthiness that could (and I think should) be simplified and clarified. It seems to me that a summary of Arlene Fiore's comprehensive chemical modeling comparison	See above #1022.

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						study maybe was added on to the CMIP5 results without synthesizing information from the two studies in a more coherent way. [Government of United States of America]	
11-1025	11	44		52		This is an interesting section, but it could be shortened substantially. Note that the discussions about ozone are redundant and could be combined; same for aerosols. Both are critical as raid-cycle climate feedbacks, so the discussion is naturally a bit awkward, but in particular, 11.3.5.1.2 and 5.1.3 could be folded into the material in 11.3.5.2. It seems that two pieces of similar text were combined without synthesis. [Government of United States of America]	We agree that the sections needed to be cleaned up and have done so and even shortened, but we disagree about redundancy. The sections on O3 and aerosols 11.3.5.1.2-3 discuss global changes, related to climate forcing and UV, Section 11.3.5.2 is strictly air quality (surface). These cannot be synthesized into a single section - even the units used (DU vs. ppb) are different.
11-1026	11	45	10	45	18	This section, and especially this paragraph, has abruptly lost the chapter's focus on the "near term". Please refocus this discussion or else give good reasons why not. [Government of United States of America]	The atmospheric composition & air quality extends to 2100 by inter-chapter agreement, rather than splitting discussion between CH 11 and 12. Likewise, we do not cover near term CO2. We have changed the titles of the section to be more clear.
11-1027	11	45	13	45	13	These numbers aren't quite consistent with Ch6, table 6.7. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Yes, we have worked with Chapter 6 to revise their table. 6.7 & 6.8.
11-1028	11	45	13			I am surprised that the total emissions have a lower uncertainty than the anthropogenic emissions, but maybe this is OK. [David Stevenson, United Kingdom]	Please check the papers quoted. Overall emissions are constrained by CH3CCCl3 decay and other, lesser uncertainties, but anthropogenic CH4 emissions have a minimum of 25% uncertainty from bottom up inventories. No further response required.
11-1029	11	45	14	45	15	What is the scaling factor being referred to here? Are the RCP emissions through the whole 21st century rescaled to match observations in 2010? This needs to be explained to set the context for this paragraph. [Nathan Gillett, Canada]	You are correct. We have revised for clarity.
11-1030	11	45	15	45	15	"... OH lifetime of CH4 ..." The number "4" should appear in the subscript form. [Gan Zhang, United States]	Yes, thanks.
11-1031	11	45	15	45	18	What is the context here for mentioning the OH lifetime? Is this used in the RCPs to estimate the CH4 concentration, or is this independent? Is the point of this text just to document the numbers in the appendix? Either way, the context needs to be more clearly explained. [Nathan Gillett, Canada]	Good point. The trop OH lifetime of CH4 is the core value used to derive the lifetimes of many HFCs and other GHG. This is explained better now in 11.3.5.1.1
11-1032	11	45	19	45	19	"... CH4 and the HFCs with ..." The number "4" should appear in the subscript form. [Gan Zhang, United States]	Yes.
11-1033	11	45	24	45	25	What models does this 'model uncertainty' refer to? Is this tropospheric chemistry models? [Nathan Gillett, Canada]	Yes, this is now noted to be "atmospheric chemistry and the anthropogenic component of the CH4 budget." It includes uncertainty estimated in the anthrop emissions and in the changing atmospheric lifetime.
11-1034	11	45	27	45	28	To help be a bit more specific I'll suggest 'Uncertainty in projecting abundances from a given set of emissions is much smaller than the difference between scenarios.' [David Plummer, Canada]	Yes, have revised section to similar wording.
11-1035	11	45	30	45	35	Figure 11.29: The sentence 'The thick solid lines show the published RCP values: red plus, RCP85...' implies the symbols should be on the thick lines when in fact they are on the thin lines. [David Plummer, Canada]	Yes, we have revised the caption.
11-1036	11	45	31	45	35	How reliable are those RCP's if biosphere feedbacks and in the case of CH4 e.g. changes of CH4 emissions from livestock are not fully included? This might be mentioned in the legend [European Union]	Not sure what is meant here. The RCPs do project changing livestock and hence their associated CH4 emissions. We are not sure what they include in terms of changing feed and GMO-related issues that could reduce CH4 emission per ruminant. The biogenic natural changes in CH4 are not included

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							because we do not have an assessment from Chapter 6.
11-1037	11	45	41			Same as comment 1: I am surprised the total emissions are not more uncertain than anthropogenic. [David Stevenson, United Kingdom]	Look at the referenced papers.
11-1038	11	45	49	47	6	This section includes discussion out to 2100, which is beyond of scope of this chapter. [Government of United States of America]	The atmospheric composition & air quality extends to 2100 by inter-chapter agreement. The section headings now state this explicitly.
11-1039	11	46	6	46	12	There should be mention that the future evolution of the Montreal Protocol gases are dependent on the atmospheric lifetimes and that these may decrease more slowly than currently assumed in the WMO 2010 A1 scenario. In particular, it seems probable that CFC-11 has a longer atmospheric lifetime than that used to construct the WMO 2010 A1 scenario. [David Plummer, Canada]	This is an ongoing debate in the community and we have no specific consensus with which to change the WMO 2010. The range of uncertainties noted here is included in the RF calculation in Annex II. It is a very small effect.
11-1040	11	46	8			No apostrophe [David Stevenson, United Kingdom]	Yes.
11-1041	11	46	15	46	38	it would be good to be more explicit here that the projections are for average or background ozone levels, not changes in peak levels. The chapter implies that the latter are more related to local emissions and thus harder to project. But it's important to be explicit about this since peak ozone is what matters for many impacts. for example, see Avnery, S., Mauzerall, D. L., Liu, J. & Horowitz, L. W. Global crop yield reductions due to surface ozone exposure: 1. Year 2000 crop production losses and economic damage. Atmospheric Environment 45, 2284-2296 (2011). Avnery, S., Mauzerall, D. L., Liu, J. & Horowitz, L. W. Global crop yield reductions due to surface ozone exposure: 2. Year 2030 potential crop production losses and economic damage under two scenarios of O3 pollution. Atmospheric Environment (2011). [David Lobell, United States of America]	This point is now clarified in an ES statement and within the section (11.3.5.2). Background vs. peak events. Note, the section referred to here addresses impacts on RF and UV.
11-1042	11	46	19	46	23	Stevenson et al. 2012 (section 3.1.3) discusses the impact of climate change on ozone in the ACCMIP models. It might be worth including some of that discussion here. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Good point, the paper was a late addition and we have added it to our discussion to use its analysis more fully.
11-1043	11	46	20			Better to say enhanced B-D or Strat-to-trop transport rather than just strat circulation? [David Stevenson, United Kingdom]	Text revised to be more general. Due to "changing" stratosphere, do not want to add unneeded technical terms.
11-1044	11	46	22	46	22	NOx emissions from soils are of higher importance as compared to lightning and soil NO (Nox) emissions are projected to further increase in the future due to increased use of fertilizers. Moreover, NO emissions from soils will most likely increase (positive feedback) due to climate change. See e.g. Kesik et al 2006 Future Scenarios of N2O and NO emissions from European forest soils, Journal of Geophysical Research, 111, G02018, Butterbach-Bahl et al 2009 A European wide inventory of soil NO emissions using the biogeochemical models DNDC/ Forest DNDC. Atmospheric Environment, 43, 1392-1402 [European Union]	Fertilizer component is included in anthropogenic scenarios. We have added soil NO and references.
11-1045	11	46	23			Could ref Schumann & Huntrieser (2007?) for lightning; plus perhaps Arneth or P.Young for bVOC? [David Stevenson, United Kingdom]	Beyond scope of this section (specific to the ozone response). This is not intended to reference the current emission algorithms/inventories as would be done in a review but rather assess their impact on trop O3 which those two studies do across a range of models and some observational estimates.
11-1046	11	46	28	46	28	Remove Wild et al. 2011 (duplication: Wild et al 2012 is the same paper) [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Yes.
11-1047	11	46	29	46	38	the term DU is used several times but I didn't see a clear definition of what these units are [David Lobell, United States of America]	Yes, done.
11-1048	11	46	30			When describing changes in tropospheric ozone burden it would likely be more helpful to have the percentages for CMIP5, in addition to DU. This is for comparison between model uncertainties and scenario	Yes, this has been done.

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						uncertainties. [Government of United States of America]	
11-1049	11	46	30			2.6 and 8.5 [David Stevenson, United Kingdom]	Yes, got it.
11-1050	11	46	31			I didn't understand this sentence. [David Stevenson, United Kingdom]	OK, have revised to clarify.
11-1051	11	46	32			6.0 [David Stevenson, United Kingdom]	Yes, done.
11-1052	11	46	34			typo: "The RF from these tropospheric O3 changes (AII.6.7)" should refer to Table AII.6.8. [Government of United States of America]	Cross-references to Annex II are being carefully checked for consistency. This table is now AII.6.7b.
11-1053	11	46	35			NB 0.36 is the O3t RF from 1850 to 2000. Chapter 8 quotes the value for 1750-2010, which is 0.40 - you should probably be consistent (or at least clarify the time period). [David Stevenson, United Kingdom]	We are using the Stevenson 2012 numbers and will cross-check with Chapter 8's final numbers (that should be in the Annex II).
11-1054	11	46	37			Oman ref out of place? [David Stevenson, United Kingdom]	Endnotes problem, have fixed
11-1055	11	46	41	46	41	Add "or warmer temperatures due to dynamical processes linked to increased tropical SSTs". Please refer to: Li, S., J. Perlwitz, M. P. Hoerling, and X. Chen, 2010: Opposite annular responses of Northern and Southern Hemisphere to Indian Ocean warming. J. Climate, 23(13),3720-3738. [Shuanglin Li, China]	This section has been shortened to reduce text. This reference is not relevant to discussion here.
11-1056	11	46	44	46	47	Is it true that none of the quoted references have an estimate for the amount of tropospheric ozone predicted due to increased start-trop exchange? There ought to be some way to put this effect into the context of changes discussed in the previous section. [Government of United States of America]	Yes, the statement is true. Diagnosing STE flux is complicated and inconsistently done across models; further, these studies do not report values but use indirect approaches for their attribution.
11-1057	11	46	44	46	47	The two sentences 'The O3 recovery... This effect is difficult to quantify...' should really be part of the discussion of global tropospheric ozone in the preceding paragraph. The presence in the paragraph about stratospheric ozone disrupts the flow of ideas significantly. Perhaps add 'The O3 recovery and an increase in the overturning circulation of the stratosphere (see following paragraph)...'? [David Plummer, Canada]	Sentence moved to previous discussion of trop O3.
11-1058	11	46	49	46	51	To the discussion of the changes in stratospheric ozone, I might suggest adding a statement along the lines of 'These global average estimates mask marked latitudinal differences in the future stratospheric ozone distribution versus the pre-1980 distribution due to the accelerated overturning circulation.' [David Plummer, Canada]	Have added 'with latitudinal variations'
11-1059	11	46	53	47	6	I'm rather surprised not to see any discussion around the impacts of recent and near-term changes in stratospheric aerosols that are not included in the historical and future CMIP simulations. I'm referring here to the Solomon et al. (2011) piece in Science. I'm also refering to a paper that is in review in GRL by Fyfe et al. titled "Surface response to stratospheric aerosol changes in a coupled atmosphere-ocean model". This paper shows a surprisingly large impact of stratospheric aerosols on regional change over the next decade, say for example on tropical precipitation. [Fyfe John, Canada]	The historical analysis of stratospheric aerosols, including the Solomon et al paper, does not lead to any clear or justifiable projections and thus does not belong here; it is not directly relevant to atmospheric composition & air quality.
11-1060	11	46	55	46	55	A reference to Chapter 7 would be nice here [Gunnar Myhre, Norway]	Yes we incorporated detailed suggestions from Olivier Boucher as to where and which sections to refer to Ch 7 in Ch11.
11-1061	11	47	1	47	2	Please provide reference to support the assertion that black carbon is primarily of anthropogenic origin? What is the relative contribution from wildfires vs. anthropogenic biomass burning? [Government of United States of America]	This statement refers to Chapter 7. Classification of what portion of wildfire is anthropogenic is complicated and too detailed to include given space restrictions
11-1062	11	47	3	47	6	I think it would be worth reiterating here that the RCPs all assume stringent air pollution policies. This is mentioned at the start of 11.3.5 but would be worth repeating here. [Nathan Gillett, Canada]	We are avoiding value judgment associated with "stringent" so instead quantify the decreases occurring under the RCPs
11-1063	11	47	3	47	6	This is shown in Figure 8.2 [Gunnar Myhre, Norway]	Thanks, have referenced.
11-1064	11	47	9	47	12	This is a rather weak argumentation. Is it not possible to limit uncertainty here? Since biosphere emissions/ depositions are dominating the atmospheric sink/sources of CO2/N2O/CH4 I would at least expect a risk/	This section was cut as major points are addressed in previous sections.

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						uncertainty assessment [European Union]	
11-1065	11	47	18	47	21	What does this mean precisely? That predicted changes in temperature can be much larger as shown so far? Or/ and that pathways of the development of atmospheric greenhouse gas concentrations are possibly wrong? Need to be precise here. [European Union]	Section removed.
11-1066	11	47	18	47	21	"With climate change, the atmospheric ..." -- please provide a reference for this statement [Thomas Stocker/ WGI TSU, Switzerland]	Section removed.
11-1067	11	47	23	48	2	I suggest to shorten this section since it is not of high relevance for WG1 [Gunnar Myhre, Norway]	Thanks, we have shortened, but it is a critical part of WGI..
11-1068	11	47	23			Section 11.3.5.2: This section attempts to pull together the model-based projections/predictions of changes to air quality due to climate change. I assume that this was the specific scope of this section. However, there are two clear consequences to this approach: (1) The section does not give a good feel for what to expect for AQ in a changed climate; and (2) what the causes are, i.e., changes in climate (I assume that this is mostly for surface temperature change) versus what is due to (or expected from) emission changes- either as a consequence of the feedbacks or human influenced emission changes. A summary paragraph at the end of this section could succinctly pull together the information to answer a few key questions: (a) What will the AQ in populated regions be in a changed climate (please go beyond just surface temperature increases); (b) what are the factors that are/could be under local (or regional control); and (c) perhaps state that the AQ changes over the past few decades do not give any clear information about the direction and magnitude of changes due to climate change to date. [Akkihebbal Ravishankara, United States of America]	These are good points. An extensive rewrite to shorten this section attempts to incorporate these suggestions. Much of this review and discussion is in Fiore et al. 2012 review . To answer the primary questions: Trends at present driven primarily by emissions rather than climate, and this is brought forward to the ES, as is the distinction between changing baseline ozone versus projected changes on continental-scale regions as well as in more polluted regions.
11-1069	11	47	23			Section 11.3.5.2: Since this is a very model-based chapter/section, some simple qualitative expectations are missing. The focus, as stated in the beginning of the section, is limited to ozone and PM. But, neither of these, for the most part, is directly emitted , but produced in the region (or location). Therefore, a clear delineation of the changes to the precursors would go a long way. [Akkihebbal Ravishankara, United States of America]	Yes, we discuss changes to precursors earlier in section and refer to Figure 8.2, and also explicitly point out that we are concerned with the precursor emissions to ozone, and for aerosols there are direct emissions in addition to photochemical production.
11-1070	11	47	23			Section 11.3.5.2: I realize that this chapter is about the impact of climate change on AQ. But, one of the key information needs for policy makers is how air quality emissions can impact climate change. At least a mention of this issue would be beneficial and possibly a placeholder for future assessments and SPM. [Akkihebbal Ravishankara, United States of America]	This point is covered in Section 11.3.6.1
11-1071	11	47	25	47	25	“tied first?” do you mean controlled by? [Akkihebbal Ravishankara, United States of America]	We have rephrased.
11-1072	11	47	25	47	42	Please say some thing about what the air quality pollutants are, and what are controlled to regulate those pollutants. [Akkihebbal Ravishankara, United States of America]	Yes, we state we focus on ozone and aerosols, and will add a quick list of the key precursors. The full didactic discussion should be in the FAQ of Chapter 8.
11-1073	11	47	32	47	33	please check correctness of references to WGII chapters; to what extent do these Chapters provide a model evaluation of regional climate models? Or should the reference be to WGI Ch9 and 14? (same comment applies to page 48, l2) [Thomas Stocker/ WGI TSU, Switzerland]	Yes, this was meant to be WG1 Chapters and is fixed.
11-1074	11	47	34			What does 'latter two' refer to here? [Nathan Gillett, Canada]	Section revised to avoid confusion.
11-1075	11	47	34			the latter two elements' - I guess this refers back to l26-27 but it is a few sentences ago. [David Stevenson, United Kingdom]	Section revised to avoid confusion.
11-1076	11	47	37			of -> on [David Stevenson, United Kingdom]	Yes, this was fixed by a larger revision of the section
11-1077	11	47	40			Air toxics -> Toxic atmospheric species [David Stevenson, United Kingdom]	Yes, done.
11-1078	11	48	5	48	9	The discussion of the sensitivity of future air quality projections to some quite difficult to assess meteorological quantities is very important here. The US GCRP assessment of climate change effects on ozone showed very significant differences in the magnitude and sign of future changes in ozone in certain regions due to details in how the different regional climate models (RCMs) projected changes in quantities like boundary layer ventilation, deep convection and cloudiness. The regional details of these changes can result from internal	These are good points - specifically that just increasing resolution or running RCMs does not converge to the "best" answer - and we have fully revised and shortened this section, and eliminated this phrasing.

