Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-1	11	0		200		The FOD of Chapter 11 has much improved over the ZOD. However, I feel there is more potential to further improve it and make it more homogeneous. Specifically, the distinction between near-term predictions and projections is addressed in several places but does not make it very clear. The separaton between the two seems quite artifical in a way and the lay reader may wonder why this distinction/separation is made at all. If the predictions include beside the initial state information also forecasts of the externally forced component, why do we need projections any longer? I believe this chapter would graetely benefit if these two aspects could be brought together closer. [Antje Weisheimer, UK]	Two issues are raised: (i) homogeneity: Efforts have been made to improve text on this front. However, it will require futher attentuion post-SD. (ii) the need for projections: Decadal prediction is still in an experimental phase. There are several major technical difficulties that impede our ability to conduct decadal predictions. For example, spurious climate drift that arises after the initialization phase in decadal predictions (see Section 11.3.2.1). While drift can be a problem in projections it can be reduced very substantially. So this problem is not generally as large an issue for projections as it is for predictions. Furthermore, the community has only been conducting CGCM-based decadal predictons for several years. In contrast, the community has been conducting projections for several decades. In addition, the number of groups conducting projections is greater than the number of groups conducting CGCM-based decadal predictions. The projections spefically target the externally-forced signal, whereas prefictons aim to exploit predicatbiltiy from external forcing and internal varibility. It is of great scintific interest to know about both sub-components. So while the projections are not initialized with as much information as the decadal predictions, the near-term projections continue to be a very valuable source of information on near-term climate.
11-2	11	0				I very much appreciate that both the introduction chapter and the summary provide paragraphs with clear definition of key concepts (words like projection and prediction etc.). I would urge the authors to add a clear and separate paragraph (or a FAQ) where the tem “near-term predictions” is defined and related to the terms projection and prediction . I am aware that this is difficult but it would be extremely helpful for the reader. Numbers I have seen in the text are the “next 10- to 30-years” and “2016-2035” or “until 2040” and for e.g. chapter 11 page 19 “2016-2025” are used for predictions and “2016-2035” are used for projections. [Christof Appenzeller, Switzerland]	The Introduction has been expanded to provide more information on the definition of "near-term": "Near-term" refers to future decades up to mid-century, the period for which the climate response to different future emissions scenarios are generally similar. While the chapter assesses research covering differerent sub-periods up to mid-century, emphasis is given to the period 2016-2035".
11-3	11	0				Throughout the entire report there is general agreement that natural variability could dominate the anthropogenically forced response in the near-term and that in mid-latitudes the NAO/AO/NAM related variability is a key for understanding and quantifying this uncertainty. In my opinion an explicit figure on the expected changes (near and long term) in NAO/AO/NAM variability derived from the CMIP5 models used would be extremely helpful for the reader. The same could be argued for ENSO variability. Both figures could be placed either in chapter 9, 11 or 14. [Christof Appenzeller, Switzerland]	As stated in the ext, the large response uncertainty and the potentially large influence of internal variability mean there is limited confidence in near-term projections of Northern Hemisphere circulation change. Hence we do not think a figure is warranted.
11-4	11	0				Questions and scientific findings relevant to the 2 degree target could be better reflected in this part of AR5. Questions such as, which of the RCP used in the studies fulfills such a goal and the consequences of delayed mitigation and peak year should be addressed in the executive summary of this chapter. [Øyvind Christophersen, Norway]	The possibility that global temperature change relative to pre-industrial level might exceed 2 degrees C (and 1.5 degrees C) is now addressed in Ch 11.
11-5	11	0				Please include a reference to Shindell D., et al. Science 335, 183 (2012); DOI:10.1126/science.1210026 [Øyvind Christophersen, Norway]	Accepted, now referenced in 11.1 and 11.4.6
11-6	11	0				Overall the chapter 11 conveys a large amount of information and draft 1 represents an enormous improvement with respect to draft 0. However the chapter still lacks of homogeneity. Some fundamental concepts (like the difference between the initial condition and boundary/external forcing related predictions) are not clearly stated. Also, it should be clarified that there is no real difference between projections and non-	Agreed. The revised chapter clarifies that "The use of observational-based initial conditions is a fundamental difference between a climate predition and a climate projection." [see Box 11.1]. Box 11.1 also note that "A

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						initialised predictions. Another thing that it is not stressed much in this chapter is the concept of ensemble predictions (and projections). It is given for granted that ensembles are used to estimate uncertainty. However it would be nice to have a section on the use of ensembles in box 1 (for example when the probabilistic predictions are introduced). [Susanna Corti, Italy]	probabilistic climate prediction takes the form of a probability distribution. Typically, an ensemble of climate prediction using slightly different initial condition and different models are used to contruct this probability distribution. " The revised chapter also includes a clarified discussion of the sources of uncertainty such as those arrising from initial conditions, forcing, and model [see Section 11.2.1].
11-7	11	0				Last but not least, so far the section related to the climate predictions is very short compared to the one with climate projections. This is a bit disappointing because after all predictions represent the novelty of the Fifth Assessment report. I believe that this is due to the fact that most of the work on predictions is still under way and most of scientists are still working on the recent CMIP5 integrations. However, it would be nice to have more results on the topic in the next draft. [Susanna Corti, Italy]	We have assessed literature available at the time. A little more information could be provided post-SOD.
11-8	11	0				I want to thank the authors for all of their hard work in producing this first order draft. Their work is much appreciated. [Thomas Delworth, USA]	Noted - no action required.
11-9	11	0				As an overarching comment, the chapter at this point contains a number of figures that show analyses without attribution to a paper in the literature. One possibility is that these reflect analyses conducted by the authors of this chapter as part of their contirbution to the chapter. In general, I would very much prefer to see only figures and analyses that are drawn from the peer-reviewed literature. My opinion is that this report should be an assesment of what has been shown in the peer-reviewed literature, and not the analyses that the chapter authors do using CMIP5 or other data for writing the chapter. I have no doubt in the quality of the analyses performed by the authors, but I do not think that is appropriate. While the report itself does obviously undergo peer review, that is rather different than the peer review of a single manuscript, and so analyses that appear in the IPCC report for the first time have not had the same type of scruitny and review as papers that have appeared in journals. If this is to be an assessment, how can it assess new work that is done as part of the assessment? That seems inconsistent to me. If there is only a small amount of literature on the topic to assess, that is fine; in fact, that is a statement in and of itself about the state of knowledge on the topic. [Thomas Delworth, USA]	Where appropriate figures captions will include appropriate citation and attribution. Figures for the chapter will be based on peer reviewed literature, but may be redrawn to include additional models or years in the analysis. Figures may be redrawn to improve presentation. However, the central quanitative result will always be based on the peer reviewed literature.