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						variability of the climate system, peculiarities in the RCM parameterizations (the diurnal cycle of deep convection is a great example here) and details of regional changes that are transmitted from the driving GCM. These effects are mentioned in the current text but it is all summarized with the statement that the projections are 'only as good as the regional climate modeling'. The statement seems like an oversimplistic summary given that GCMs show increasing disagreement for relatively straight-forward quantities as the region of analysis becomes smaller. (continued below) [David Plummer, Canada]	
11-1079	11	48	5	48	9	Getting consensus out of GCMs for changes in precipitation on sub-continental scales is hit and miss and the situation for changes in more 'esoteric' quantities such as cloudiness, boundary layer depth or the frequency of deep convection must be even worse. So it is not just a RCM issue; these same uncertainties apply to GCM (or CCM) studies when projections are pushed to produce results on airshed-scale geographic regions. I think section 11.3.5.2.1 should present some discussion of the sensitivity of air quality projections to these difficult to assess physical climate variables and the utility of an ensemble of models to provide us with a measure of the confidence we can have in projections for different regions. I'll note that the issue comes up again on page 49, lines 6 - 8, page 49, lines 41-44 and page 51, lines 54-55 underlining the importance of the issue. I think the discussion would benefit from having a thorough overview at the beginning of section 11.3.5.2.1. (continued below) [David Plummer, Canada]	These are good points - specifically that just increasing resolution or running RCMs does not converge to the "best" answer - and we have fully revised and shortened this section, and eliminated this phrasing.
11-1080	11	48	5	48	9	Although uncertainty about projecting climate change effects on air quality at fine scales should not cloud the fact that, and I am happy to see it mentioned in several places, future air quality will primarily depend on the future trajectory of regional scale precursor emissions. [David Plummer, Canada]	Thanks, this major conclusion has been brought forward.
11-1081	11	48	9	48	9	They are not only "only as good as the regional climate modelling", but the regional climate modelling is only as good as the driving GCM boundary condition. As discussed elsewhere in this chapter there is low confidence in the GCM projections of synoptic meteorology. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Have revised text to address this point in terms of ability to represent key processes, rather than framing as a concern with the regional climate modeling.
11-1082	11	48	11	48	13	I think that the issue is more than just incomplete understanding of the chemical pathways. There are major uncertainties in deposition and emission changes also. [Akkihebbal Ravishankara, United States of America]	Yes, the sources and sinks are referred to in the first part of the sentence although text in final draft is re-written.
11-1083	11	48	17	48	35	Could you please be clear as to what climate changes you are talking about? It appears to me that this is mostly temperature changes. Are the changes to transport included? What about precipitation changes? Also, the key point is that we care more about ozone changes over polluted regions that have the most emissions of key precursors. So, polluted is almost equated with populated because of emissions and health impacts. Therefore, a focus on populated area is what is important. (also see lines 17-21 on page 11-49). [Akkihebbal Ravishankara, United States of America]	As detailed in the cited references, these are chemistry-climate model results which means that they are simulating the air pollutant distributions in a changed climate, so not just temperature, but circulation and hydrological changes. While indeed we tend to focus on populated regions for health impacts, it is also important to understand how the baseline levels upon which the regional pollution builds, are changing.
11-1084	11	48	20	48	20	These lines are green, not blue. What's the difference between the solid and dashed line? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Yes, corrected.
11-1085	11	48	20			Ref to Fig. 11.30 - I think CLIMATE is in green? [David Stevenson, United Kingdom]	Yes, corrected.
11-1086	11	48	30	48	31	Recommend providing a citation for the sentence beginning with "In polluted regions." [William Landuyt, United States of America]	Yes, have rewritten and added references.
11-1087	11	48	32			Blue dashed line' - I don't see any dashed lines on Fig. 11.30 [David Stevenson, United Kingdom]	Yes, corrected.
11-1088	11	48	44	48	46	The text here introduces the finding that high-ozone events correlate with high temperaturtes, and then suggests that this scaling may not apply to projections because 'stagnation episodes…. are unlikely to scale linearly with temperature in a warming world'. Isn't the reason that the variable driving the high ozone is the stagnation episodes, which influences both the temperature and the ozone? Based on this there is no reason to expect that an increase in temperature driven by GHG increases should increase tropospheric ozone. This is not due to some departure from linearity in the response to temperature changes as suggested here. How about 'but largely reflects the fact that stagnation episodes are associated with both high temperatures and	There are non-linear chemical (PAN chemistry) and biophysical responses (isoprene emissions shut off) at extremely high temperatures as detailed in the subsequent sentences. The point raised here is made and addressed elsewhere in the section.

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						high tropospheric ozone, thus may not be predictive of the ozone response to greenhouse-gas driven warming.' [Nathan Gillett, Canada]	
11-1089	11	48	57			Replace 'exposure' with 'concentration'. [Nathan Gillett, Canada]	Done.
11-1090	11	49	7	49	8	This sentence is unclear for several reasons. First the previous sentence refers to differences in climate response between models on the regional scale, but this one refers to climate variability at regional scales. Which one is key here? I would guess that it is the differences in climate response between models. Second I recommend not using 'detection' for the identification of something significant in a model simulation, since it has a particular meaning in the context of the detection and attribution of changes in observations. Use 'identification' instead. Lastly what does 'many ensembles' mean here? Many ensembles of simulations from different models? Many ensemble members from one model? I suppose this gets back to the question of whether it is differences in response between models or regional climate variability which are most important here. Lastly, if a significant climate response is so difficult to identify even in a model context it probably isn't that important. [Nathan Gillett, Canada]	This was meant in terms of signal-to-noise issue, but the sentence was removed in revision as being not critical to the discussion.
11-1091	11	49	19	49	19	Please specify what kind of evidence? Do you mean observational? [Akkihebbal Ravishankara, United States of America]	This is discussed in previous text in 11.3.5.2; we add here a reference to HTAP report and Doherty et al. 2013 that explicitly discuss changes in intercontinetnal tranpsort due to climate warming
11-1092	11	49	25	49	26	Please be specific as to what pollutants are being talked about. PM10? PM2.5? Also, in this section, it is important to note that most of the AQ-related PM is sulfate based. Most of the SO2 is anthropogenic. Sources of SO2 change significantly with the fossil fuel being burned. Acknowledgement of this would be useful. There is nothing said about the mixing of aerosols (e.g., organics and sulfate). Lastly, there is nothing here about soot. [Akkihebbal Ravishankara, United States of America]	Have now carefully defined throughout as PM2.5.
11-1093	11	50	17			Replace 'degrades' with 'increases' if this is the intended meaning. [Nathan Gillett, Canada]	Correct, done.
11-1094	11	50	23	50	24	The sentence 'The largest surface O3 changes under the RCP scenarios are much smaller than those projected under the older SRES scenarios' is a good example of how the section would be helped if there was some quantitative discussion of how the global precursor emissions have changed between the SRES scenarios of the TAR, the MFR and CLE variants for AR4, and the current RCPs. See general comments on section 11.3.5. [David Plummer, Canada]	This is now discussed in 11.3.5.
11-1095	11	50	23	50	32	Can the authors make an assessment as to their expert judgement on the likely range of future surface ozone. Is MFR the likely minimum? Is RCP85 the likely maximum - or might it go higher than that if the NOX/VOC controls in RCP85 aren't followed? [William Collins, United Kingdom of Great Britain & Northern Ireland]	This assessment of scenario likelihood falls outside the scope of WG1. Rather than make a value judgment, we limit our discussion to a neutral comparison of the ranges in the different scenarios. [pls confirm that this is still case given assessment of NT global temepratuire -- YES for all RCP discussion. THis comment isn't addressing T response]
11-1096	11	50	23	50	39	Are the lower projected increases under the RCPs compared with SRES due to the RCPs tending towards MFR? This section probably contains the relevant information, but could draw together the summary points with a bit more clarity. [European Union]	RCPs fall between CLE and MFR with a narrower range - this is now discussed qunatitatively earlier in 11.3.5.
11-1097	11	50	23	50	39	Assuming the paper mentioned in comment no. 16 will be published before the AR5 deadline, it would be worthwhile to include a reference to it in this section. [Twan van Noije, Netherlands]	As noted above, paper was not accepted in time.
11-1098	11	50	27			Do you mean Dentener et al 2006 (not 2005?). This may also explain comment 17? [David Stevenson, United Kingdom]	Yes.
11-1099	11	50	37	50	37	I don't think the sentence on intercontinental transport is needed. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Agreed, sentence removed. [confirmj action taken-YES this is done]
11-1100	11	50	43			Should 'major' be 'dominant'? Based on what I have read in this section, it seems as though emissions are the dominant driver of tropospheric ozone. [Nathan Gillett, Canada]	Actually 'major' is the intended usage since it is not necessarily dominant everywhere.

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11-1101	11	50	44			Should 'O3 air quality' be 'O3 concentration'. Or is 'air quality' here measuring something more subtle? Explain if retained. [Nathan Gillett, Canada]	Yes, have changed.
11-1102	11	50	47	50	47	Replace Wild et al. 2011 with Wild et al. 2012. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Yes, thanks, references updated. [confirm action taken YES
11-1103	11	50	52	50	56	Recommend providing a citation for the sentence on the competition between sulphate and nitrate, for example Unger (2011) in Geophysical Research Letters. [William Landuyt, United States of America]	This is textbook knowledge, preceding the given reference. We now refer to Chapter 7 where this can be reviewed.
11-1104	11	50	54	50	56	We find this sentence confusing to the point of being potentially ambiguous. One possible reading is that one consequence of increasing NH3 is a reduction in sulfate. We don't believe that is the intent of the sentence. If not, please clarify the sentence. [Government of United States of America]	Yes. Have revised to simplify and clarify.
11-1105	11	51	1	51	2	Would nitrate aerosols have comparable radiative effects to sulphate aerosols? What about effects on health and the environment? [Nathan Gillett, Canada]	This is a good question, but far too much detail given the tenuous nature of the conclusion (medium evidence). We leave at "aerosols levels equal to or greater than." We cannot go into health here and the AF/RF comparison has little data.
11-1106	11	51	16			We think that Fairlie et al. (2007) should be added to the list of references here. [Fairlie, T.D., D.J. Jacob, and R J. Park (2007), The Impact of Transpacific Transport of Mineral Dust in the United States, Atmospheric Environment, Vol. 41, No. 6, 1251-1266.] [Government of United States of America]	Yes, have done so.
11-1107	11	51	17			The intercontinental influence of NOx and CO emissions on PM, as suggested in the quoted reference, is through indirect mechanism involving a change in the atmospheric levels of ozone, and hydrogen peroxide and its effect on the formation of sulphate particles downstream. There is a large uncertainty. The text should be expanded to convey this information fully. Anthropogenic emissions of NOx and CO are usually accompanied by emissions of organic and black carbon, which may also contribute to the PM levels downwind. [Government of United States of America]	Expanding text places too much emphasis on one uncertain study. Instead, we include this reference with the others noting that oxidant changes affect PM air quality.
11-1108	11	51	24	51	26	I think the statement on the narrowness of the RCPs needs some care. While what is written is accurate, it is simply a statement about the different constructions of 2 sets of scenarios. Whereas I do think we have a clearer understanding of the likely future air quality by 2100 compared to AR4 and this expert assessment should be brought out here. RCPs are likely to be towards the low end of possible future (non-methane) emissions, whereas the SRES A2 is now thought to be way beyond anything that's likely. So I think it can be said that range of probable air quality futures is significantly narrower (and cleaner) than thought at the time of AR4, but not as narrow as the RCPs would suggest. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Good point. Last paragraph revised accordingly.
11-1109	11	51	24	51	29	I realize that there is some artificial constraint here about using RCPs versus any other scenarios. But, just because the RCPs used here have much smaller impact than the previously used SRES scenarios really does not tell us what to expect! [Akkihebbal Ravishankara, United States of America]	Agreed, it is an artificial constraint. We do note that other scenarios (modern) have wider ranges.
11-1110	11	51	29	51	30	Insert 'anthropogenic' before 'emissions' (PM pollution is presumably controlled by emissions in dust and wildfire events too, but these are natural emissions). [Nathan Gillett, Canada]	Yes, but that sentence was deleted.
11-1111	11	51	33	51	43	The argument presented here does not make sense to me. Air pollution is associated with stagnation events. Stagnation events are associated with heat waves. Greenhouse gas increases increase the probability of heat waves. But they do this by causing a global warming through change in the radiative balance, not primarily through changing the meteorology. They do not increase the probability of heat waves primarily by increasing the occurence of stagnation events. Therefore why should we expect more tropospheric ozone based on this argument? [Nathan Gillett, Canada]	Indeed, this needed rewriting. We have re-worked the logic and conclusions.
11-1112	11	51	33	51	43	On p. 48L44-48 it is stated that the main ingredient in the pollution events is stagnation, not temperature as such. Assessment of increased heat wave frequency in the future is directly based on simulated temperatures, and is not necessarily an indicator of increased frequency of stagnation events. In a warmer climate, the same atmospheric circulation will produce higher temperatures. Conversely, for the same temperature, a less	Good point. We have rewritten the section. But, heterogeneity of evidence and impacts precludes a making a likelihood statement regarding your conclusion.

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						extreme circulation state is needed in a warmer climate. [Jouni Räisänen, Finland]	
11-1113	11	51	38			Suggest add ref: Lee, J. D., Lewis, A. C., Monks, P. S., Jacob, M., Hamilton, J. F., Hopkins, J. R., Watson, N. M., Saxton, J. E., Ennis, C., Carpenter, L. J., Carslaw, N., Fleming, Z., Bandy, B. J., Oram, D. E., Penkett, S. A., Slemr, J., Norton, E., Rickard, A. R., Whalley, L. K., Heard, D. E., Bloss,W. J., Gravestock, T., Smith, S. C., Stanton, J., Pilling, M. J., and Jenkin, M. E.: Ozone photochemistry and elevated isoprene during the UK heatwave of August 2003, Atmos. Environ., 40, 7598–7613, 2006. [David Stevenson, United Kingdom]	Yes, we have added this ref.
11-1114	11	51	45			I would guess that change in the occurrence of stagnation events with climate change is regionally-dependent. Insert 'in some regions', unless this has been shown to be true globally. [Nathan Gillett, Canada]	Re-written
11-1115	11	51	51			I would have thought that a change in prevailing wind could either worsen or ameliorate pollution. [Nathan Gillett, Canada]	Yes, removed phrase.
11-1116	11	51	52	51	52	minor typographical error 'waves waves'. [David Plummer, Canada]	Thanks, done.
11-1117	11	51	52	51	52	Delete the second “waves” [Akkihebbal Ravishankara, United States of America]	Thanks, done.
11-1118	11	52	1	52	1	The use of the word 'statistically' in 'it is likely that, statistically, a warming climate will exacerbate extreme O3 and PM pollution events for some populated regions...' renders the sentence a bit opaque. There is already 'likely' in the sentence that assigns a statistical strength to our belief and there is 'for some populated regions' which implies that less than 100% of the populated regions will see an exacerbation of air quality issues. It is not clear which facet of the changes is being qualified by the word 'statistically'. Please consider rewording the passage. [David Plummer, Canada]	Yes, the section was revised and this sturcture deleted.
11-1119	11	52	1			What does 'statistically' mean here? 'On average'? [Nathan Gillett, Canada]	Yes, the section was revised and this sturcture deleted.
11-1120	11	52	46	52	49	Discuss the conditions here for the CMIP5 ensemble spread to be a reliable probabilistic projection (the indistinguishable hypothesis, Annan and Hargreaves, 2010). The IPCC Guidance guidance paper on assessing and combining multi model climate projections is relevant here too. Suggested revision to text replacing from 'and rely on': 'and rely on the 5-95% spread amongst the CMIP5 models as a measure of uncertainty. This is interpretable as a 5-95% confidence range for the real world projection under the assumption that the real world is drawn from the same distribution as the CMIP5 climate models (Annan and Hargreaves, 2010; Knutti et al., 2010), and conditional on the forcings following the RCP 4.5 scenario used. It is possible that the real world...' (Annan, J. D. and J. C. Hargreaves (2010), Reliability of the CMIP3 ensemble, Geophys. Res. Lett., 37, L02703, doi:10.1029/2009GL041994. Knutti, R., G. Abramowitz, M. Collins, V. Eyring, P.J. Gleckler, B. Hewitson, and L. Mearns, 2010: Good Practice Guidance Paper on Assessing and Combining Multi Model Climate Projections. In: Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Assessing and Combining Multi Model Climate Projections [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, and P.M. Midgley (eds.)]. IPCC Working Group I Technical Support Unit, University of Bern, Bern, Switzerland. [Nathan Gillett, Canada]	Noted. See our response to Comment 22. Whilst we don't disagree with the broad points made by the reviewer, the idea that climate models and the real world might be drawn from some notional common distribution is a rather philosophical point which we don't believe it is helpful to discuss in the chapter. The key point, on which we agree, and which we make clear, is that the raw model range provides only a crude measure of uncertainty and therefore it is necessary to take into account other sources of evidence when making overall assessments. We have discussed our response to this comment with a Review Editor (Francis Zwiers).
11-1121	11	52	46	52	55	The brevity of this discussion in conjunction with the brief discussion of Figure 11.12 in section 11.3.2.1.1 I find surprising. Media and semi-experts are likely to perceive a downward departure of observed temperature trends from the predicted corridor as a major set-back to climate science. This possibility warrants a more comprehensive and less technical discussion in the chapter. Maybe the issue should also be addressed pro-actively in the executive summary? [Jochen Harnisch, Germany]	Noted. The comparison between CMIP5 models simulations and past observations is discussed in Chapter 9 and 10, including a new box in Chapter 9 focussing on the last 1-2 decades. A synthesis of all the available evidence to produce the best assessed projections for global mean surface air temperature is presented in 11.3.6.3.
11-1122	11	53	14	53	16	The implication here is that the 1452 eruption caused cooling lasting until the 20th century. Pg 62, ln 21-22 says that that volcanoes cause significant surface cooling for a year or so. There is a contradiction here! [Nathan Gillett, Canada]	Clarified in text.

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11-1123	11	53	18			I found this a well put together and useful subsection. [Nathan Gillett, Canada]	Thanks.
11-1124	11	53	38	53	45	Is it possible to say anything about the comparison of the RCP aerosol emission trajectories with observations in the period 2005-2012? The discussion is about them falling at the low end of possible future trajectories, rather than 'realisable' or 'plausible' future trajectories. Are the RCPs already inconsistent with the real world? [Matthew Collins, United Kingdom of Great Britain & Northern Ireland]	We cannot make this statement definitely and have revised text. There are some new studies showing some geographic shifts in aerosol loading but no significant global trends in AOD over the last decade. Sulfate (SO2) is a major anthropogenic aerosol, but not the most important aerosol (dust) nor the only anthropogenic (nitrate). So, our discussion needs to be more balanced about what is known or not known about all the aerosols. SO2 emissions appear plausible to date according to the most recently available bottom-up global emission estimates. We will attempt to clearly differentiate in the revised version between the RCP performance over the recent past versus the projected declines beyond 2010/2020 as compared to other projections.
11-1125	11	53	40	53	40	These scenarios DO NOT assume "uniformly aggressive" reductions. This statement is not supported by the literature cited. The scenarios all assume a middle of the road reduction policies that assume current trends in emission controls continue into the future. [Steven Smith, United States of America]	Sorry, the words are taken directly from the van Vuuren lead paper on RCPs that you are a co-author. This discussion has been moved to earlier and reflects the new SO2 emission papers. Other (non-SO2) aerosol sources do not have as good an evaluation.
11-1126	11	53	42	53	43	There is no support for the statement "The RCP emission trajectories for SO2, ... may not represent the most likely possible future " in the literature. There is strong support for the opposite, that these trajectories are on track (Klimont et al. 2012, and references cited therein). I would be happy to discuss this with the authors of this chapter. [Steven Smith, United States of America]	Yes the one study cited supports that the RCPs are on track. This statement was based on the comparison in Figure 7 of van Vuuren et al., Climatic Change which compared RCPs with many prior future projections. We now discuss both results.
11-1127	11	53	43	53	45	It is not true that these scenarios "fall at the low end of the possible pathways in the published literature". It would be correct to say that these scenarios fall at the middle to low end of current scenarios. Substantial care must be taken when comparing to all the scenarios in the literature (instead of just more recent scenarios) is that older scenarios, such as the SRES scenarios, substantially overestimated current SO2 emissions -- e.g., the older scenarios underestimated the rate of SO2 controls as compared to what has actually happened up to 2010 (Klimont et al. 2012 The last decade of global anthropogenic sulfur dioxide: 2000-2011 emissions. ERL, submitted). There is no evidence that the RCP scenarios for SO2 emissions are off track. [Steven Smith, United States of America]	See above; indeed, we now consider this new study and point out that while Figure 7 of van Vuuren suggests these scenarios may be biased low compared to the published literature, in light of Klimont et al. which suggests the higher SRES scenarios were overly pessimistic. This is about more than SO2.
11-1128	11	53	44	53	45	The references (Hawkins and Sutton, 2009; Hawkins and Sutton, 2010) do not say anything about pollutant gas emissions and, therefore, do not support the incorrect conclusions drawn in the text. [Steven Smith, United States of America]	In the final SOD compilation, endnotes appears to have mis-linked. Those references should be van Vuuren et al., 2011; van Vuuren et al. 2012
11-1129	11	53	44			"may not represent the most likely possible future"? This is a vastly different phrasing and target than the rest of the chapter. We feel that this assessment should be restated in a way more in keeping with IPCC categorizations of projections and uncertainties? [Government of United States of America]	This has been rewritten, it is not appropriate assessment here.
11-1130	11	53	44			References to HS2009,2011 may not be appropriate here? [Ed Hawkins, United Kingdom]	In the final compilation, endnotes appears to have mis-linked. Those references should be van Vuuren et al., 2011; van Vuuren et al. 2012
11-1131	11	53	56	53	56	The decision to "consider here additional scenarios, including the SRES used in the TAR and AR4" cannot be defended by the literature. These scenarios substantially overestimate current SO2 emissions (Klimont et al, Figure 4, and S-2). [Steven Smith, United States of America]	It would be useful to have more evidence for this point. We will present the Klimont view, but it is new and does not address the other major anthropogenic aerosols (NO3, BC, OC, NH3).

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11-1132	11	53		55		The section 11.3.6.1 does not have sub-sections, but can be organized as follows with three sub-sections for more consistent and organized structure: 11.3.6.1 Uncertainties in Future Anthropogenic Forcing and the Consequences for Near-Term Climate, 11.3.6.1.1 The effects of future aerosol emissions (e.g., p53, L47~L54; p54, L55-p58, L8; p55, L10-27), 11.3.6.1.2 The effects of future short-lived greenhouse gases (e.g., p54, L7-L16; p54, L46-L55), 11.3.6.1.3 The effects of land use and land cover changes (p55, L29-L45) followed by the summary paragraph (P55, L47-L57). They will involve some minor editing, though. This organization would better reflect the conclusion of this section that for the near-term projections, different aerosol emission trajectories are likely to have more important effects than those from greenhouse gases. This may substantially improve the readability of the section. [Government of United States of America]	We originally had these subsections -- and will consider restoring if it does not add to the length. We have shortened substantially to meet space restrictions so thus final version will not split into sub-sections.
11-1133	11	54	1	54	28	The paragraph-to-paragraph switching between describing sensitivities in terms of degrees C vs W/m**2 vs degrees C here seems needlessly confusing. We recommend the use of consistent sensitivities, or an explanation as to why it is necessary to switch back and forth. [Government of United States of America]	We have more clearly separated the analysis that is based on the RF (W/m2) which can be evaluated as to separate causes, and the temperature (C) which is based on climate model ensembles. We have shortened this section to make comparisons clearer.
11-1134	11	54	7	54	16	The value given in this paragraph might be appropriate for tabulation. [European Union]	With fewer numbers in the revised section this is not necessary.
11-1135	11	54	15	54	16	How many models include nitrate aerosol? [European Union]	As it says in the sentence, 1 model from that ACCMIP study included nitrate. This has been dropped from text and noted where appropriate in figure captions.
11-1136	11	54	21	54	25	This statement is not clear. Is it referring to a modelling study, with 12 years later being 2002? [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Endnotes problem again - should be Prather, Penner et al., 2009. Text revised for clarity
11-1137	11	54	21	54	25	This sentence is slightly unclear. Is it saying that, assuming emissions were cut in 1990, the global cooling would have reached -0.11 in 2002? [European Union]	Yes, the is correct. Please see above.
11-1138	11	54	24	54	24	Use of °C in some places and K in others; consistency required. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	Thanks, corrected to degrees C everywhere.
11-1139	11	54	26	54	29	This difference between RCP 2.6 and RCP 4.5 is not very compelling in Fig 11.32a. The likely range, 5th percentile, and median are all higher for RCP 4.5 in the 2026-2035 mean - it is only top of the 'very likely' range which is higher for RCP 2.6. It is not clear that this difference is significant. Looking at Fig 11.32a, a much larger difference is apparent between RCP 4.5 and RCP 6.0, with RCP 6.0 exhibiting less warming than RCP 4.5 over all the decadal averages shown. Is this mainly due to aerosol forcing or GHGs? [Nathan Gillett, Canada]	This is a good observation. We now discuss the RCP6.0 anomaly in the text and possible cause. The argument for 2.6 given here is only marginally supported by the SO2 emissions decline that occurs only by 2030, not 2020 - now noted..
11-1140	11	54	34	54	39	Does the comment on applicability of pattern scaling apply to long-term or near-term projections, or both? Or for different variables? - Chapter 12 suggests this might be more of an issue for precipitation. [European Union]	We have removed pattern scaling discussion as it is not relevant to the assessment in Ch 11.
11-1141	11	54	35	54	37	The attribution literature is fairly clear that the aerosol pattern is not the same as the GHG pattern. [Steven Smith, United States of America]	Thanks. This discussion was not wise and not really assessed here it has been deleted.
11-1142	11	54	35	54	39	From the cited studies it may not be clear whether or not the spatial pattern of surface temperature response to aerosols follows the GHG response, but that is because the cited studies generally don't directly address this question. These studies either generally examine the surface temperature response in a single region, focus on only one particular componenet or source of aerosols, or examine a variable other than surface temperature. Detection and attribution studies often rely on differences in the spatial pattern of response to greenhouse gases and aerosols to separately detect the effects of each. From preindustrial to the present, the aerosol response is more intensified over the mid and high latitude NH land than the GHG resopnse. For example see Figure 2 in Gillett et al. (2012). Gillett, N. P., V. K. Arora, G. M. Flato, J. F. Scinocca, and K. von Salzen (2012), Improved constraints on 21st-century warming derived using 160 years of temperature observations, Geophys. Res. Lett., 39, L01704, doi:10.1029/ 2011GL050226. [Nathan Gillett, Canada]	Yes, see above #11-1141
11-1143	11	54	43			"may still dominate"? This seems like a lot less certain confidence statement on this topic than many others in	Good point, statement deleted. Note: references are

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						this chapter. At the regional scale, wouldn't we expect that IV and model uncertainty would still dominate in the near term? If not, then what sets this one apart from all the other places where we would (in this chapter)? We recommend that it be cast in terms of IPCC "likelihood" levels? [Government of United States of America]	not correct, endnotes mis-link. These references have been put into section on aerosols modifying the regional impacts, but that responses are still not fully understood in a way to make a clear assessment statement. We have fully revised the discussion of the role of aerosols in section 11.3.6.1 including confidence statements where supported by the evidence. [unlcear if resposne has been fully dealt with]
11-1144	11	54	46	54	48	This is not the correct references for this statement. Note also that the potential for slowing warming by use of short-lived forcers has not been replicated in the peer reviewed literature (only in grey literature), so this result needs to be caveated as such. [Steven Smith, United States of America]	In the final SOD compilation, endnotes appears to have mis-linked. Those references should be (Hansen, Sato et al. 2000, Fiore, Jacob et al. 2002, Dentener, Stevenson et al. 2005, West, Fiore et al. 2006, e.g., Fiore, West et al. 2008, Royal Society 2008, Jacobson 2010, Penner, Prather et al. 2010, United Nations and World Meteorological Organization 2011). On the second point we have revised these statements providing peer-reviewed references as well as the UNEP reviewed document.
11-1145	11	55	14	55	17	Although Allen and Sherwood (2010) identify an AO-like response to aerosols in winter, Gillett and Fyfe (2012) do not find a significant AO response to aerosols in the CMIP5 models with suitable simulations. Gillett, N. P. and Fyfe, J. C., Attribution of observed sea level pressure changes to greenhouse gas, aerosol and ozone changes, Nature. Clim. Ch., Submitted, 2012. [Nathan Gillett, Canada]	Paper not accepted in time.
11-1146	11	55	26			Is "more likely than not" consistent with "(medium confidence)" here? [Government of United States of America]	We are unsure to what this refers, p 55 line 26 does not seem relevant.
11-1147	11	55	29	55	45	Can an estimation of the relative area of land projected to be subject to change be provided here? [European Union]	See Chapter 6 for LULUC discussion as noted. Our discussion has been shortened and some references added.
11-1148	11	55	29	55	45	This paragraph only focuses on what LULUC might do to climate. Mention should also be included about the prospect that LULUC might (is likely to?) change the land surface and landscape responses to climate. [Government of United States of America]	Yes, new discussion includes pointers to this and related LULCC impacts (Ch.6) - here we focus on the GHG /aerosol changes.
11-1149	11	55	47	55	57	Key point that the near-term CMIP5 projections may overestimate near-term warming. This is well captured at TS-38 and SPM-12. But can more be said regarding the possible systematic errors in the CMIP5 RCP simulations. [European Union]	This discussion has been fully revised, particularly in light of new publications. We found it difficult to make that specific statement and could only point to the limited range of RCPs in terms of aerosols, and the generally lower emissions compared with alternative modern (non-SRES) scenarios.
11-1150	11	55	49			I think saying that aerosols are a 'major source of uncertainty' for near term projections may be overstating things. Looking at Figure 11.32a, it looks like internal variability and model uncertainty are both larger sources of uncertainty than aerosol forcing uncertainty (the width of the uncertainty bars is much larger than differences between scenarios with different aerosols e.g. for 2030). [Nathan Gillett, Canada]	The issue is that the RCPs do not span a particularly wide range for aerosol trajectories so the ranges of futures may be much larger than represented in these cross-scenario differences in Figure 11-32a. We now quantify the "narrow-ness" of the range in comparison to other available emission scenarios in section 11.3.5
11-1151	11	55	50	55	51	The role of nitrate aerosols is not mentioned here. Figure 8.20 shows almost no net change in net aerosol AF between 2000 and 2030 in RCP 2.6 and RCP 8.5 due to the compensating affects of decrease in sulphate aerosol and increases in nitrate aerosol. Based on this figure nitrate aerosols are important, and I think are missed out from most of the CMIP5 models. [Nathan Gillett, Canada]	Yes, the competition for NH3 between nitrate and sulfate aerosols is now discussed in 11.3.5.2 in terms of pollutant aerosols, but the RF changes during this are difficult since not enough CMIP3 models included nitrate aerosols and thus we cannot say more than what is in chapter 8.