11-10	11	0				I generally recommend to cite the source of all figures. [Holger Pohlmann, Germany]	Agreed. Where appropriate figures captions will include appropriate citation and attribution. Figures for the chapter will be based on peer reviewed literature, but may be redrawn to include additional models or years in the analysis. Figures may be redrawn to improve presentation. However, the central quanitative result will always be based on the peer reviewed literature.
11-11	11	0				Title: Add "and Prediction" or delete "Projections and Predictability" [Holger Pohlmann, Germany]	Changes to title at this stage in IPCC process are not allowed.
11-12	11	0				The executive summary of this chapter does not provide likelihood and confidence assessments using the calibrated IPCC language. This makes it difficult for the reader to assess the reliability of the provided assessments. [Sonia Seneviratne, Switzerland]	Agreed. This has now been corrected.
11-13	11	0				Content of the present chapter is sufficiently descriptive. Readily available bibliography has been sufficiently taken into account. No significant modifications are suggested to text or figures at this stage. [Dirk Thielen, Venezuela]	Noted - no action required.
11-14	11	0				The parts related to atmospheric composition and air quality in this chapter are very well written. [Twan Van Noije, Netherlands]	Noted - no action required.
11-15	11	0				Please ensure accuracy when quoting/repeating statements from Chapter 3 of SREX, e.g., regarding heavy precipitation ('medium confidence' should be "high confidence - likely') and tropical cyclone statements. In	Quote corrected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						regards to tropical cyclones mentioned on page 29, line 58, it's important to note that Chapter 3 of SREX did not assess a possible decrease "up to a third" as is currently implied. A decrease of between -6 and -34 % was reported by Knutson et al. 2010. [Thomas Stocker/ WGI TSU, Switzerland]	
11-16	11	0				The assessment of near-term projections based on CMIP5 should to be coordinated closely with Chapter 12 in order to avoid potential redundancy. For example, it would not be ideal if substantial parts of Chapters 11 and 12 would simply assess two different time periods of the the same models and simulations from the CMIP5 archive. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed. Greater coordination has since occurred. Note that a major focus for Chapter 11 is on quantification of signa- to-noise-ratio and degree of emergence in near-term. So while Ch 11 does present some fields that Ch 12 presents, our plots provide valuable information (central to Ch 11) that Ch 12 does not provide.
11-17	11	0				We suggest to consider strengthening Section 11.4.7, e.g., expanding on the effect of volcanoes (including figures, and a specific example). There is a need to also quantify the forcing implications of 'surprises'. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. This section has been revised as part of restructuring the chapter. Quantification of volcanic and solar forcing was already included. Quantification of other forcing "surprises" is in chapter 12. We considered including a volcanic eruption scenario in the projections figures, but have instead added a clear label "Assuming no future volcanic eruptions" to a new figure which presents our overall assessment for global mean temperature.
11-18	11	0				We note that there is a gap with respect to near-term effects of geo-engineering (ref. the bullet in the approved extended outline). You might even want to consider adding a new Section between 11.4.6 and 11.4.7. [Thomas Stocker/ WGI TSU, Switzerland]	There was a multi-chapter/TSU agreement at LA1 that Geo-engineering would be covered in other chapters, and not be covered in Ch 11.
11-19	11	0				Please describe how multimodel results are combined, put on a common grid, and presented in, e.g., maps (incl. grid information etc). Please check and ensure consistency of approach across chapters, especially for Chapters 9,11, 12, 14 and, of course, Annex I: Atlas [Thomas Stocker/ WGI TSU, Switzerland]	The following text has been added to the decadal prediction section "The model original data have been bilinearly interpolated to the observational grid. The ensemble mean of each forecast system has been estimated before computing the multi-model ensemble mean.
11-20	11	0				Section on Sea Ice (11.4.5.1): We note quite some discrepancy between the September sea ice free projection here (2037) and the assessment (September ice free by 2100) given in Chapter 12, page 38. Please ensure that the differences in these two Chapter assessments are evaluated and clearly explained. Please provide appropriate cross-referencing to the assessment given in Chapter 12. [Thomas Stocker/ WGI TSU, Switzerland]	There is now greater coordination between the cryosphere sections in ch. 11 and 12; note that the studies assessed that show an earlier ice free summer Arctic are from individual models, with some calibrated CMIP3 model results; Ch. 11 now refers to the time series figure in Ch. 12, and notes that 90% of the CMIP5 models for RCP8.5 show a summer ice-free Arctic "by 2100", though some show a summer ice-free Arctic sooner than that.
11-21	11	0				Please add a discussion of the issues related to the first time slice of the Atlas (including a discussion of signal-to-noise etc). We also think that the assessment would benefit from inclusion of more regional information (see also the general comment regarding Chapters 11, 12, 14 coordination of RCM coverage). [Thomas Stocker/ WGI TSU, Switzerland]	Comment not understood.
11-22	11	0				The Executive Summary needs to be restricted to key findings from the underlying assessment. We therefore suggest to substantially shorten, e.g., the scenario introduction. The statement concerning the influence of volcanic eruptions (page 4, line 37-38) needs further quantification and has to be based on the assessment provided in the Chapter. [Thomas Stocker/ WGI TSU, Switzerland]	ES has been substantially shortened. Assessments associated with volcanoes will be based on chapter as suggested in the SOD
11-23	11	0				The Cryosphere section seems relatively brief, and might be strengthened through the involvement of relevant contributing authors. [Thomas Stocker/ WGI TSU, Switzerland]	With cross-referencing to 12.4.6 in Ch. 12, it doesn't need to be longer, otherwise discussions will be duplicated

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-24	11	1	1	1		Near-term Climate Change: Projections and Predictability [Medani Bhandari, Nepal]	No response required.
11-25	11	1	1	102	0	There is a definition for hindcast on page 8 in the forcast quality section. It is an unusual word for those not in climate science. I hope the word "hindcast" appears in the glossary. [Philip Rasch, United States of America]	'Hindcast' is briefly defined in ES and Chapter 11 Box. More details are given in section on forecast quality. Ch 11 has requested that 'hindcast' be added to the WG1 Glossary.