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11-1152	11	55	51	55	51	An important point is that removal of aerosols would likely cause relative warming especially of the north Atlantic (in addition to global warming). This is important since it could increase Atlantic tropical cyclone frequency (Dunstone, N. J., D. M. Smith, L. Hermanson and R. Eade, Aerosol forcing of Atlantic tropical storms, Nature Geoscience, submitted). [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The authors do not have a copy of this paper. The results are very interesting, but the regional focus here does not fit in section 11.3.6.1.
11-1153	11	55	51			This conclusion regarding the impact of reductions in sulphate aerosols is overstated in my view. The RCPs all simulate a progressive decrease in aerosols over the course of the full 21st century to close to pre-industrial levels. My understanding is that these SO2 emissions may be considered as a lower bound on SO2 emissions. Chapter 10 shows that the aerosol-attributable cooling to present from preindustrial is a best estimate of ~0.4 K (see Figure 10.4). Thus a plausible range of aerosol-induced warming is ~ 0.4 K/century over the 21st century if SO2 emissions follow the RCP trajectory. This is small compared to the GHG-induced warming over the same period. The current wording 'Removal of sulphate aerosol.... could lead to rapid near-term warming' implies to me that the warming induced would be much larger than that due to GHGs alone. Better wording would be 'could enhance near term warming by approximately X% compared to the warming due to GHGs alone.' [Nathan Gillett, Canada]	Yes, we agree with your analysis, and this assessment has been dropped. We now point to Figure 10.4 and note the bounds for rapid near term change by aerosols.
11-1154	11	55	55	55	57	There is no evidence in the literature support the statement that there is an "overestimate of sulphate reductions under the RCP scenarios" [Steven Smith, United States of America]	This is based on Figure 7 and relevant discussion in van Vuuren et al., Clim. Change, 2011; and also Pozzer, A., Zimmermann, P., Doering, U.M., van Aardenne, J., et al. (2012) Effects of business-as-usual anthropogenic emissions on air quality, Atmos. Chem. Phys., 12, 6915-6937 *("The emission scenario assumes that population and economic growth largely determine energy and food consumption and consequent pollution sources with the current technologies ("business as usual"). This scenario is chosen to show the effects of not implementing legislation to prevent additional climate change and growing air pollution, other than what is in place for the base year 2005, representing a pessimistic (but feasible) future.') We have carefully revised discussion in light of available evidence, now located in 11.3.5 since it affects both this section and also the air quality discussion and so fits best there.*
11-1155	11	55	56	55	56	Should not "implies" rather be "would imply"? The present formulation appears too deterministic. [Jouni Räisänen, Finland]	Yes, we have revised the sentence due to other considerations and adopted use of the subjunctive.
11-1156	11	55	56			Is the rate of aerosol reductions in the RCP scenarios definitely an overestimate? Isn't it just on the high side of the plausible range? [Nathan Gillett, Canada]	We have carefully rephrased in light of new evidence as a subjunctive (were/would) since future reductions are uncertain. Note that the discussion of the emission scenarios has moved to 11.3.5.
11-1157	11	56	25			This section should probably refer to (and take account of) the excellent recent review by Timmreck (2012): WIREs Clim Change 2012. doi: 10.1002/wcc.192. Also, Figure 1 from this paper is probably an improvement on FAQ 11.2 Figure 1 (Chapter 11 Page 129) [David Stevenson, United Kingdom]	Accepted. Reference has been added.
11-1158	11	56	26	57	28	The discussion of solar and volcanic forcing is in several places too absolute. For example, there is uncertainty in how much cooling Pinatubo exactly caused given internal variability (eg ENSO!) so l 29 shouldnt give just one number (and there are lots more papers). [Gabriele Hegerl, United Kingdom]	Accepted.Text has been amended.
11-1159	11	56	26			The phrase 'shock value' comes across as unscientific. How about 'largest potential to influence climate on interannual timescales'? [Nathan Gillett, Canada]	Accepted. Text has been amended.
11-1160	11	56	28	56	30	Be careful with the wording. The often cited 0.5 degree cooling may be a fair estimate for the largest short-term cooling following the eruption, but cooling of this magnitude lasted only for a few months and the annual	Accepted. Text has been amended.

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						mean cooling was smaller (e.g. Fig. 2 of Bender et al. 2010) [Jouni Räisänen, Finland]	
11-1161	11	56	31			"discussed in Chapter 8" should be changed to "discussed in chapters 8 and 10". [Adrian Simmons, United Kingdom]	Accepted. Text has been amended.
11-1162	11	56	35	56	36	"…In addition to global mean cooling, there are effects on the hydrological cycle (e.g., Trenberth and Dai, 2007), atmosphere and ocean circulation (e.g., Stenchikov et al., 2006)…". One new related paper should be added here. Thus this sentence is suggested to change to "…In addition to global mean cooling, there are effects on the hydrological cycle (e.g., Trenberth and Dai, 2007), atmosphere and ocean circulation (e.g., Stenchikov et al., 2006; Wang et al., 2012)…" [Reference: Wang, T., O.H. Otterå, Y.Q. Gao, and Wang H. J., 2012: The response of the North Pacific Decadal variability to strong tropical volcanic eruptions, Climate Dynamics, doi:10.1007/s00382-012-1373-5.] [Dabang Jiang, China]	There are many relevant papers but the purpose here is simply to highlight the most basic points and illustrate with a few key references. There is not space to provide a detailed discussion.
11-1163	11	56	35	56	36	"…In addition to global mean cooling, there are effects on the hydrological cycle (e.g., Trenberth and Dai, 2007), atmosphere and ocean circulation (e.g., Stenchikov et al., 2006)…" One new related paper should be added here. Thus this sentence is changed to as "…In addition to global mean cooling, there are effects on the hydrological cycle (e.g., Trenberth and Dai, 2007), atmosphere and ocean circulation (e.g., Stenchikov et al., 2006; Wang et al., 2011)…" Reference: Wang, T., O.H. Otterå, Y.Q. Gao, and Wang H. J., 2012: The response of the North Pacific Decadal Variability to strong tropical volcanic eruptions, Climate Dynamics, doi:10.1007/s00382-012-1373-5 [Shuanglin Li, China]	There are many relevant papers but the purpose here is simply to highlight the most basic points and illustrate with a few key references. There is not space to provide a detailed discussion.
11-1164	11	56	36	56	39	Other studies noting long-term surface temperature and ocean responses to volcanoes are: (1) Otter˚O H, Bentsen M, Drange H and Suo L 2010 External forcing a as a metronome for Atlantic multidecadal variability Nature Geosci. 3 688–94 (2) Iwi, A.M., L. Hermanson, K. Haines, R.T. Sutton (2012) "Mechanisms linking volcanic aerosols to the Atlantic meridional overturning circulation", Journal of Climate, 25, pp3039-3051. [doi: 10.1175/2011JCLI4067.1] [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	There are many relevant papers but the purpose here is simply to highlight the most basic points and illustrate with a few key references. There is not space to provide a detailed discussion. However, the Ottera et al 2010 paper has been added since it provides a significant new perspective concerning the potential effects of volcanic forcing on the MOC.
11-1165	11	56	43			The phrase 'are not predictable until after the eruption' sounds wrong. If we wait until after the eruption, then we can presumably measure these variables, we don't need to predict them. So either delete 'until after the eruprtion', or replace 'predictable' with 'known'. [Nathan Gillett, Canada]	Accepted. Text has been amended.
11-1166	11	57	4		10	I wouldn't just assume a single sensitivity parameter, apart from that, the response to volcanism isnt well described by ECS alone - this whole estimate is a bit risky and doesn't really account for uncertainties in any convincing way, The discussion of the MM is also tricky as it was forced by a variety of forcings - this is discussed in chapter 10 and also 5 and 9, worth doublechecking and synchronizing. Not sure where the 'unlikely to exceed -0.1 comes from but I doubt this properly accounts for uncertainties - it sounds more like a back nof the envelope calculation (the medium confidence probably expresses this but at the minimum Ii would make sure its consistent with the TCR estimates) [Gabriele Hegerl, United Kingdom]	Accepted. The text here has been simplified and shortened.
11-1167	11	57	30	57	30	The title should include 'near-term'. [European Union]	Accepted ** need to modify text
11-1168	11	57	30	57	30	I struggle with the logic of this subsections placement. It seems to me that this is a pretty important subsection, yet it's buried at the end of the chapter. [Fyfe John, Canada]	This section is placed at the end of the chapter because it draws together information from previous sections. An early decision was taken to focus most of the discussion of projections on RCP4.5 and then discuss variations from this scenario afterwards. We accept that other choices might have been made.
11-1169	11	57	37	57	37	Does likelihood indicate statistical likelihood , uncertainty quantification or another metric ? [Aneesh Subramanian, India]	Likelihood here is intended in the qualitative sense: how likely is one particular scenario as compared to another.
11-1170	11	57	37	57	44	Key point that the near-term CMIP5 projections may overestimate near-term warming. This is well captured at TS-38 and SPM-12. [European Union]	Agreed.

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11-1171	11	57	40	57	40	There is actually no evidence, not "there is some evidence," that "that anthropogenic aerosols may not decline as rapidly as assumed in the RCP scenarios,". All the evidence to date is that actual emissions are declining much as shown in the RCP scenarios. [Steven Smith, United States of America]	*Agreed, text has been revised accordingly.*
11-1172	11	57	45	57	46	Sec. 11.3.6.3, point 2: it seems crucial to include some scenarios including volcanic forcings (strong, average, weak) in the overall assessment. The assumption of no volcanic forcing is extremely unlikely. [Government of United States of America]	Accepted. This part of the discussion excludes volcanic eruptions, but the effect of such eruptions is explicitly discussed in 11.3.6.2.1 and is now included in the Executive Summary statement on global mean temperature rise.
11-1173	11	57	53	57	55	Erors in the spatio-temproral patterns of response to forcings (including those due to errors in the forcings themselves) are not accounted for in the ASK uncertainty estimates (only errors in the magnitude of the response are accounted for). This is a source of uncertainty which isn't accounted for in the ASK method, so this will tend to make the errors bars too narrow, compared to an analysis which accounts for this uncertainty. This could be stated in a more straightforward way in the text here - the current wording hints at this, but doesn't say it explicitly. [Nathan Gillett, Canada]	Noted. Possible sources of error in the ASK estimates are commented on in 11.3.2.1 and the text here has been revised.
11-1174	11	58	5	58	11	"Over the last two decades the rate of global warming that has been observed is at the lower end of rates simulated by CMIP5 models" Should note that the volcanic contribution alone should have caused an even larger rate of observed warming, thus this overestimation by the models is even more in error. [Richard Keen, United States of America]	There is a new box in Chapter 9 (Box 9.2) that provides a full discussion of the comparison of model simulations and observations over the last 1-2 decades.
11-1175	11	58	9	58	11	This seems to me a too important statement to leave unqualified regarding the chances that this scenario may be in fact the case (like in page 60 line 4). [Ramon de Elia, Canada]	There is a new box in Chapter 9 (Box 9.2) that provides a full discussion of the comparison of model simulations and observations over the last 1-2 decades, and the text in this section has been revised accordingly.
11-1176	11	58	12	58	12	Why "to some extent"? [Ramon de Elia, Canada]	Some projections (e.g. Lean and Rind, GRL, 2009) make indirect rather than direct use of climate models. However, the discussion in this section has been extensively revised.
11-1177	11	58	16			See comment 303. One Pinatubo-type eruption can probably be allowed. [Adrian Simmons, United Kingdom]	Accepted. This part of the discussion excludes volcanic eruptions, but the effect of such eruptions is explicitly discussed in 11.3.6.2.1 and is now included in the Executive Summary statement on global mean temperature rise.
11-1178	11	58	21			Since the warming is being discussed in a probabilistic framework, use probabilistic language here too. Instead of 'is considered inappropriate' use 'is unlikely'. [Nathan Gillett, Canada]	Rejected. The discussion here concerns the assessment of the likely range. Use of "unlikely" when discussing the bounds of the "likely" range is likely(!) to cause confusion. However, the text here has been extensively revised.
11-1179	11	58	26	58	26	Fix typo “that that” [Aneesh Subramanian, India]	Accepted. Text has been amended.
11-1180	11	58	54			Recommend "it is expected' --> "it is highly likely" [Government of United States of America]	Accepted. Text has been amended.
11-1181	11	58	57	59	2	Does this refer to first time crossings, or when the threshold is permanently crossed? [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text had been clarified.
11-1182	11	58	57	59	2	Do these crossings refer to permanent crossings, or first time crossings? [European Union]	Accepted. Text has been clarified.
11-1183	11	59	5	59	12	Table 11.2 It seems to me that for the reader there is a confidence jump to go from evidence, to likelihood, to "moderated" likelihood. The avoidance of the concept of subjective probability does not change the fact that they are some form of that. In page 9 it is discussed the concept of "probabilistic prediction". I wonder if something like "probabilistic projection" should also be defined in order to clarify concepts. Quoting Chapter 12	Noted. The discussion of Table 11.2 has been extensively revised to make clear the relationship to likelihood assessments.