11-26	11	1	1			I don't know where to exactly place this comment: We have found that some of the RCP projections for HFCs are grossly over-estimated, in some parts clearly wrong/impossible Our measurement-based emission estimates for HFC-245fa and HFC-365mfc (Vollmer et al., 2011, JGR) has shown, that the RCP near-term emissions for HFC-245fa and HFC-365mfc and grossly overestimated, or wrong, for example, the RCPs for HFC-245fa have emissions for the year 2000, that is a time before this compound was even released to the atmosphere. Even if the contribution to the radiative forcing from these 2 compounds is minor, this requires correction in the original RCPs, or some post-comments. In needs to be fixed somehow, not at last because it is a potential platform for sceptisism. Thank You. [Martin Vollmer, Switzerland]	We agree with your comments, but the RCPs have many aspects in which they may be wrong. Correcting the RCPs is not the job of the 5th Assessement Report. We have added a section discussing the RCPs and HFC emissions in our chapter with updated references.
11-27	11	1	2	1	2	It has taken the IPCC over twenty years to realise that climate models are useless if they cannot predict future climate, but this will not do unless there is proven success for a long perioid. [VINCENT GRAY, NEW ZEALAND]	Model evaluation is treated extensively in Chapter 9. Past IPCC projections are compared with observations in Ch 9. The skill of decadal predictions is assessed in Ch 11.
11-28	11	1		40		I reviewed Chapter 11 and have only one suggesstion. [Anthony Lupo, USA]	Noted. No action required.
11-29	11	1				Note that the EC-Earth model results for CMIP5 have become available and published in peer-review literature. If not on the ESG server the data can be found on climexp.knmi.nl, together with the other CMIP5 runs. The references are 14. Hazeleger, W. et al, 2010. "EC-Earth: seamless earth system prediction in action." Bull. American Met. Soc., 91, 1351-1356. and 1. Hazeleger W., et al., 2011: "EC-Earth V2.2: description and validation of a new seamless Earth system prediction model." Clim Dyn. in press [Sybren Drijfhout, Netherlands]	EC-Earth is now included in the initiailized decadal prediction figures.
11-30	11	1				The distiction between Near term predictions and Near term projections is clear and useful. Reading the Near term projections I got the impression that some issues have overlap with Chapter 12, long term projections.Like the widening of the Hadley Circulation, weakening of Hadley and Walker circulation, tropical cyclones etc. The simulated changes are qualitatively not different between the first and second half of this century. What is different between those two periods is the signal to noise ratio. This is discussed, but could perhaps get more attention. [Reindert Haarsma, Netherlands]	The issue of S:N was prominent in the FOD and is even more prominent in the SOD. Emergence is now mentioned in the ES.
11-31	11	1				Given the strongly research nature of initialised decadal predictions, it would seem more prudent to discuss the near-term projections before the predictions in this Chapter. [ED HAWKINS, United Kingdom of Great Britain & Northern Ireland]	Our assessment: given that in practice the decadal predictions emphasize short time scales, and that the near-term projections naturally lead into the mid-century and beyond projections of chapter 12, we have decided to lead with the predictions.
11-32	11	1				References to Hawkins & Sutton (2010) on precipitation uncertainty should be Hawkins & Sutton (2011). [ED HAWKINS, United Kingdom of Great Britain & Northern Ireland]	accepted.
11-33	11	2	0	7	0	The executive summary is much rougher than the rest of the document and is not currently in an acceptable state. I made numerous minor comments before recognizing that it is so rough it needs a complete overhaul. [Philip Rasch, United States of America]	ES has been substantially modified. It has been shortened and now includes numerous assessment statements. Numerous revisions made to improve readability and clarity.
11-34	11	2	1	2	57	To facilitate the understanding of the executive summary as an important part of chapter 11, somewhat more structuring and clarification in some parts of the summary is recommended. It would be helpful to shorten long sentences especially in the middle paragraph. [Claudia Mäder, Germany]	Agreed. ES has been substantially modified. It has been shortened and now includes numerous assessment statements. Numerous revisions made to improve readability.
11-35	11	2	1	6	49	The executive summary for this chapter adress many very important findings, but the text could be shortened,	Agreed. ES is being substantially modified. It has

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						especially the two first pages. And the key findings should be highlighted and please try to describe findings in a less technical and more understandable way. [Øyvind Christophersen, Norway]	been shortened and now includes numerous assessment statements. Numerous revisions made to improve readability.
11-36	11	2	1	6	49	The ES is too long, probably by a factor of 2 or 3. Significant sections of the discussion could be moved to the introduction or elsewhere -- e.g. discussion of the RCP's. Indeed any discussion of these should be rationalised with Chapter 12, as there is a lot of overlap of what needs to be discussed. I suggest also remove first person discussion -- e.g. 'We are already committed …'. [Robert Colman, Australia]	ES has been shortened. First person refrences removed.
11-37	11	2	3	2	7	Maybe I ought to come back when you are better organised [VINCENT GRAY, NEW ZEALAND]	Noted. No response required.
11-38	11	2	9	2	19	It cannot possibly work unless you ditch the absurd radiation-based greenhouse theoiry altogether and concentrate on improving the methods used by weather forecasters [VINCENT GRAY, NEW ZEALAND]	No supporting evidence provided. Backgound to greenhouse effect given in earlier chapters. No response required.
11-39	11	2	9	2	19	All you are going to come up with are yet another set of "projections" which could not poaaibly compete with the far more reliable weather forecasts [VINCENT GRAY, NEW ZEALAND]	Distinction between nature and accuracy of projections and weather predictions isdiscussed in chapter. See e.g. Box 11.1 and FAQ 11.1.
11-40	11	2	9		19	The difference between projected and predicted that is meant to be conveyed here is not at all clear. projected (when the climate is most influenced by changes in external forcing such as increasing greenhouse gases) predicted (when the climate is most influenced by the time evolution of processes related to the observed initial state) It seems that "predicted" is nearer term than "projected" but not even wholly convincing. The AMS glossary of meteorlogy is informative to the distinction: climate prediction—The prediction of various aspects of the climate of a region during some future period of time. Climate predictions are generally in the form of probabilities of anomalies of climate variables (e.g., temperature, precipitation), with lead times up to several seasons (see climate anomaly). The term “climate projection” rather than “climate prediction” is now commonly used for longer- range predictions that have a higher degree of uncertainty and a lesser degree of specificity. For example, this term is often used for “predictions” of climate change that depend on uncertain consequences of anthropogenic influences such as land use and the burning of fossil fuels. It may be that the present authors mean to convey a distinction different from that, but they should at least take cognizance of the distinction drawn there. The reference to "initial state" conjures up the notation of a weather forecast that is highly dependent on initial state of the atmosphere but the accuracy of which decreases on a time scale of days. However that is surely no what is intended. [Stephen E Schwartz, USA]	The difference between predictions and projections is clarified in several places: in the text, in the Box and in FAQ 11.1. The definitions provided by reviewer in sentences 2 and 3 are not the definitions employed in Chapter 11. The essential difference is in the way the two are intialised as discussed in Chapter. The definition the reviewer has apparently taken from the AMS glossary is not ideal. It misses the essential difference. We have reworked the text to try and make the difference even clearer. In the Box we now say: "A climate prediction proceeds by integrating the governing equations forward in time from observation-based initial conditions. This almost always includes observational data describing the intial state of the ocean. .... Climate projections also proceed by integrating the governing equations forward in time, but from intial states that incorporate less information than the initial states used for predictions. The initial states used for projections are derived from climate models forced with past external forcing. Additonal data on the intial state from e.g. ocean observing networks are excluded".
11-41	11	2	9		19	The present document continues: “Near term” refers to future decades up to mid-century, the period for which the climate response to different future emissions scenarios are [sic; should be "is"] generally similar. The similarity of projections over the next several decades (e.g., Fig 12. 4) is almost certainly a consequence	The text does not state that the degree of similarity of climate over the near-term is an intrinsic property of the climate system.The text states that in climate science the term "predictability" is an intrinsic part of the climate system. The text in Box has been modified in response to his comment to make this

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of similarity of forcings over this period (in turn a consequence of persistence of CO2 in the atmosphere and persistence of emissions of aerosol precursors), not an intrinsic property of the climate system. If aerosol emissions were turned off in some scenario, the projections of the temperature change over the next several decades would differ greatly. So similarity is NOT an intrinsic property of the climate system. This is evidenced in model calculations in which emissions of aerosols are halted and temperature responds on time scale of 5 years). Brasseur GP, Roeckner E (2005) Impact of improved air quality on the future evolution of climate. Geophys Res Lett 32:L23704. doi:10.1029/2005GL023902 Knutti R, Krähenmann S, Frame DJ, Allen MR (2008) Comment on ‘‘Heat capacity, time constant, and sensitivity of Earth’s climate system’’ by S. E. Schwartz. J Geophys Res 113:D15103. doi:10.1029/2007JD009473 Knutti R., and G.-K. Plattner, 2012: Comment on “Why Hasn't Earth Warmed as Much as Expected?” by Schwartz et al. 2010. J. Climate. In press, http://dx.doi.org/10.1175/2011JCLI4038.1 Matthews HD, Caldeira K (2007) Transient climate-carbon simulations of planetary geoengineering. Proc Natl Acad Sci USA 104:9949-9954 The similarity of forcings is explicitly stated on page 11-2, line 41: "For the near term, the range in anthropogenic RF of the RCPs is similar across the RCPs." substantiating my point. From this it appears, at least tentatively, that the only difference between prediction and projection is that in projection forcings can differ, whereas in prediction, the authors assume that forcings are pretty much locked in. If that is the case this should be made explicit. This conclusion seems at variance with line 17 which refers to the "processes (presumably of the climate system) underpinning predictability," and even more with the statement page 11-3, line 19-20 that states explicitly: In climate science the “predictability” of a given climatic feature is a quantifiable intrinsic property of the climate system. [Stephen E Schwartz, USA]	clearer: "Formally, predictability is a feature of the physical system itself, rather than of our "ability to predict" which depends on the accuracy of climate models, initial conditions, and the accuracy with which external forcing over the period being predicted can be modelled. Climate predictability may be studied diagnostically, by analyzing past climate system behaviour (observed or modelled), or prognostically by making a sequence of predictions with a model of the climate system. The rate of separation of initially close states or, in the probabilistic view, the evolution of the probability distribution is studied and quantified. The predictability of different variables in the atmosphere and ocean will be different and will also vary with location. Estimates of the predictability of the climate system provide insight into the possibility of, and the expected limitations to, skilful climate forecasts".
11-42	11	2	9			The statement about the timescales of interest here does not really reflect the material later in the chapter - nor does it agree with the CMIP5 decadal predictions out to 10 years. It would be better to state that the emphasis here includes the next 10 years. See also point 12 below [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	The ES states that : “Near term” refers to future decades up to mid-century, the period for which the climate response to different future emissions scenarios are generally similar. Greatest emphasis is given to the period 2016–2035, though some information on projected changes before and after this period (up to mid-century) is assessed. We think this is a good reflection of situation.Very little information is actually provided from the predictons for next ten years.
11-43	11	2	15	2	15	Change to "is also assessed" [Philip Rasch, United States of America]	accepted- text changed
11-44	11	2	24	2	24	This section is too specific for an executive summary [Eric Guilyardi, France]	accepted. ES has been shortened considerably
11-45	11	2	24	2	25	I am eagerly awaiting details of these scenarios which have not been given so far in 10 Chapters [VINCENT GRAY, NEW ZEALAND]	Details on the RCPs used to force the CGCMS used to obtain projections are provided in Chapters 1 and 8. This fact is now mentioned in Ch 11.
11-46	11	2	24	2	39	This is the executive summary, and should be extremely readable. It is not The transition from introductoryl material to technical in this paragraph is very abrupt. The paragraph includes a lot of jargon (some defined, some not) but little context. It would be much more readable if one said things like "scenarios used in previous	The ES has been overhauled to make it much more readable.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						assessments differ in the following ways" (then list whatever you think are most important). The order is also strange, since you discuss CMIP5 here before introducing it a few paragraphs below at line 51. [Philip Rasch, United States of America]	
11-47	11	2	25	2	25	I am unclear what it means to be "developed for, but independently of"; please clarify, as that seems contradictory to me. This goes to a fundamental issue of whether the IPCC and CMIP processes are truly independent. [Thomas Delworth, USA]	The entire RCP discussion is revamped in 11.3.5.1 and phrasing has been revised,as has corresponding material in Exec Summary. The text was correct, but has been rephrased to make meaning clearer.