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						12.2.2 could be also used as a supplement. [Ramon de Elia, Canada]	
11-1184	11	59	5	59	13	Table 11.2 is highly policy relevant. A version with 2100 targets included would be useful, either for inclusion in Chapter 12, or possibly the SPM. [European Union]	Accepted. The Table has been revised and expanded, and a similar Table for the Late twenty first century period is included in Chapter 12.
11-1185	11	59	5			Table 11.2: It would be very valuable to include higher thresholds such as three and 4 degrees. It would need unlikely and very unlikely assessments, but this would still be very valuable information from a risk perspective (very unlikely is still possible). [Government of Australia]	Accepted. The Table has been revised and expanded, and a similar Table for the Late twenty first century period is included in Chapter 12.
11-1186	11	59	5			Table 11.2: It woudl be very valubale to include a higher threshold such as three degrees. It would need unlikely and very unlikely assessments, but this would still be very valuable information from a risk perspective (very unlikely is still possible). [Penny Whetton, Australia]	Accepted. The Table has been revised and expanded, and a similar Table for the Late twenty first century period is included in Chapter 12.
11-1187	11	59	12	59	12	After "and assume no future volcanic eruptions", add "which would cause temporary cooling". [Government of Australia]	Noted, but the Table and related discussion have been extensively revised.
11-1188	11	59	15			Box 11.2: This is an extremely valuable box. It would be nice to include further assessment of the reliability of different trend estimation and comparison methods but maybe this is for next time when there has been more work done. In any case the points discussed here, particularly lines 29-30, should be highlighted in the executive summary of this chapter. This box and its implications are also of importance for both chapter 12 and chapter 9. Explicit reference to it should be made in those chapters - and in the executive summary of chapter 12. Indeed the box itself would appear to be more suitable for inclusion in chapter 9 where it can be referenced by both chapters 11 and 12. [David Stainforth, United Kingdom]	Noted. The Executive Summary already needed shortening by a factor three. Chapter 9 does not accept that this material fits better there, we have requested a cross-reference. Given the lack of correlation in the model world between past trends and long-term trends a reference in Chapter 12 does not seem useful.
11-1189	11	59	17	59	51	The information given in this box is essential for a proper evaluation of near term (and non- near term) climate projections. This box (and the relative figure) should be placed before (or at the beginning) of the section 11.3 (Near term projection). [Susanna Corti, Italy]	Placement of the box was be discussed but none of the other chapters accepted it fitted with their material.
11-1190	11	59	17			This box seems like it should belong in chapter 9, since the topic is model validation, and the focus is comparing simulated and observed trends in the past. [Nathan Gillett, Canada]	Noted. Chapter 9 does not accept this material.
11-1191	11	59	17			This box needs to start off with a brief discussion of the unceratinty model being used to compare observations and simulations. I would suggest that the 'indistinguisable' assumption is used, which means that consistency is tested by assessing whether the obs trend lies within e.g. the 5-95% range of model trends. See Annan and Hargreaves (2010), Knutti et al. (2010). Annan, J. D. and J. C. Hargreaves (2010), Reliability of the CMIP3 ensemble, Geophys. Res. Lett., 37, L02703, doi:10.1029/2009GL041994. Knutti, R., G. Abramowitz, M. Collins, V. Eyring, P.J. Gleckler, B. Hewitson, and L. Mearns, 2010: Good Practice Guidance Paper on Assessing and Combining Multi Model Climate Projections. In: Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Assessing and Combining Multi Model Climate Projections [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, and P.M. Midgley (eds.)]. IPCC Working Group I Technical Support Unit, University of Bern, Bern, Switzerland. [Nathan Gillett, Canada]	Accepted - Rank histograms showing the reliability of the linear trends most applicable to the Atlas and Chapter figures are now also shown, and the error model is briefly discussed, citong Annan and Hargreaves (2010).
11-1192	11	59	17			This box needs more assessment. At the moment it reads a bit like a list of regions and studies, with overall conclusions not clear. [Nathan Gillett, Canada]	Accepted - reworded to read more like an assessment.
11-1193	11	59	17			Overall this box seems to argue that models do a poor job of simulating regional trends. But I don't think this is a fair assessment. Given that we can only identify inconsistencies in global mean warming in observations and a few models, it seems unlikely that we can identify significant differences on the regional scale, given the larger noise. Fig 10.2 shows only a few locations where CMIP5 average and observed trends are significantly different over the past 110 years. Individual studies are likely to focus on regions of difference, but the box is missing the big picture, which is that overall regional temperature trends are realistic. [Nathan Gillett, Canada]	Taken into account. The figure has been extended with rank histograms as in Yokohata et al 2012, with uncertainty range computed as in Annan & Hargreaves (2010). These show that the regional precipitation temperature trends are not reliable. Temperature trends are reliable, but only because of the large differences in global mean trends, if these are excluded the trends are also not reliable. See van Oldenborgh et al, ERL, 2013. TODO If this is accepted before March 15 it will be cited.
11-1194	11	59	21	59	21	Past performance of climate models? This wording seems ambiguos to us -> do you mean "the ability of	Accepted - Changed as suggested.

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						models to simulate past climate'? [Thomas Stocker/ WGI TSU, Switzerland]	
11-1195	11	59	24	59	25	I don't think Stott et al. (2010) say that trends are not well-represented on sub-continental and smaller scales in climate models - and they definitely don't demonstrate this from data. I think they argue that the noise is higher on these scales and other possible sources of uncertainty are more important, and therefore attribution is harder. This is not the same as a demonstrated inconsitency between simulated and observed trends on these scales. [Nathan Gillett, Canada]	Accepted, reworded to make clear that Stott et al (2010) only emphasaise that the noise is higher and the models differ more.
11-1196	11	59	29	59	30	Who are 'they'? And does this just apply to the regions or to all projections? And what is the uncertainty model assumed here? [Nathan Gillett, Canada]	Accepted - The statistical test is a comparion of the rank histogram with the inter-model spread as used by Annan & Hargreaves (2010. Bhend & Whetton(2013) and Knutson et al (2013) obtain similar results using differnt tests, as in fact already did Räisänen (2007) for CMIP3.Reworded to make this clear.
11-1197	11	59	35	59	36	What does it mean that 'the range in global mean temperature is larger than observed'? The variability is higher? The range of trends calculated over different periods of global mean temperature is larger? - this would be the same as saying the variability is larger. The mean model trend is larger than the observed trend? The spread of simulated trends is larger than different estimates of the observed trend? The latter is not a like-for-like comparison because there is no reason why model uncertainty and observational uncertainty shoudl be the same magnitude. Clarify. [Nathan Gillett, Canada]	Accepted. The phrase has been deleted.
11-1198	11	59	36			It is probably worthwhile to refer to Sakaguchi et al., 2012 here. For example, '… larger than observed (Chapter 2). The regional temperature trends shorter than 50 years simulated by selected CMIP3 and CMIP5 models were assessed in Sakaguchi et al.(2012) across different spatial scales, and they found the skills for the regional (5° x 5° - 20° x 20° grid scales) trend for less than 40 years are still limited, although the CMIP5 models showed slight improvement over the CMIP3 models. Using another metric, Knutson et al. (2012a)… . Sakaguchi, K., X. Zeng, and M. A. Brunke (2012), The hindcast skill of the CMIP ensembles for the surface air temperature trend, J. Geophys. Res., 117, D16113, doi:10.1029/2012JD017765. [Government of United States of America]	Rejected. Indeed, this same question was posed in Sakaguchi et al. However, we find that the methods emloyed in this paper do not allow for an answer to the question, as the verification statistics are computed per grid point with only 2 degrees of freedom for the 50-yr running trends, and 10-yr running trends dominated by natural variability, which is never separated from the forced response.
11-1199	11	59	43	59	45	What error model was used in these studies? Did they use the truth plus error model (i.e. did they calculate the uncertainty in the mean simulated trend by dividing the standard deviation across models by the square root of the number of models, and then use this to assess consistency with obs?). If they use a different error model, this should be coinsidered in the assessment. Was the error model appropriate? [Nathan Gillett, Canada]	Accepted. We mention the error model used by Van Oldenborgh et al, (which replaces van Oldenborgh and Drijfhout) who etst wether the rank histogram is within the band of inter-model variations, i.e., whether natural variability and model spread are enough to explain the spatial variability. Bhend & Whetton have updated thier error model in the revised published version, they now count the number of models in which the observed trend is in he tails of the distribution given by the observed or modelled variability around the model mean, and do a spatial significance test on this field. Knutson et al (2013) also obtain a larger fraction of trends outside in the top 10% of the ensemble (their Fig.13) but do not formally test wehther teh differnece is significant.
11-1200	11	59	45	59	50	"In December-Feburary...ocean trends were lower." The readability of this long sentence is not great. If this sentence structure will not be modified, "The" in "... (van Oldenborgh et al., 2009a), The ..." (line 47) should be replaced with "the". [Gan Zhang, United States]	Accepted. The sentence has been made more readable: "In December–February the observed Arctic amplification in Asia and North America extends further south than modelled. In June–August southern Europe and North Africa have warmed significantly faster than both CMIP3 and CMIP5 models simulated (van Oldenborgh et al., 2009a)."
11-1201	11	60	0	0	0	FAQ 11.1: Why is the first part of the answer in italics and the second half in normal font? I find the style of	This is the agreed format that all FAQs in the report

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						writing for this FAQ in quite a contrast to the very (too) detailed level of explanation in the main Chapter's text. It's not clear to me whether this was fully intended. [Antje Weisheimer, United Kingdom]	have.
11-1202	11	60	1			Gillett and Stott (2009) did not demonstrate an inconsistency between simualted and observed SLP trends. They found that when considering all seasons together and in a global analysis the magnitudes of simulated and observed trends agreed well. [Nathan Gillett, Canada]	Accepted. Added "December–February".
11-1203	11	60	7		17	Analysis of precipitation is too thin in my opinion. This chapter focuses on "Near-term Climate Change: Projections and Predictability" In many areas the rain is a major factor in climate. It is therefore surprising analysis of precipitation is not well developed! [Ibouraïma YABI, Benin]	Taken into account. We have expanded this section, but the problem is that the larger amount of natural variability in precipitation relative to the trends makes it harder to draw conclusions yet in many areas of te world. The amount of literature published on precipitation trends is also smaller than for temperature.
11-1204	11	60	11	60	11	"... circulation change discreoancies ..." "discreoancies" seems to be a typo. [Gan Zhang, United States]	Accepted. The typo has been fixed.
11-1205	11	60	12	60	13	There are actually two Bladé et al papers on the subject. The other is : Bladé, I., Liebmann, B., Fortuny, D., & Oldenborgh, G. J. (2011). Observed and simulated impacts of the summer NAO in Europe: implications for projected drying in the Mediterranean region. Climate Dynamics. doi:10.1007/s00382-011-1195-x [Ileana Bladé, Spain]	[check spelling] Noted. As far as we know, only the second paper compares the NAO teleconnections in the whole CMIP3 ensemble with observations.
11-1206	11	60	15	60	15	As a co-author of this paper I can report that the results are based on 3 models not 1 model. [Fyfe John, Canada]	In fact there are only two.
11-1207	11	60	17	60	17	Fix typo “11.xx)” [Aneesh Subramanian, India]	Accepted, this has been deleted
11-1208	11	60	17	60	17	The citation "Bhend and Whetton, 2012; Figure 11.xx" seems incomplete. Where to find the figure should be made clearer. [Gan Zhang, United States]	Accepted, this has been deleted
11-1209	11	60	23	60	23	Based on Räisänen (2007) is too strongly said. [Jouni Räisänen, Finland]	Accepted. It now states they are based on Räisänen (2007) and van Oldenborgh et al (2013).
11-1210	11	60	27	62	3	FAQ 11.1 (Climate prediction): The chapeau for this FAQ is very long - nearly half as long as the following text. Is it possible to summarise the "key" answer in just (preferably) one or two paragraphs for the chapeau ? [David Wratt, New Zealand]	Chapeau has been reduced.
11-1211	11	60	29			FAQ 11.1: Chapeau is currently much too long. We found the last sentence of the Chapeau particularly useful, so some combination of this with the opening paragraph would make a good length chapeau. [Thomas Stocker/ WGI TSU, Switzerland]	Chapeau has been reduced. Suggestion has been adopted.
11-1212	11	60	29			FAQ 11.1: We think it would be appropriate to have some mention of Volcanoes in this FAQ, and how a volcanic eruption would invalidate near-term climate projections immediately. (see, e.g., paragraph on p63, lines 32-36). This would also link nicely to FAQ 11.2. Some text based on the second to last paragraph of FAQ 11.2 would fit well in FAQ 11.1. [Thomas Stocker/ WGI TSU, Switzerland]	Done (see 2nd last paragraph)
11-1213	11	60	29			This FAQ will be helpful for many readers to understand the predictability difference between weather and climate. However, Paragraph 3 (starting with "Climate predictions") simply states "can have some accuracy" and "can be predicted to an extent" without revealing the real cause to the readers. Trying to convince people with statement rather than evidence will only make the readers even more confused. It is known that weather forecasting exploits the initial condition, while climate predictions rely on the boundary conditions. The first point was made straightforward to understand in Paragraph 2; for the greater convincing power, a similar effort should be made about the second point in Paragraph 3. [Gan Zhang, United States]	Some of the reasons (now expanded) are given in paragraphs 2, 5, and 6.
11-1214	11	60	38	60	41	This paragraph is rather simplistic. The transition from weather to short-term climate (seasonal) prediction is rather more seamless than this. There are aspects of the circulation (that influence temperature and weather type) for which there is a degree of predictability for three and sometimes four weeks ahead; recent extreme cases of the Arctic oscillation are an example of the latter. ECMWF provides operational forecasts for the monthly range, and sub-seasonal prediction is one focus of coordinated international activities. [Adrian	Paragraph has moved, and now contains 'typically' to cater for exceptions. More importantly FAQ 11.1 know contains sentence: " Weather, seasonal-to-interannual and decadal prediction systems are similar in many ways – e.g. they all incorporate the same

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						Simmons, United Kingdom]	mathematical equations for the atmosphere, they all need to specify initial conditions to kick-start predictions, and they are all subject to limits on forecast accuracy imposed by the butterfly effect".
11-1215	11	60	40	60	41	The phrase 'inevitably typcailly leads to' is an oxymoron. [Nathan Gillett, Canada]	'inevitably' removed
11-1216	11	60	45	60	46	We recommend that the authors consider changing the text to "Statistics of some these changes can be…" [Government of United States of America]	Sentence no longer includes this reference to weather events: "For example, increases in long-lived atmospheric greenhouse gas concentrations tend to increase surface temperature in future decades".
11-1217	11	60	46	60	47	"...these changes can be predicted reliably, even though the hour-to-hour or day-to-day evolution of weather conditions cannot be predicted accurately beyond a week or so." [James Renwick, New Zealand]	Mention of 'climate scientists' dropped. Sentence now reads:"For example, increases in long-lived atmospheric greenhouse gas concentrations tend to increase surface temperature in future decades".
11-1218	11	60	55	60	55	Add a sentence at the end like e.g.: "A reason for this is that while weather mainly is the result of the mostly 'accidental' distribution of energy within the climate system, the statistics (or climate) are more influenced by external factors like the hemispheric solar irradiation changes (for seasonal changes in regions), volcanic eruptions (for year-to-year variability) or greenhouse gases (for long-term changes). [Urs Neu, Switzerland]	We introduced final sentence paragraph: "Finally note that decadal prediction systems are designed to exploit both externally-forced and internally-generated sources of predictability. Climate scientists distinguish between decadal predictions and decadal projections. Projections only exploit the predictive capacity arising from external forcing. While previous IPCC Assessment Reports focussed exclusively on projections, this report also assesses decadal prediction research and its scientific basis". Solar influences are discussed in paragraph 2.
11-1219	11	61	14	61	17	Repetition from previous page. Also I think everybody know that weather forecasts try to address questions like "Will it rain tomorrow?" [Antje Weisheimer, United Kingdom]	Sentence shortened.
11-1220	11	61	23			FAQ 11.1: "We know, for example…" -- who is "we" referring to? The Chapter authors, the IPCC, the climate modelling community? Suggest to avoid personal nouns and to rephrase as, e.g., "It is known, for example" [Thomas Stocker/ WGI TSU, Switzerland]	'we' has been avoided
11-1221	11	61	27	61	28	It seems to me that internal variability -usally highly chaotic- is an obstacle for prediction. The fact that some variability is less chaotic or has a longer predictability time is very good news. But I would not say that internal variability can help us to predict. [Ramon de Elia, Canada]	This issue is now clarified by insertion of 'Some types of' in "Some types of naturally occurring so-called 'internal' variability can – in theory at least – extend the capacity to predict future climate"
11-1222	11	61	37	61	37	typo "diminish the further the" [Aneesh Subramanian, India]	This is not a typo.
11-1223	11	61	38	0	0	"in case you are wondering" Can we be a bit less casual perhaps? [Antje Weisheimer, United Kingdom]	term dropped
11-1224	11	61	38			FAQ 11.1: Please reword the sentence beginning "In case you are wondering.....". This currently is a much too informal and casual style of writing. [Thomas Stocker/ WGI TSU, Switzerland]	term dropped
11-1225	11	61	50	61	50	What does "though imperfect" means? Is the statistical significance imperfect? [Government of United States of America]	sentence is accurate.
11-1226	11	61	51			p. 11-61, line 51: replace "predicting" by "hindcasting" or "retrospectively predicting." Decadal prediction systems have not yet existed for 9 years, so no predictions have yet been verified. [Government of United States of America]	'hindcast' is now used and explained.
11-1227	11	61	52	61	52	"Theory indicates…" Which theory indicates that skill for temperature SHOULD be higher than for precipitation. Can a citation be provided. It is more model simulations that indicate this. [Government of United States of America]	These issues are addressed in other parts of Chapter 11. Please see 11.2.2 for discussion of the relevant (predictability) theory and Section 11.2.3.4 for discussion of the skill of both surface temperature and