11-48	11	2	31	2	39	This sentence is confuse to me. I do not understand what is different, and why. AR4 used SRES scenarios, while this AR5 will use RCPs. CMIP5 simulations are also based on RCPs. Why RF trajectories for the same scenario (=same RCP?) differ by X%? And why RCP emissions are unuslally low after mid-century? Despite my confusion can be due to my personal difficulties in understanding this matter, I think that the sentence could be written more clearly also for other readers. [Claudio Cassardo, Italy]	Text in SOD has been clarified (see 11.3.5.1). This text is correct as written, but clarified in the SOD. See Chapter 1 SOD for more history of the emissions scenarios.
11-49	11	2	36	2	39	I agree that the RCPs don't represent the range of SLCF emissions. The climate consequences of this needs to be brought out more with a sentence like "Hence the resulting climate predictions may also not represent the range of possible futures." [William Collins, United Kingdom of Great Britain & Northern Ireland]	Yes, this is now highlighted in the SOD and ES of our chapter
11-50	11	2	43	2	45	Confusing sentence. Maybe better as "For the RCP2.6 pathway, which involves major mitigation …, the uncertainty range of continued near-term warming is narrow ..." (if I understood the idea correctly) [Jouni Räisänen, Finland]	Statement no longer appears in shortened ES
11-51	11	2	47	2	47	Variations from these paths … in radiative forcing or climate? [Jouni Räisänen, Finland]	statement no longer appears in shortened ES
11-52	11	2	47	2	57	the relative importance of internal variability and external forcing isn't really known, so the statements "variations in these paths will be strongly influenced by natural variability" and "the climate we observe in the near term will be strongly influenced by the internally generated natural variability" might be a bit too strong. For example, recent analysis (Booth, B.B.B, N.J. Dunstone, P.R. Halloran, T. Andrews and N. Bellouin, 2012, Aerosols Implicated as a Prime Driver of 20th century variability within the North Atlantic, Nature, in press) suggests that anthropogenic aerosols may have been much more important than internal variability in driving the observed decadal changes in the Atlantic. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Accepted. - "strongly" has been deleted.
11-53	11	2	49			“natural variability varies from quantity to quantity and from location to location” add “and area aggregated”. [Christof Appenzeller, Switzerland]	statement no longer appears in shortened ES
11-54	11	2	54			First person plural: "We are already committed to further externally forced near-term warming due to past 55 emissions. " Who? The chapter authors? Avoid first person plural; restrict use to chapter authors. "Earth is already committed.." [Stephen E Schwartz, USA]	Accepted. First person dropped.
11-55	11	2	55	2	55	change "reinforced" to "amplified" or "increased" [Philip Rasch, United States of America]	changed to increased
11-56	11	2		6		The Executive Summary does not yet read as a smooth piece of text but rather as a collection of paragraphs in very loose (or none) relation to each other. For example, the level of detail and abbreviations used (RF is not explained, CMIP5 is only explained in a pater paragraph) in the paragraph beginning with line 24 on page 11-2 is in contrast to most of the other paragraphs and needs a re-write. Further, some paragraphs do include references whereas most do not - this needs to be homogeneised. [Antje Weisheimer, UK]	Agreed. ES hs been overhauled taking these issues into account.
11-57	11	2				For the Executive summary, I suggest (a) shortening substantially, (b) provding more structure via sequenced subheadings. For example, I was not sure whether or not the title "Predictability" on page 3 meant that we had now left the Executive Summary, since the font style for "Predictability" was the same as "Executive Summary". Additional granularity in the headings would help. As one example of text that could be cut, the discussion on p. 3, lines 27-47 seems inappropriate for an Executive Summary. [Thomas Delworth, USA]	Agreed. ES substantially reduced. More structure provided.
11-58	11	3	5	3	56	However you try to wangle it, pedictability always depnds on a proven ablity in successful future prediction. If you don’t have that, you are wasting your time. And mine [VINCENT GRAY, NEW ZEALAND]	Predictive skill is assessed and discussed in this chapter. The ability of models to simulate past climate is assessed in Chapter 9. A comparison between

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							projections described in previous reports and subsequent observations is provided in the same chapter. FROM PACO: Climate predictions always include an estimate of the skill (cf Fig 11.7-11.9)
11-59	11	3	5		16	Back to "prediction" vs "projection." "Near term predictions ... can provide estimated changes in the time evolution of the statistics of near-term climate, which can be used as a scenario-independent basis for adaptation in the near term (see WGII) in which uncertainty associated with internal variability can be reduced." First, trying to parse the sentence: do the authors really mean "estimated changes in the time evolution of the statistics of near-term climate"? Or do they mean something more like "estimated changes in the the statistics of climate over the near term"? or "estimates of the time evolution of the statistics climate over the near term"? I continue to assert, as, I hope convincingly and persuasively demonstrated above, that predictions as defined here are scenario DEpendent, not independent as stated. That demonstrated, I think the entire distinction between prediction and projection vanishes. [Stephen E Schwartz, USA]	The meaning is as stated in text: "estimates of the time evolution of the statistics climate over the near term". We do not state that predictions are scenario independent. We state e.g. in ES that: “Near term” refers to future decades up to mid-century, the period for which the climate response to different future emissions scenarios are generally similar". This leaves room for the exceptions you are mindful of. A key difference between predictions and projections - as they are currently performed in practice - is the extent to which ocean and other data are used in intialisation. Please seee.g. Box 11.1 for further details.
11-60	11	3	9	3	9	Should "initialized" be "uninitialized"? [Thomas Delworth, USA]	agreed, and revised accordingly.
11-61	11	3	9	3	9	should "initialized" be "uninitialized"? [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	agreed, and revised accordingly.
11-62	11	3	9	3	9	What is meant by "(initialized) projections"? This is VERY confusing given all the effort to define predictions and projections. [Antje Weisheimer, UK]	The use of language for projection and prediction has been clarified in Box. 11.1. A projection makes no attempt to predict the actual evolution of the natural variability of the climate system. The projection is the response to the climate system to the external forcing associated with the particular senario. A prediction used best available observations of the climate system to intialize the prediction system and is attempting to predict the natural variability in the presence of the external forcing assocaited with the particular senario. Section 11.2.3.1 also clarifies that "To be clear, in the context of this report these studies are viewed as projections since no attempt is made to use observational estimates for the initial conditions. Essentially, an “uninitialized” prediction is synonymous with a projection. "
11-63	11	3	18	3	56	The section regarding predictability is good, but it could benefit from being shorter and more precise. [Øyvind Christophersen, Norway]	Section has been re-jigged to improve accuracy and clarity and to keep as short as possible.
11-64	11	3	18	6	49	I couldn’t understand the rationale behind the order of subsections in the Executive Summary. First comes Predictability (and here I would expect to have a summary of the definitions of projections and predictions, since the distinction between projections and predictions is very much stressed in the chapter and since results from both projections and predictions are presented lately). Then there is a subsection on Atmospheric composition, then a couple of lines on near-term projections, then Temperature Changes, Hydrological Cycle	Executive Summary has been completely revised for the SOD taking into account these suggestions.

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						etc... But maybe these last ones are sub-sections of near-term projections. The last Section is on Near-term predictions. On the other hand the Near-Term predictions is the first Section later in the Chapter. In conclusion I believe that the Executive Summary should be more consistent with the rest of the chapter (being a summary), therefore it needs to be accordingly reshaped. [Susanna Corti, Italy]	
11-65	11	3	19	3	37	in practice predictability of the real world is not quantifiable (unless we have a perfect model). This discussion appears to summarise model results, but the real world could be very different. This should be made clear. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	agreed. See latest version of ES
11-66	11	3	19			Certainly NOT "quantifiable intrinsic property of the climate system". Future climate even on the time scale of a decade is strongly dependent on forcing; future climate on this time scale depends strongly on continued forcing or change therein. [Stephen E Schwartz, USA]	ES has been overhauled. Expression no longer appears.
11-67	11	3	20	3	20	Reword, dropping 'loosely'. We need a precise definition of predictability in this summary. [Robert Colman, Australia]	ES has been overhauled. Word no longer appears.
11-68	11	3	27	3	37	Quantify "low", "higher" and "very low" predictability. [Holger Pohlmann, Germany]	Quantification now given
11-69	11	3	30	3	31	"Predictability in suface temperature … tends to arise from external sources of predicatiblity". The sentence is awkward and could use a rewrite to make it clearer. Can you include a sentence identifying the source of predictability? The discussion in the chapter itself is much stronger [Philip Rasch, United States of America]	agreed, clarified.
11-70	11	3	30	3	32	Phrase unclear [Eric Guilyardi, France]	clarified.
11-71	11	3	31	3	31	Insread of "arising form external sources of predictability" I will say "arising from external forcing" [Susanna Corti, Italy]	agreed.
11-72	11	3	36			“By the end of the first decade the low level of predictability in precipitation evident is almost entirely dominated by the forced component” WHAT DOES THIS EXACTLY MEAN? [Christof Appenzeller, Switzerland]	this has been replaced to make clearer: "On long timescales the forced component is the primary source of predictability in precipitation. However, the level of predictability provided is low".
11-73	11	3	39			General comment: I think the chapter should make a clearer distinction between actual predictability demonstrated by comparing the results of predictions studies with observations and potential predictability when predictions are compared against model estimates or studied within the perfect model world. On the same issue: the chapter defines the predictability as an intrinsic property of the climate system. And then on page 3, line 39 it says that AMOC exhibits decadal predictability. How do we know that AMOC has a decadal predictability, when there are no long term continous AMOC observations. Potential predictability yes, but not predictability. [Daniela Matei, Germany]	a. Text has been modified in several places to further clarify this distinction. See e.g. Box 11.1 and the relevant part of main text. Re predictability of AMOC. It is important to be clear on the definition of "predictability" used here. Please see Box 11.1 and text introducing this and related terms. Strictly speaking one does not need observations to estimate predictability. One can use model output and examine predictability of the simulated AMOC. One needs observational estimates of AMOC to verify predictions, but this is a different issue.
11-74	11	3	49	3	49	Is there a citation for this statement? [Thomas Delworth, USA]	this statement has been deleted
11-75	11	3	51	3	57	again, the sentences in this paragraph could be clarified. Is it correct to rewrite the first sentence in the following way? "Predictability assocated with the initial state decays due to the amplification of small differences in the field associated with internal variability. On the other hand, the climate system response to external forcing is more easily discernable as time increases, making the relative importance of external forcing larger as time progresses. [Philip Rasch, United States of America]	Sentence has been changed to: Predictability associated with the initial state of the system decreases with time while that due to the forced component increases, with average cross-over times of the order of 4-9 years.
11-76	11	3		6		Executive summary not reviewed since it missing relevant numbers though the overall structure looks very good. [Larry Thomason, United States of America]	Noted. No response required.
11-77	11	4	3	4	3	"at near steady state" would better be said "in near equlibrium" (the later phrase allows concentrations to change with emissions. The former implies the concentrations don’t change) [Philip Rasch, United States of America]	Sentence no longer appears in shortened ES.

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11-78	11	4	7	4	7	Section 7.3.6.2 presents the changes in aerosols (including mineral dust) with climate, which can be mentioned here. [Hong Liao, China]	Sentence no longer appears in shortened ES.
11-79	11	4	15	4	15	by emissions of NOx and CH4, should be "by emissions of Nox, CH4 and VOC [Xuemei Wang, China]	Sentence no longer appears in shortened ES.
11-80	11	4	15	4	18	It is not clear if the evidence points to increasing frequence and/or duration of stagnation episodes, and how general and robust this finding is. [Twan Van Noije, Netherlands]	Sentence no longer appears in shortened ES.
11-81	11	4	18	4	21	Suggest mentioning that the model projected regional patterns of ozone to climate change is still uncertain on intracontinental scales. [William Landuyt, United States of America]	Accepted - sentence inserted: "On the regional scale, uncertainties remain in the magnitude, and in some cases the sign, of the projected surface O3 responses to climate change"
11-82	11	4	29	5	50	This is just a note relative to the formatting for sessions: all subsequent sections after "Near-term Projections of the Climatic Response to External Forcing" (e.g. "Temperature Changes", "Hydrological Cycle", …) seems to me sub-sections, thus I am suggesting a different format for their title. [Claudio Cassardo, Italy]	The structure has been carefully reasessed and altered.