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							precipitation forecasts.
11-1228	11	62	7			FAQ 11.2: We recommend that the authors consider adding a second, quantitative figure that shows the effect of volcanoes on climate. An option might be a millennium scale temperature time series showing the timing of major eruptions. [Thomas Stocker/ WGI TSU, Switzerland]	The author team has agreed to the single figure.
11-1229	11	62	9			In my view this FAQ is weak. A number of statements are made which are speculative, incorrect, or not supported by the literature. This FAQ deserves careful attention, and should require the same level of proof for the statements made and support in the literature as the rest of the assesment. [Nathan Gillett, Canada]	noted - hopefully improved.
11-1230	11	62	11	62	11	Amend "upper atmosphere" by "upper atmosphere (called stratosphere)" to show that both expressions mean the same thing (or explain the expression somewhere else) [Urs Neu, Switzerland]	noted and change made.
11-1231	11	62	17	62	17	"0.5 degrees for a year" is too much or too long (e.g. Fig. 2 of Bender et al., Climate Dynamics 2010) [Jouni Räisänen, Finland]	text moified to reflect a change up to 0.5 for as much as a year.
11-1232	11	62	18	62	19	Precipitation is not controlled primarily by the amount of available water vapour, but by the energy budget of the troposphere (or the surface energy budget can equivalently be considered). See e.g. Allen and Ingram (2002), already cited in the chapter. A simplified explanation would be that the reduced incoming shortwave at the surface is compensated by a reduction in latent heating i.e. in evaporation, and hence rainfall. [Nathan Gillett, Canada]	Text modified as suggested.
11-1233	11	62	42	62	46	Is the surface NAO/NAM response to volcanoes really robust? It isn't seen in the CMIP5 models (Driscoll et al., 2012). Driscoll, S., Bozzo, A., Gray, L. J., Robock, A., & Stenchikov, G. (2012). Coupled model intercomparison project 5 (CMIP5) simulations of climate following volcanic eruptions. Journal of Geophysical Research, 117(D17), D17105. [Nathan Gillett, Canada]	text in this regard has been removed.
11-1234	11	62	52	62	55	This seems overly speculative for an IPCC assessment. If 'several studies failed to prove a connection' then is this really robust? [Nathan Gillett, Canada]	Speculative text has been removed.
11-1235	11	62				FAQ 11.2. The following paper discussed how volcanic eruption possibly affects the predictability of natural variability. Shiogama H., Emori S., Mochizuki T., Yasunaka S., Yokohata T., Ishii M., Nozawa T., Kimoto M.(2010) Possible influence of volcanic activity on the decadal potential predictability of the natural variability in near-term climate predictions.Advances in Meteorology. Vol 2010, Article ID 657318, doi:10.1155/2010/657318. http://www.hindawi.com/journals/amet/2010/657318/ [Hideo Shiogama, Japan]	noted thank you.
11-1236	11	63	1	63	2	FAQ 11.2: We consider the 'Frankenstein" sentence to be of limited relevance to this FAQ, and we therefore suggest removing this sentence. [Thomas Stocker/ WGI TSU, Switzerland]	removed.
11-1237	11	63	10	63	16	FAQ11.2: 1. These lines do not seem to be quite consistent with descriptions of the LIA and the contributing factors in Ch. 5 section 5.3.5, where contributions from solar forcing and internal variability are also mentioned as having contributed to the LIA. In particular, Figure 5.8a shows the LIA as spanning the period 1400-1700 whereas here, the text implies the LIA began with these large volcanic eruptions beginning in 1258 and extending through the next 40 years (so to about 1300). The next statement that the 1452 CE Kuwae eruption perpetuated this cooling is puzzling as this eruption occurred about 150 years after the other four? And finally, the final statement that" the climate did not warm again until greenhouse gases from human activities became the dominant cause of climate change in the past century" implies there was no climate warming until the second half of the 20th century (given IPCC conclusions in this report and in the AR4 that anthropogenic influence became dominant in the 2nd half of the 20th century). Ch.5 (page 22) refers instead to the LIA as a period "that lasted from the middle centuries of the millennium to the rise in global temperatures that began in the late 19th century." [Government of Canada]	Text modified so as not to create confusion with the little ice age and IPCC statements in AR4 or AR5.
11-1238	11	63	10	63	16	"the impacts of consecutive large eruptions can last longer" Another period of interest here would be the three large, and several lesser, volcanic events during 1963-1991. From Fig 8.15, the AOD during 1985-1993 ranged 0.050 to 0.150, decreasing to 0.01 to 0.02 since 1995. The climate response due to volcanoes alone since 1963 should be on the order of 0.1 to 0.2C; what do the CMIP models say of the volcanic contribution to	Suggestions included in revised FAQ.

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						the warming ? [Richard Keen, United States of America]	
11-1239	11	63	10	63	16	"The 1452 CE Kuwae eruption perpetuated this cooling, and the climate did not warm again until greenhouse gases from human activities became the dominant cause of climate change in the past century." Having determined that volcanoes affect climate for 2 years, it is now implied that this one volcano had a 400-year cooling effect? Is the John Eddy solar theorywritten off so completely? [Richard Keen, United States of America]	sentence removed.
11-1240	11	63	10	63	16	It should be mentioned that solar forcing also had an influence on cooling of the Little Ice Age, e.g. amend "The 1452 CE Kuwae eruption perpetuated this cooling" by "The 1452 CE Kuwae eruption together with lower solar activity perpetuated this cooling" [Urs Neu, Switzerland]	sentence removed.
11-1241	11	63	10		16	This buys the Millar paper lock stock and barrel. It's a hypothesis - an interesting one but chapters 5, 9 and 10 assess other contributors to the LIA. Also, the 1258 eruption did actually not cause that much of a cooling in reconstructions. This para needs synchronizing with chapters 5 and 10. [Gabriele Hegerl, United Kingdom]	paragraph has been completely reworked.
11-1242	11	63	11	63	15	The 13th century is during the Medieval Warm Period, not the Little Ice Age. According to the glossary the MWP extends from 900 to 1400, and the LIA from 1400 to 1900. This is completely wrong. [Nathan Gillett, Canada]	paragraph has been completely reworked.
11-1243	11	63	13	63	16	Can we really attribute the whole Little Ice Age to a series of a few individual eruptions? [Jouni Räisänen, Finland]	paragraph has been completely reworked.
11-1244	11	63	13	63	16	FAQ 11.2: The link that is made here between volcanoes and the Little Ice Age seems considerably stronger than what is supported by the literature and Chapter 10 assessment, and does not seem to reflect the scientific uncertainty that remains surrounding the cause of the LIA. Chapter 10 refers to the LIA cooling as resulting from a combination of Volcanic, Solar, and Greenhouse Gas forcing, but this combination is not reflected in the wording that is used here. [Thomas Stocker/ WGI TSU, Switzerland]	paragraph has been completely reworked.
11-1245	11	63	15	63	16	Omit the last part ", and the climate did not warm again until greenhouse gases from human activities became the dominant cause of climate change in the past century" and replace by "until the 19th century". Reason: In my opinion this is not true. Greenhouse gas forcing can only be said to be the dominant cause of warming after the mid 20th century. Warming after the end of the Little Ice Age started much earlier and is strongly caused by the absence of volcanic cooling and by the increase in solar activity after the Maunder Minimum. [Urs Neu, Switzerland]	agreed. Paragraph has been completely reworked.
11-1246	11	63	18	63	22	FAQ 11.2: "We can…", "our ability to project", "our climate predictions...", "we are" -- who is "we" referring to? The Chapter authors, the IPCC, the climate modelling community? Suggest to avoid personal nouns and to rephrase as, e.g., "It is possible", … [Thomas Stocker/ WGI TSU, Switzerland]	corrected.
11-1247	11	63	20	63	22	how do you evaluate the time of the response into 2 years? [Annalisa Cherchi, Italy]	This can be donw with controled numerical experiments, but this discussion is beyond the scope of this FAQ.
11-1248	11	63	25	63	28	Uncertainty is dramatically understated here. The text says that we are confident that a large eruption would cause a global cooling lasting two years - no probability assessment is given. I agree that this is the expected response, but it isn't 100% certain that an eruption would be followed by global cooling. For example, Krakatoa, a large reaction, was not followed by global cooling - see e.g. Joshi et al. (2009). The text says that based on simulation of recent eruptions we are confident that a large tropical eruption would drive winter warming of NH continents for one or two years, again with no probability assessment. But no increase in the NAO and no Eurasian winter warming at all is simulated in the CMIP5 models (Driscoll et al., 2012). How can we be so confident that this response is robust? What is missing in the CMIP5 models which prevents them from simulating this effect? Personally, I think this finding calls into question whether the observed response, derived based on a limited sample of volcanoes, is really robust, but certainly the statement that we have high confidence in this projection based on simulation of the response to recent erurptions is not supported. Joshi, M. M., & Jones, G. S. (2009). The climatic effects of the direct injection of water vapour into the stratosphere by large volcanic eruptions. Atmos. Chem. Phys, 9, 6109-6118. Driscoll, S., Bozzo, A., Gray, L. J., Robock, A., & Stenchikov, G. (2012). Coupled model intercomparison project 5 (CMIP5) simulations of climate	We agree the text is too certain. We also note that this is an FAQ so details of uncertainty are limited. Nevertheless the certainty of this statement has been significantly reworked. Staements about impact on monsoons and the like have been removed.

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						following volcanic eruptions. Journal of Geophysical Research, 117(D17), D17105. [Nathan Gillett, Canada]	
11-1249	11	63	26	63	26	what do you mean by large? [Annalisa Cherchi, Italy]	large here is definitely qualitative - as appropriate for an FAQ 11.2
11-1250	11	63	26	63	30	how do you know? it would depend on the amounts of particles ejected [Annalisa Cherchi, Italy]	text modified so comment no longer an issue.
11-1251	11	63	36	63	36	Amend "do not include volcanic eruptions" by "do not include supposed volcanic eruptions" or something alike, for being clear. [Urs Neu, Switzerland]	modified as suggested.
11-1252	11	63	42	63	42	Amend "But they cannot test all the mechanisms involved in global warming over the next century, because they involve long term oceanic feedbacks" by "But in this way not all the mechanisms involved in global warming over the next century can be validated, because these involve long term oceanic feedbacks, which...". Reason: Original sentence is grammatically incorrect (relation of both 'they') [Urs Neu, Switzerland]	modified as suggested.
11-1253	11	64	47	64	48	Use small cases for this reference [Xiaolan Wang, Canada]	corrected.
11-1254	11	66	32			"Submitted" would be "39, L21710, doi:10.1029/2012GL053901" [Yoshimitsu Chikamoto, United States of America]	corrected.
11-1255	11	67	43	67	46	The two reference items seem to be the same paper. [Gan Zhang, United States]	corrected.
11-1256	11	67				TFE.9 Table 1Chapter 11 lacked assessment of drought. This gap propagated into the Technical Summary Table 1 where "Increases in frequency and/or intensity of drought" is "Not assessed" when it comes to "Likelihood of future trends based on projections for the next few decades". Even an assessment of 'Low confidence' would be of more value than "Not assessed". [Government of United States of America]	noted.
11-1257	11	68	27	68	27	Please add missing paper ID and doi: "L05707, doi:10.1029/2010GL042710". [Georg Feulner, Germany]	corrected.
11-1258	11	69	37	69	37	After “Reviews of Geophysics” add “50, RG3005, doi:10.1029/2012RG000388”. [Paul Ginoux, United States of America]	corrected.
11-1259	11	70	46			Repeated reference to Hawkins & Sutton 2011 [Ed Hawkins, United Kingdom]	corrected.
11-1260	11	73	11	73	12	Use small cases for this reference [Xiaolan Wang, Canada]	corrected.
11-1261	11	73	13	73	16	Knutson et al 2012a and 2012b are the same [Fabrice Chauvin, France]	corrected.
11-1262	11	75	43	75	43	The Massonnet reference is a funny mixture of two references, apparently. Here the correct citation: Massonnet, F., T. Fichefet, H. Goosse, C. M. Bitz, G. Philippon-Berthier, M. Holland, and P. Y. Barriat, 2012: Constraining projections of summer Arctic sea ice. The Cryosphere. submitted. [François Massonnet, Belgium]	corrected.
11-1263	11	75	43	75	43	citations of "Massonnet" and "Matei" are mixed up [Holger Pohlmann, Germany]	corrected.
11-1264	11	75	43	75	44	Massonnet and Matei et al. 2012a references have been mixed-up [Daniela Matei, Germany]	corrected.
11-1265	11	80	39	80	40	This references is wrong and basically a duplicate of the correct citation of this reference in line 45-46 [Jana Sillmann, Canada]	corrected.
11-1266	11	84	42	84	42	This reference should be Wild M., and Leipert [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	corrected.
11-1267	11	84	43	84	43	This reference should be removed (duplicates 2012 reference) [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	corrected.
11-1268	11	84	44	84	44	Paper now published: Atmos. Chem. Phys. 12, 2037-2054, 2012. [Oliver Wild, United Kingdom of Great Britain & Northern Ireland]	corrected.
11-1269	11	87	3	87	4	Please add the information FGOALS-g2 (one of CMIP5 model, whose initialized predictions were submitted through the Earth System Grids) to Table 11.1, and include the results of of this model in the ensemble-mean initialized predictions.	Table 11.1 has been updated to include all the models that participated in the CMIP5 decadal experiment, by contacting modeling groups. Information on FGOALS-

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						The detail information of FGOALS-g2 for near-term experiment is as follows: 1) CMIP5 Near-term Players / name of modelling center (or group): LASG, Institute of Atmospheric Physics, Chinese Academy of Sciences, and CESS, Tsinghua University; 2. CMIP5 official model-id: FGOALS-g2; 3. AGCM: 2.8L26; 4. OGCM: 1.0L30 (0.5 in latitude over Tropic); 5. Initialization / Atmosphere/Land: No; Ocean: SST, T&S (Ishii et al, 2006); Sea Ice: No; Anomaly assimilation: No; 6. Perturbation: perturb the ocean with Dynamic Bias Correction (Wang et al, 2012, refer to Comment No.2). [Bin Wang, China]	g2 is now available on the table.
11-1270	11	87				Table 1 lists the models that entered the CMIP5 near-term experiments, yet it does not include all the models. In particular it does not include the NASA/GMAO contribution (GEOS-5) or the COLA-CFS. These models should be included in the list and the list should be reviewed to see if any other models are missing. The text should state what subset of model results are used and why. Some Table entries are blank. Three of our reviewers noted this issue. [Government of United States of America]	Table 11.1 has been updated to include all the models that participated in the CMIP5 decadal experiment, by contacting modeling groups. Information on GEOS-5 and COLA-CFS is now available on the table.
11-1271	11	87				The description of the MPI-M decadal prediction system in table 11.1 is incorect with respect to the model resolutions. The model was run in two set-ups: MPI-ESM-LR (1.9L47 in the atmosphere and 1.5L40 in the ocean) and MPI-ESM-MR (1.9L95 in the atmosphere and 0.4L40 in the ocean). [Daniela Matei, Germany]	Table 11.1 has been updated to include all the models that participated in the CMIP5 decadal experiment, by contacting modeling groups. The MPI entries have also been double-checked by the MPI modelers.
11-1272	11	87				In Table 11.1 for MPI-M Model reolution is: (a) MPI-ESM-LR AGCM 1.8L47 OGCM 1.5L40 (b) MPI-ESM-MR AGCM 1.8L95 OGCM 0.4L40 [Wolfgang Müller, Hamburg]	Please see the response to 11-1271.
11-1273	11	87				Table 1: The entry of the Max Planck Institute for Meteorology is wrong. The model is run in two resolutions: MPI-ESM-LR atm 1.8L47 ocean 1.5L40 & MPI-ESM-MR atm 1.8L95 ocean 0.4L40 [Holger Pohlmann, Germany]	Please see the response to 11-1271.
11-1274	11	89	2		2	In Figure 11, distinguish colors of historical data (until 2000) with the data obtained by simulation [Ibouraïma YABI, Benin]	done.
11-1275	11	89	5	89	5	enlarge the period 2000-2015 to increase clarity of the graph, indicate volcano forcing directly in the graph [European Union]	The graph has been redrafted and the example forecast is now initialized at an earlier date which, we hope, clarifies the graph.
11-1276	11	89	10	89	10	Should read "The grey areas along the X-axis" [Government of Canada]	Yes
11-1277	11	89		91		figures 11.2 and 11.a don't give sources, models, datacitations etc.would be good to know. Figure 11.3 not sure what the vertical axis shows. Figure 11.5 doesn't look very impressive…not sure what its trying to show [Gabriele Hegerl, United Kingdom]	Fig 11.5 has been redrawn and now shows the results for each forecast system using a different colour. It also looks simpler by choosing simulations for one start date every ten years. The figure is not expected to look impressive but rather to illustrate a couple of points with which most usual readers of the report might not be familiar: the importance of the drift in climate predictions and the relevant size of the systematic errors, which is comparable to that found for the historical runs used as a basis to interpret the projections.
11-1278	11	89		126		Many captions do not have references to papers. Results in chapter should be based on the published literature. If the figures themselves haven't been published, then they should at least be updates of published figures based on new model simulations, in which case the original studies should be cited. Perhaps more papers will be available to cite by the time the TOD is written. [Nathan Gillett, Canada]	Figures are consistent with IPCC policy. References are given where appropriate. Basic analyses of CMIP5 model output is alllowed.Additional explanatory material is provided in caption when needed.
11-1279	11	89		129		Many figures in Chap. 11 have apparently been generated from the CMIP5 database and have not been included in the peer-reviewed literature, as there are no references given in the figure captions. This seems to be contrary to the policy applied to referring to publications. We suggest that only figures that have appeared	Figures are consistent with IPCC policy. References are given where appropriate. Basic analyses of CMIP5 model output is alllowed.Additional