11-83	11	4	30	4	30	Again the sentence is awkward. It should not be necessary to repeat the phrase AR4 three times. Perhaps it is sufficient to say "conclusions and inferences regarding near-term projections to external forcing are generally consistent with those of AR4" [Philip Rasch, United States of America]	agreed. ES has been overhaul;ed. This no longer occurs.
11-84	11	4	34	4	41	The results in Figure 11.38a should be used in this paragraph, UNEP CH4+BC lies outside the range of the RCPs. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted - sentence inserted: Larger differences in near-term temperature (up to XX degrees), however, could occur in the near-term along other emission pathways for aerosols and methane which are not represented by the narrow range across the RCPs (Figure 11.38a)
11-85	11	4	34	4	47	These indications did come from CMIP5 simulations initialized with RCPs or from AR4 scenarios? It seems to me there is confusion here about old and new results. [Claudio Cassardo, Italy]	These were placeholder figures only. They have been updated using CMIP5 in SOD. Chs 11 & 12 are using the same methods to calculate such things hence concisistenty is assured.
11-86	11	4	38		41	"The AR4 concluded that, based on climate change commitment (further warming that would occur if concentrations of GHGs were instantly stabilized), the system is already committed to warming that amounts to about xx per decade out to several decades. Therefore, approximately 50xx% of the 2016–2035 warming occurs in response to past emissions." It would appear that the "xx" and other similar symbols are placeholders. One would wish to see the numbers. However the concept of substantial committed warming and this statement, whatever numbers are put in it, are wholly inconsistent with the following from chapter 12, page 7, line 8: "If radiative forcing were stabilized, the fraction of realized warming at that point is around 85 ± 10% of the total, and is almost independent of the forcing scenario. Equilibrium is reached only after centuries to millennia". This inconsistency needs to be resolved. [Stephen E Schwartz, USA]	Yes, they are placeholders. They do not apear in the SOD. Figures have been cross-checked with Ch 12. Chs 11 and 12 are using the same methods to calculate such things hence concisistenty is assured.
11-87	11	4	38			Will there also be an additional range added here to represent the range of internal variability? [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	An allowance for estimated internal variability has been included in the ranges provided
11-88	11	4	39			In the executive summary the term “climate change commitment” (further warming that would occur if concentrations of GHGs were instantly stabilized) is used, which is a very useful concept. I am wondering whether in a chapter with the focus “Near-term Climate Change” the term “committed climate change” should also include a certain scenario for the near term release of greenhouse gases. It seems to me impossible to immediately change our energy system. [Christof Appenzeller, Switzerland]	Chapter 11 mainly uses the RCP4.5 scenario for near-term GHG concentrations
11-89	11	4	50	4	54	This paragraph should also mention future changes in the Asian monsoons due to pollution in the Atmospheric Brown Cloud [William Collins, United Kingdom of Great Britain & Northern Ireland]	changes in the Asian monsoons is covered in Ch. 14

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11-90	11	4	53	4	53	Seems like an understatement. "most of the regions" suggested. [Jouni Räisänen, Finland]	Es has been carefully re-crafted.
11-91	11	4	56	5	16	A specific sub-section "Stratospheric Forcing of the Troposphere" has been separated by the one for the "Atmospheric Circulation". However, the stratospheric forcing mentioned influences the atmospheric circulation, so I suggest to merge the two sub-sections. In fact, the "stratospheric cooling due to stratospheric ozone depletion" (chapt. 10 p. 31 line 26), is considered (chapt. 10, page 4, lines 30-31) as a probable cause for the differences between the atmospheric circulation in the two hemispheres. [Claudio Cassardo, Italy]	Careful considration has been given to these and related comments. The structure has been re-ssessed and latered to what we think is the best structure.
11-92	11	5	2	5	50	Extrapolation from the past is at least a beginning, and you have not practiced it much before [VINCENT GRAY, NEW ZEALAND]	Noted - no action required.
11-93	11	5	11	5	11	"stratospheric ozone is expected fully recover by approximately 2060": the specification of the exact year, even if with the word "approximately", seems to me excessive, as it is not yet sure we have reached already the minimum. I suggest a more conditional sentence, and also to cite a reference. See also pag. 27 line 9, in which a different year is mentioned for the recover, and there is a reference. [Claudio Cassardo, Italy]	The statement no longer appears in the SOD.
11-94	11	5	11	5	11	Should the word "to" follow "expected"? [Thomas Delworth, USA]	Es has been overhauled. No longer appears.
11-95	11	5	14	5	14	TYPO: "by" instead of "be" [Ladislav Metelka, Czech Republic]	Es has been overhauled. No longer appears.
11-96	11	5	14	5	14	change "be arrested" to "stop"; change "be mid-century" to by mid-century [Xuemei Wang, China]	Es has been overhauled. No longer appears.
11-97	11	5	19	5	21	It is probably more instructive to mention the sea ice changes for the different seasons. [Hugues Goosse, Belgium]	agreed--we now specify the seasons for these changes in sea ice area
11-98	11	5	21			0.5xxm should be cubic meters? [David G. DeWitt, USA]	this sentence has been deleted
11-99	11	5	23	5	24	These effects have repercussions on the hydrological cycle at local scale. [Claudio Cassardo, Italy]	agreed, and no change necessary
11-100	11	5	26	5	26	"snowfall is projected to increase": please cite references. [Claudio Cassardo, Italy]	this is the executive summary--discussion and references are given in section 11.4.5 and 12.4.6.2
11-101	11	5	31	5	31	"include changes with the same sign as the long-term projected changes in" might more easily read by saying "show similar signatures to those of" [Philip Rasch, United States of America]	This is a useful suggestion, but the respective text has been removed while shortening the executive summary.
11-102	11	5	38	5	38	Please be more explicit with "Daytime extreme temperatures" - warm extremes in summer, cold extremes in winter, or both? [Jouni Räisänen, Finland]	The text has been improved, and now mentiones "high-percentile daytime summer temperatures" for cliarification.
11-103	11	5	39	5	40	This sentence essentially says that diurnal temperature range in winter is decreasing. Is this what is meant (and is this the best way of saying it)? [Jouni Räisänen, Finland]	Not necessarily, as the statement addressed high-percentile daytime temperatures. The possibility that this is related to the diurnal temperature range is now mentioned in the text.
11-104	11	5	42	5	42	"increases in heavy precipitation events": increases in number, or intensity, or both? [Claudio Cassardo, Italy]	Text has been clarified
11-105	11	5	45	5	45	delete) [Xuemei Wang, China]	Typo corrected.