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						in the peer-reviewed literature be included in the body of the report. Materials that have not been through the peer-review process should be moved to supplementary material and should include a description of methodology and data sufficient to reproduce the results. [Government of United States of America]	explanatory material is provided in caption when needed.
11-1280	11	89		129		Figure quality: In many of the figures, the stippling and crosshatching make the figures difficult to interpret. Gradations in the colors underneath are impossible to make out, even when the figures are zoomed. [Government of United States of America]	The figures have been improved in this regard.
11-1281	11	89				Comment on Fig.11a: I do like this type of introductory figure but the provided one could be misleading because the chosen prediction in 2007 is cooler than the projection, which is happened to be true in reality. The reader may think that predictions are "correcting" the projection towards colder states. In addition to the 2007 prediction, I would add a new set at the beginning of the period (to avoid overlap and to have a clear picture) chosen to be warmer that the projections. I is true for prediction in the late 1960's or realy 1070's. In addition, the name of the model used for this figure should be provided in the legend. [Christophe CASSOU, France]	The graph has been redrafted and the example forecast is now initialized at an earlier date to avoid this.
11-1282	11	89				Fig 11.1a What model was used for the simulations shown? (CMIP5 models?). Is there a reference for this plot? Somewhere in the text it might be worth commenting on the fact that all the intialised simulations show a cooling (as was also observed). Is this related to ENSO? [Nathan Gillett, Canada]	Figure 11.1a, although based on an actual model, is meant to be a schematic to illustrate the various concepts explained in the box. This is why the model is not referenced. The example forecast is now initialized ant an earlier date in order to avoid questions about the warming "hiatus" which is treated in another Box.
11-1283	11	89				Is Figure 11.1a just a schematics like Figure 11.1b or an example from a collection of model runs? Please clarify. [Government of United States of America]	Figure 11.1a, although based on an actual model, is meant to be a schematic to illustrate the various concepts explained in the box. The caption now states that the Figure is a schematic.
11-1284	11	90				fig. 1b Although this figure is schematic, it would still be useful to provide some sense of what sort of time scale is being schematicized. Is this the next couple of decades or the next hundred years? [Government of United States of America]	This is not easy to characterize in a simple way since the timescale depends on the particular variable and location considered. Nevertheless, we now refer to the schematic evolution of a "decadal" forecast in order to give some sense of the timescale.
11-1285	11	91	1		1	Learn the legend of the vertical axis (Y) [Ibouraïma YABI, Benin]	As noted in the title, it is the globally averaged correlations skill
11-1286	11	91				Fig 11.2. Define 'local correlation skill score' and 'corresponding predictability measure', so that these are easily understandable to non-speecialists. What model was used? Is there a reference? [Nathan Gillett, Canada]	The caption has been rewritten.
11-1287	11	91				Are the results presented in Figure 11.2 from the CMIP5 multi-model ensemble? Please specify. [Daniela Matei, Germany]	No they are an example from a particular model which is now cited in the caption.
11-1288	11	92	2		2	Why the difference in scales of years on the x-axis. On the top figure is not the 2 years while the bottom figure is not the 1 year [Ibouraïma YABI, Benin]	This figure no longer appears.
11-1289	11	92				Figure 11.3. More explanation is needed. First clearly explain what the diagnostic being shown is - is this the correlation between forecasts of the MOC strength and observations? (if so where do the obs come from?), or is this a perfect model result? Second what does 'results for perfect predictability of the MOC based on de-correlation time' mean? Are these perfect model forecasts? How can you assess the predictability from the de-correlation length time? The black curve is labelled 'GFDL perfect predictability' - do the other curves also show 'perfect predictability' or is this different? If it is perfect why is the anomaly correlation lower than for some of the other forecasts? In panel b, what is 'mean squared distance' (from the forecast to the observations - from the original state to subsequent states?). What does 'PC1-10 MSD' (the y-axis label) mean? This may all be obvious to a specialist, but as noted in my general comments, this topic is new to IPCC assessments, so I think it would be helpful to explain in more detail. [Nathan Gillett, Canada]	This figure no longer appears.

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11-1290	11	92				Figure 11.3 Top Panel: Change MOC Predictability to MOC Potential Predictability since these are results from "perfect model studies" [Daniela Matei, Germany]	This figure no longer appears.
11-1291	11	93				fig. 4 What is the metric being mapped? Please be somewhat specific in the caption. [Government of United States of America]	This figure has been modified. The potential predictability variance fraction referred to in the caption is now defind explicitly in the text with a simple formula to make the quantity specific.
11-1292	11	94	0	0	0	Fig 11.5 is very confusing - too many lines and similar colours [Government of United Kingdom of Great Britain & Northern Ireland]	We believe this figure is very important. It has been redrawn to respond to several comments. The purpose of this figure is to illustrate both model drift and systematic error. These are important features of climate predictions, and at the same time show how anomalies are obtained. Besides, it is one of the few figures in the chapter that shows the relative size of the anomalies to be predicted with respect to the systematic error of the different models.
11-1293	11	94				fig. 5 Are these "full initialization" or "anomaly initialization" or some mix? [Government of United States of America]	All systems are included.
11-1294	11	94				Fig 11-5: The figure title indicates SAT instead of SST [RYM MSADEK, United States of America]	The figure has been redrawn. It now reads SST.
11-1295	11	94				Fig11.5: What is shown here, SAT (headline) or SST (caption)? [Holger Pohlmann, Germany]	The figure has been redrawn. It now reads SST.
11-1296	11	95				Fig. 11.6: 1-sided T and F tests should be used to test if initialization improves forecasts, not 2-sided test, which test just whether initialization causes differences of either sign. [Government of United States of America]	It was decided to use a 2-sided test because it is important to detect the regions where initialization could degrade the skill. These regions require special care when interpreting any results.
11-1297	11	95				Fig 11-6: The colored lines defined in the caption and on the figure are not consistent [RYM MSADEK, United States of America]	This has been corrected. Only one example of the multi-model, the one with start dates every five years, is now shown.
11-1298	11	96	5	96	6	Figure 11.7: Is the correlation between CMIP5 Init models and observation? Are they based on annual mean time series? [Government of United States of America]	The figure has been simplified and now only shows the RMSSS. As the caption explains, the scores are computed between the CMIP5 multi-model ensemble mean and an observational reference using four-year means.
11-1299	11	96	10	96	11	The caption needs to say what the sign of the Z difference means - i.e. do the initialised forecasts do better where the difference is positive or negative? (Same comments applies to Figure 11.9 and Fig 11.10a). [Nathan Gillett, Canada]	The correlation has been removed from the figures to simplify the message.
11-1300	11	96				Fig 11.7 I think some work is needed to make this figure easier to understand. First are both the correlation skill score and RMS needed? RMS and the correlation measure were not discussed separately in the text. I suggest picking one measure. Second, add panel labels, so that the panels are interpretable at a glance i.e. '2-5 years' on the left and '6-9 years' on the right. Add a simple description of what is shown after the first sentence of the caption, such as 'The top row shows where the climate forecasts have significant skill compared to an assumption of climatology (dotted regions), and the second row shows where the intialisation makes a significant contribution to that skill (positive regions enclosed by a black contour). Skill in other regions arises from variations in external forcing.' The same comment applies to Figures 11.9 and 11.10a. [Nathan Gillett, Canada]	Showing multiple scores is important because forecast quality is a multi-faceted aspect of climate predictions. Correlation and RMSE inform about different aspects of the skill of a system. However, to simplify the message the figure now shows only RMSE-based metrics. The reader is referred to the literature to learn more about the other metrics. Panels have labels and titles now.
11-1301	11	96				Fig 11.7 Contrary to what is said in the caption (lines 12-14 and 17-19) there are no dotted regions in the second and fourth rows of panels. Is this a mistake, or are there no regions where the initialisation significantly improves the forecast? Even under the null hypothesis (i.e. no increase in skill from initialisation), we would expect 10% of the map to be dotted by chance. Is there something wrong with the test applied? [Nathan	There are some regions with dots in current version of Figure 11.6b.

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						Gillett, Canada]	
11-1302	11	96				Figure 11.7 and several other figures mention that dots are used to show agreement, but dots are just not visible. Perhaps in this draft version the figures are of lower quality. [Government of United States of America]	There are some regions with dots in current version of Figure 11.6b.
11-1303	11	96				Figure 11.7: The color range from -100 to 100 does not seem to be standard values for correlation (-1 and 1). [Government of United States of America]	Correlation has been removed from figures 11.6 and 11.8.
11-1304	11	96				Figure 11.7, 11.9 and 11.10a. These figure are quite hard to read; can titles for each pannel be added at the very least? [Jonathan Robson, United Kingdom]	Titles have been added and the figures have been heavily simplified.
11-1305	11	97	9			fig. 8 What does "sharpness" mean in this caption and in text? [Government of United States of America]	The sharpness is the variance of the forecast probabilities and measures the ability of the system to issues probability forecasts different from the naive climatological probability of the event.
11-1306	11	98				These plots all look like a mixture of red and blue with no clear patterns. Can a global significance test be applied? If there is no global significance here, then perhaps the plot is not worth showing. Global significance could also be reported in the text. [Nathan Gillett, Canada]	Global significance is not relevant to these figures because each region has completely different dynamical reasons to show or not improvements with the initialization.
11-1307	11	98				fig. 9 Can't see contours in the maps. Are the only contours in the Canadian archipelago? [Government of United States of America]	There are contours over the eastern Mediterranean. The lack of large areas with contours is due to the more local nature of precipitation when compared to temperature.
11-1308	11	99				The fourth row is not described in the caption. [Nathan Gillett, Canada]	The fourth row is now described in the caption.
11-1309	11	99				Figure 11.10a.This figure is inconsistent with other map figures in terms of colors and projections. [Government of United States of America]	This figure has been simplified. It only shows the spread-to-RMSE ratio.
11-1310	11	99				Fig. 11.10a: We suggest changing “air temperature forecast quality” to "quality of air temperature forecast" for clarity. [Government of United States of America]	Done.
11-1311	11	99				fig. 11.10a The caption does not say what row 4 is. [Government of United States of America]	Corrected.
11-1312	11	99				Fig 11-10a: The last row of the figure is not defined in the caption [RYM MSADEK, United States of America]	Corrected.
11-1313	11	99				Figure 11.10a. Part of the colorbar is invisible. [Gan Zhang, United States]	Corrected.
11-1314	11	100	1		1	I suggest that the figure numbering begins with a instead of d. [Ibouraïma YABI, Benin]	The figures have been reorganized.
11-1315	11	100	4	100	4	Why is this 10b, and the next 10c. [Noel Keenlyside, Norway]	These figures have been removed.
11-1316	11	100		101		Figure 11.10b and 11.10c. How were the forecasts initialised at the end of 2012 if the SOD was finalised in August? Add references. [Nathan Gillett, Canada]	This was a placeholder for a figure that would use the set of decadal predictions initialized the closest in time to the IPCC deadlines. However, the figures have now been removed.
11-1317	11	100				fig. 11.10b Not clear how one can observe the temperatures of the future (2013-2017). Two of our reviewers noted this issue. [Government of United States of America]	This figure has now been removed.
11-1318	11	100				Figure 11.10b: What observation is used in this figure? Figure caption does not explain what the panels (k) - (o) show. [Government of United States of America]	This figure is now removed. It was a placeholder for the set of decadal predictions initialized in 2012.
11-1319	11	100				Fig 11-10b: I do not understand the figure caption. How can there be observed anomalies for the period 2013-2017. I guess there is a typo and these are only the forecast anomalies over than period.The caption refer to panels a) j) but it is not consistent with the figure that shows panels named from d) to o). It is also not clear what the last row shows and why there are data only over part of the globe. [RYM MSADEK, United States of America]	This figure was a placeholder for the decadal predictions to be initialized in 2012. However, it has now been removed.

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11-1320	11	101	5	101	5	the forecasts used within the decadal exchange exercise have been started near the end of 2011! (and not 2012) [Daniela Matei, Germany]	This figure was a placeholder for the decadal predictions to be initialized in 2012. However, it has now been removed.
11-1321	11	101	23			Annex II, page AII-1, line 23: add references to Ch1 and Ch12, Section 12.3 (and perhaps also Ch11) where the RCPs are being introduced and where they are primarily discussed in the WGI AR5. The reference to Chapter 12, section 12.3 should be repeated on lines 34ff where the difference in IAM-derived emissions and ESM-derived emissions is mentioned [Thomas Stocker/ WGI TSU, Switzerland]	Yes, this has been done. The reference to the Section appears later as this section only lists the chapters. The sections in 6, 11, and 12 are noted in this later paragraph.
11-1322	11	101	29			Annex II, page AII-1, line 29: "RCP emission scenarios" -- delete emissions as RCPs are concentration pathways. [Thomas Stocker/ WGI TSU, Switzerland]	Yes and no. The RCPs are truly emissions based and then use a simple model to give "concentrations" or abundances. The RCPs really start with activity and map it onto emissions and then RF. The statement was reworded to "RCP scenarios for emissions"
11-1323	11	101	31			Annex II, page AII-1, line 31-32: delete reference to website as this is not a proper citation. [Thomas Stocker/ WGI TSU, Switzerland]	Have deleted web site from Introduction, but there are some sites given in the table notes. They are not used a peer-reviewed documents, but as locations. I think this is appropriate as the full numbers never appear in the papers. Where forexample are ALL the CMIP5 data?
11-1324	11	101	33			Annex II, page AII-1, line 33: "this assessment" -- suggest to add reference to specific Chapter(s) (and sections if appropriate) [Thomas Stocker/ WGI TSU, Switzerland]	Yes, done.
11-1325	11	101	34			Annex II, page AII-1, line 34: "RCP anthropogenic emissions" -- change to "inferred RCP anthropogenic emissions" [Thomas Stocker/ WGI TSU, Switzerland]	This has been rewritten differently: "Present-day natural and anthropogenic emissions of CH4 and N2O are assessed and used to scale the RCP anthropogenic emissions to be consistent with these best estimates (Chapters 6, 11)."
11-1326	11	101				Could these results be explained just by the initiialisation correcting biases in many of the models? For example if a model is warm compared to obs in 2012, initialising it will presumably cool the forecast. But this may not be telling us that there is true deterministic predictability in the system. Would significant differences remain if the mean unitialised model states were centred on the observations? Add some discussion in the text. [Nathan Gillett, Canada]	This is a difficult question for which we can at this stage only speculate. It would be difficult to justify centering the mean uninitialised model state on the observation because this would just be a linear correction.
11-1327	11	101				Fig. 11.10c: As presnted, we feel this is misleading. The “unitialized” forecasts have initial conditions when viewed as forecasts. Please show also the difference between the initialized and “uninitialized” forecasts at the initialization time for the initialized forecasts. The comparison of initialized and uninitialized simulations should have this difference subtracted. [Government of United States of America]	This figure has been removed.
11-1328	11	102				In the caption to the top left panel, 'Historical uncertainty' should be labeleed something different. 'Historical uncertainty' sounds to me like uncertainty in observed temperature over the historical period, but this is not what is meant. Perhaps 'Historical model spread' or similar, and explain in caption. [Nathan Gillett, Canada]	Accepted. Figure has been amended.
11-1329	11	102				Fig. 11.11a: Is the internal variability band just an extension of the historical internal variability estimate draped along the projected trend? What happens if the internal variability spread (grows) with GHG forcing? Would that even have been detected in the experiment being illustrated? [Government of United States of America]	Internal variability is estimated from the models assuming no growth in variability with GHG forcing. This is an idealisation. The caption has been amended to explain these points (see Hawkins and Sutton, 2009 for further discussion).
11-1330	11	103	1	103	2	Figure 11.12(b). Are observations from 2006-2012 are missing from panel b? It is unclear how decadal mean observations were calculated. If it is a running 10 year mean, then shouldn't values for 2006-2012 be available? [Government of Canada]	Accepted. Panel b does show running 10 year means, and has now been updated to include 2006-12.
11-1331	11	103	4			Figure 11.12 is excellent! Congratulation. It would be extremely helpful to have this figure for the continent as well. For example showing a map with projected changes as e.g. in Figure 11.31a or as in the summary for	Thank you for the positive comment. Unfortunately it is not possible to include all the figures that would be

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						policy maker SPM.4 [Christof Appenzeller, Switzerland]	useful. Further information about regional changes is presented in Chapter 14 and in the Atlas (Annex I).
11-1332	11	103	4			Figure 11.12 is excellent! congratulation. It would be extremely helpful to have the same figure for projected changes in precipitation as well or if possible for the continents as well. For example showing a map with projected changes as e.g. in Figure 11.31a or as in the summary for policy maker SPM.4. [Christof Appenzeller, Switzerland]	Thank you for the positive comment. Unfortunately it is not possible to include all the figures that would be useful. Further information about regional changes is presented in Chapter 14 and in the Atlas (Annex I).
11-1333	11	103				Figure 11.12 b): Are these decadal means calculated over moving time windows? [Government of United States of America]	Yes. This is now explained in the caption.
11-1334	11	104	5	104	6	Replace 'natural internal variability in the quantitiy plotted in the left panels' with 'interannual standard deviation of surface air temperature' (or whatever is shown). [Nathan Gillett, Canada]	Revised caption provides more details
11-1335	11	104	9			Differ significantly from the control? Or from the simulated 1980-2005 mean? [Nathan Gillett, Canada]	From the simulated 1986-2005 period. Caption has been clarified.
11-1336	11	104		113		For all figures using CMIP5, would be good to give the number of ensemble members used in each case [Clare Goodess, United Kingdom]	The number is included at the top-right of the panels, additional explanation is now provided in the caption.
11-1337	11	104				Fig 11.13: Does the colorbar represent the full data range or are there values outside the range of the colorbar? In that case, the outer rectangles should be triangles or anything that indicates "open ends". [Boris Orlowsky, Switzerland]	The color bar includes the full data range.
11-1338	11	105				fig. 14 Middle row, right panel: How does this result compare to Barnett et al (2008, Science, Vol. 319 no. 5866 pp. 1080-1083 DOI: 10.1126/science.1152538) who showed detectability by late 20th Century? [Government of United States of America]	This figure shows projected future changes relative to a recent reference period (1986-2005); in contrast Barnett et al focussed on past changes relative to an earlier reference period.
11-1339	11	105				Mahlstein et al (GRL, 2012 using observations, used in ch10) shows that the Hawkins and Sutton approach is quite conservative when it comes to emergence. Maybe worth discussing emergence carefully and using all available literature. Also, a tricky topic that needs to coordinate well with chapter 10, as we already have emergence in many gridpoints in observations, so its not all in the future! [Gabriele Hegerl, United Kingdom]	Accepted. Discussion in text will be clarified.
11-1340	11	106	1	106	1	What does the number (38) in the upper right corner of each plot stand for, the number of models used? Please clarify. This occurs in many other plots in this chapter. [Xiaolan Wang, Canada]	The number included at the top-right of the panels is the number of the models available for analysis. This changes from figure to figure, depending upon availability of data. The captions have been improved, but Fig.11.15 has been deleted
11-1341	11	106	1		1	What do the points in the figure? [Ibouraïma YABI, Benin]	The stippling indicates statistical significance. The captions have been improved, Details for stippliing and hatching are provided in Fig.11.13 (SOD number).
11-1342	11	106	6			Annex II, Table AII.1.3, line 6: historical global mean surface air temperature is only including HadCRUT4. What about the other records assessed in Chapter 2? Why is this particular data set chosen over any of the others (or a combination)? Please explain. [Thomas Stocker/ WGI TSU, Switzerland]	Yes this has been corrected.
11-1343	11	106	6			Annex II, Table AII.1.3, line 6: historical global mean surface air temperature is provided relative to the 1961-1990 reference period. But the projections chapter use 1986-2005 as their reference period. We suggest to either add the difference between the 1961-1990 and 1986-2005 to allow direct comparison or to change the reference period to what is used as the standard in WGI AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Yes, this has been done with the new tables in Anneex II
11-1344	11	106				Fig 11-15: I do not know what the number 38 that appears on the upper right corner of each panel correspond to. I guess it is the number of models considered. This couls be added in the caption. Same for Fig 11-13, Fig 11-16, Fig 11-18 [RYM MSADEK, United States of America]	Yes, this is the number of models, more information is now provided in the captions.
11-1345	11	106				Fig 11.15: Does the colorbar represent the full data range or are there values outside the range of the	The color bar includes the full data range.

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						colorbar? In that case, the outer rectangles should be triangles or anything that indicates "open ends". [Boris Orlowsky, Switzerland]	
11-1346	11	107	1	107	2	Figure 11.17. Are models and internal variability being fairly compared here? For models, the 5-95% range covers 90% of the distribution, for internal variability, +/- 1 SD covers 68% of the distribution. Thus, in the figure, internal variability appears less than what it actually is. Suggest +/- 2SD for internal variability. [Government of Canada]	The figure has been changed, it now shows one standard deviation of naturak variability on top of the 17%-83% range of model spread, which for a normally distributed distribution should correspond to each other. We decided to keep percentiles for the model spread as these are used throughout the chapter.
11-1347	11	107				Fig 11.16: Does the colorbar represent the full data range or are there values outside the range of the colorbar? In that case, the outer rectangles should be triangles or anything that indicates "open ends". [Boris Orlowsky, Switzerland]	Taken into account - it will be checked
11-1348	11	108	7	108	7	What is the bin width used in the box plots? [Xiaolan Wang, Canada]	Accepted - Fig. 11.17has been changed and box plots have been omitted
11-1349	11	108				Fig 11.17. Add the zero line to the plot. Also, are the projected changes calculated from ensemble means for each model, or from single simulations? [Nathan Gillett, Canada]	Accepted - the zero line has been added more clearly.
11-1350	11	109				Fig 11.18 Right hand panels appear to be a standard deviation and are labelled 's.d.'. If so, how can the standard deviation be negative, as in the top right panel? [Nathan Gillett, Canada]	Taken into account - This figure was replaced by a new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%). The number of CMIP5 models used wil be indicated in the upper-right corner of each panel.
11-1351	11	109				Fig 11.18, top right panel: what are the negative standard deviations over the Antarctic? [Boris Orlowsky, Switzerland]	Taken into account - This figure was replaced by a new one including CMIP5 multi-model annual mean projected changes for the period 2016-2035 relative to 1986-2005 under RCP4.5 for: (a) evaporation (%), (b) evaporation minus precipitation (E-P, mm/day), (c) total runoff (%), (d) soil moisture in the top 10 cm (%), (e) specific humidity (%), and (f) absolute change in relative humidity (%). The number of CMIP5 models used is indicated in the upper-right corner of each panel.
11-1352	11	110	6	110	6	I don't see any grey shading in this figure. [Xiaolan Wang, Canada]	Figure has been revised to show convention of hatching/stippling.
11-1353	11	110				Fig 11.19 Update with CMIP5 models and use a similar format to other plots e.g. 11.18. [Nathan Gillett, Canada]	Figure has been updated.
11-1354	11	111				fig. 11.20 The caption refers to open circles, but there are none evident. Please revise the figure or the figure caption to reflect what is currently in it. Two reviewers noted this issue. [Government of United States of America]	Open circles have been added.
11-1355	11	112	1		1	In my opinion, the results of different models are too heterogeneous and do not lead to objective conclusions. I then suggested that this figure is deleted the first time that more robust models are built. [Ibouraïma YABI, Benin]	Figure has been deleted.
11-1356	11	112	7			Fig 11.21 Are the grey lines ensemble averages? [Nathan Gillett, Canada]	Figure has been deleted.

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11-1357	11	112				Figure 11.21. The unit in the y-axis lable "SLP Gradient Anoamly (hPa)" may not be the best. The SLP gradient unit should be in the form [pressure unit] / [distance unit]; so is the SLP gradient anomaly. [Gan Zhang, United States]	Figure has been deleted.
11-1358	11	113	4	113	4	The figure caption is incorrect as in panel (c) of Figure 11.22 not “wet days” are shown, but the simple daily intensity of precipitation (SDII) is shown. Please correct or replace the panel with the time series for the indix for “very wet days” (R95p). [Jana Sillmann, Canada]	Caption has been completely revised.
11-1359	11	113	5	113	5	The time series are shown relative to the reference period 1981-2000 [Jana Sillmann, Canada]	Caption has been completely revised.
11-1360	11	113				If this figure (Figure 11.22) is included in Chapter 11, please add boxes (median, quartiles, 5/95%-iles) for the near-term. [Akio Kitoh, Japan]	The uncertainty assessment for the near-term is not available from the study (Sillmann et al. 2013), for this reason it has been removed for all periods.
11-1361	11	113				Figure 11.22: Please use updated figures from Sillmann et al. 2012 [Jana Sillmann, Canada]	Suggestion followed,
11-1362	11	114				Fig 11.23 These models appear to simulate an increase in rainfall over the Mediterranean in JJA. The CMIP5 models show a significant drying in AMJJAS over the Mediterranean. Is this an artifact of the regional model setup, perhaps due to using prescribed SSTs? [Nathan Gillett, Canada]	This is useful comment. In reply, note first that the comment merely applies to the area over sea. For most of the land-area in the Mediterranean, these simulations shown are consistent with CMIP5. Further work is underway to clarify the reason for the discrepancy.
11-1363	11	115				fig. 24 Centering the representation of model internal variability in figure 11.24 at the 1986-2005 climatological values could mislead the reader to believe that changes in ocean temperature have not already been detected and attributed to human factors. As these results about the recent past are robust, it should be explicitly noted in the text or caption that detectible changes in ocean temperature and/or heat content have already been experienced and that exceedance of the illustrated bounds of natural variability are from today's climate not a pre-industrial one. [Government of United States of America]	The convention for the projection chapters has been to focus on the period 1986-2005.
11-1364	11	117				Fig 11.26 Use hatching instead of grey shading in upper panels, so as not to obscure the contours. [Nathan Gillett, Canada]	Figure has been revised with hatching/stippling.
11-1365	11	117				Fig. 11.26 upper and lower panels should use same color bar. [Government of United States of America]	Lower panels have been removed. Figure has been revised.
11-1366	11	119				Fig 11.28 It's very hard to see the differences between the upper and lower panels. I suggest showing the 1986-2005 average, and the change bewteen 1986-2005 and 2016-2035. Also the maps should be made larger. [Nathan Gillett, Canada]	figure deleted
11-1367	11	119				Fig 11-28: I do not find the choice of color shades very good to highlight the changes in sea-ice concentration. It is very dificult to see any difference between the upper and lower panels except a slight darkening in September over the Arctic. Maybe this is the point to be made given that it is a near-future projection but it could still be better shown. [RYM MSADEK, United States of America]	figure deleted
11-1368	11	120				Fig 11.29: Unable to see details for RCP2.6 [David Stevenson, United Kingdom]	Figure fixed
11-1369	11	121	12			(V) within each of the HTAP regions (Dentener et al 2005)' - is this the wrong reference? I don't think the Dentener et al 2005 paper discusses the HTAP regions - it just presents global results. But maybe you somehow extracted the HTAP regional results? Seems a bit odd. [David Stevenson, United Kingdom]	Have revised refs and totally overhauled the figure to aid comprehension.
11-1370	11	121	13			regional averages over the globe' - What does this mean? [David Stevenson, United Kingdom]	Text revised.
11-1371	11	121	17			blue - I thought climate change only results were in GREEN? Also I cannot identify any dashed lines - only dotted - and these are explained earlier in the caption as referring to individual model studies (as opposed to multimodel studies with solid lines). I struggled to extract the information from Figure 11.30 - it is a complicated diagram. [David Stevenson, United Kingdom]	Yes, this has been corrected.
11-1372	11	121				Fig 11.30 I found this plot cluttered and confusing. Perhaps show regional plots above each other, and use the	The figure and caption have been extensively revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						x-axis for the scenario. Colours could then be used to show results from difference studies with slightly offset bars. Alternatively, results from all studies could be synthesized to give an overall range. [Nathan Gillett, Canada]	to address this and other issues and we have condensed the information as suggested.
11-1373	11	121				Figure 11:30: We find this figure difficult to read: multiple symbols, letters, colors. We encourage the authors to simplify as much as possible, perhaps by removing the older scenarios. [Government of United States of America]	See above.
11-1374	11	122				Figures 11:31a and b: We don't feel the backdrop global map of regions works in these figures. It is distracting, and interferes with the axis labels. We suggest the map illustrating the regions is shown as a separate panel. [Government of United States of America]	The backdrop has been redone to be less intrusive, but still identifies each box clearly.
11-1375	11	124	4	124	4	The estimates for the CMIP3 A1b results in Fig. 32a are not well explained in the text. They are hard to relate to AR4 Chapter 10, which presented a 0.6x and 1.6x range. The 83% values seem too low, with the difference 83-50, being less than 50-17, especially for 2050. The numbers are given in Annex II, where they are called a 'one standard deviation' range. There, the time is called (e.g.) 2050's, whereas here it is centred on 2050. [Ian Watterson, Australia]	These are done consistently and explained in notes now and given in Annex II.
11-1376	11	124				Fig 11.32a. Exactly the same bar is used for each of the UNEP scenarios, so they are impossible to distinguish. Use different bars for each. [Nathan Gillett, Canada]	This is now fixed and figure revised
11-1377	11	124				Fig 11.32a The caption reports that the green lines show warming relative to a base period of 1850-1900, so they presumably correspond to an alternative y-axis. But only two green lines are shown and no labels are attached to them, so this is impossible to interpret. If this information is retained, then a second y-axis should be added on the right-hand side of the plot. Thick horizontal lines are not needed. [Nathan Gillett, Canada]	This is now fixed and figure revised
11-1378	11	125				Fig 11.32b This plot is missing a colour bar. [Nathan Gillett, Canada]	This is now fixed.
11-1379	11	125				Figure 11-32b would benefit greatly by application of the stippling/hatching scheme applied to changes from the reference period in Chapters 11 and 12. We suspect that large areas would be hatched, reinforcing the claim that differences in RCP scenario are unimportant in the near term. Also, the figure is missing its color bar to show the range of temperature differences. [Government of United States of America]	Figure has been improved.
11-1380	11	125				Figure 11.32b: Color scale needs to be added. [Government of United States of America]	This is now fixed.
11-1381	11	126	1	126	14	Figure 11.33. Panels require labels (a,b,c) [Government of Canada]	Accepted. Labels have been added.
11-1382	11	129	1	129	1	FAQ11.2, Figure 1: Amend "Increased downward flux of energy due to emission from aerosol cloud" by "Increased downward flux of energy due to heat emission from aerosol cloud", to be clear [Urs Neu, Switzerland]	figure redrafted.