11-106	11	5	46	6	43	They can't all be right and maybe they are all wrong [VINCENT GRAY, NEW ZEALAND]	Comment does not make sense. Question might be predicated on false assumption that only one mechanism can be responsible, whereas in fact sentence says that there are multiple forcings at play.
11-107	11	5	47	5	50	This is a curious statement. In the rest of this chapter, appraisals are given for global projections (which are relatively robust) and for regional projections, which on the whole are not, but in the sphere of tropical cyclones, the reader is treated only to a statement about the non-robustness of regional projections. This paragraph needs to be preceded by one pointing out that with moderate confidence, the global frequency of	The entire discussion of tropical cyclones in the Executive Summary has been revised.

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						high-intensity tropical cyclones is projected to increase. [Kerry Emanuel, United States of America]	
11-108	11	5	47	5	50	The AMO could also be listed here [Eric Guilyardi, France]	The AMO is now included.
11-109	11	5	48	5	50	"It is very likely that tropical cyclone ... ENSO and the IPO": what is the usefulness of this sentence? The important thing is to assess the variation in dependence of global warming, or anthropic emissions, but this is impossible, at the current stage. The dependence of TC number and intensity on ENSO, IPO, ... appears obvious, it is not necessary to say that it is very likely. [Claudio Cassardo, Italy]	The usefulness of this sentence lies in the fact thst it points out that for TC properties the primary cause of decadal variabiltiy of near-term is likely to be internal variabiltiy not external forcing. You are an expert and so the second statement does not surprise you. However, it might be that e.g the IPO is weak over coming period and so it might not actually drive much variability in TCs. Hence a qualification is needed.
11-110	11	5	52			"Globally and regionally, the surface temperature response is fairly independent of scenario until after 2040". This is correct only in that the forcings (which need to be shown) are quite similar. Thus the statement should be qualified. [Stephen E Schwartz, USA]	this paragraph now deleted
11-111	11	5	53	5	55	The first sentence is not needed, as the following sentence explains the difference. This difference is also evident in figure 11.38a where RCP6.0 is colder than RCP8.5 (UNEP CH4+BC is colder still). [William Collins, United Kingdom of Great Britain & Northern Ireland]	This text has been entirely revised for the SOD following new figures with more models included.
11-112	11	6	5	6	11	evidence for predictive skill in the ocean is strongest in the north Atlantic, so should be included in the discussion here along with the Pacific. [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	The SOD now contains an extended discussion of the North Atlantic skill
11-113	11	6	8	6	9	Here (and elsewhere) the term decadal is used rather loosely. In this partoicular case there needs to be a more explicit statement about the timescales for which there are skillful predictions. [Thomas Delworth, USA]	agreed, and revised accordingly.
11-114	11	6	8	6	11	Is this statement (decadal scale predictive skill for the North Pacific) consistent with the bottom panel of Fig. 11-7, in which there is essentially no skill? [Thomas Delworth, USA]	Section 11.2.3.4 now illustrates that the CMIP5 integrations have a minimum of temperature skill over the North Pacific, which is consisten across many different forecast systems and agrees well with the zero skill of the IPO.
11-115	11	6	8			I disagree that no evidence has been found of near surface temperature predictability. Smith et al., Nature Geosci., 2010 show clear evidence of prediction skill over land, it is just that this comes mainly from the boundary forcing rather than initial conditions. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	agreed, we clarify that there is some predictability from the focing over land. A statement has been included to clarify that there is limited additional predictability over land arising from initialization.
11-116	11	6	16			Missing period at end of sentence. [David G. DeWitt, USA]	accepted
11-117	11	6	20	6	21	A link/consistency check with corresponding sections in Chap 9 is needed here [Eric Guilyardi, France]	agreed
11-118	11	6	21	6	21	"that do not depend on the models incorporated into the prediction systems" might better be "that do not depend on other models, and proxies within those models that are highly model dependent" [Philip Rasch, United States of America]	accepted
11-119	11	6	21			REPLACE “The ability to verify hindcasts of past AMOC variability is severely hampered by the absence of records of past ….” BY “The ability to verify hindcasts of past AMOC variability is severely hampered BY THE LIMITED NUMBER OF INDEPENDENT SAMPLES AVAILABLE and the absence of records of past ….” [Christof Appenzeller, Switzerland]	accepted
11-120	11	6	25	6	25	without a perfect model it is not possible to partition predictability between internal variability and external forcing - suggest removing the statement that predictability of the AMOC has been attributed to internal variability [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	All attribution statements occur within the context of a model, and in the study being cited internal variability was identified as the crucial factor in the AMOC changes. Please see new ES - it has been overhauled.
11-121	11	6	25	6	26	Several recent studies show the predictability can also arise from the forcing (eg. Swingedouw et al.:	This appeared in the FOD ES but no longer appears.

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						Initialisation and predictability of the AMOC over the last 50 years in a climate model. Clim. Dyn., in revision 2012) [Eric Guilyardi, France]	The Swingedouw et al. study was also not available to the Chapter at the time. It was subsequently obtained and is now referenced in the body of the Chapter.
11-122	11	6	26	6	27	"Assessments of the skill with which associated impacts over land can be predicted have not been conducted but skill is likely to be very low": this seems a prediction of the skill over land in absence of specific studies. I suggest to rephrase the sentence. [Claudio Cassardo, Italy]	The sentence has been removed from the SOD.
11-123	11	6	37	6	37	to me the term "initialsing internal variability" is very strange. I think you are producing a series of simulations with different initial conditions with a spread based on estimates of internal variability. My way of stating it is somewhat different than yours. [Philip Rasch, United States of America]	The discussion is clarified in the SOD. The text notes that the intialized prediction are attempting (albiet probabilistically) to prediction the actual evoution of the natural variability and the forced response.
11-124	11	6	37			"providing" should be "provided". [David G. DeWitt, USA]	Section currently being re-written. Will be re-assessed after re-write.
11-125	11	6	43			I am not convinced that total skill decreases with lead time, only that the part from initialisation decreases with lead time (see e.g. Smith et al, Nat. Geosci., 2010 where skill increases with lead time by some measures due to boundary forcing). It is important not to confuse total skill with that coming from initial conditions. See also comment 10 above. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	ageed and text is being re-written to clarify the fact that there is predictability from the forcing that does not necessarily decrease as a function ot lead-time
11-126	11	6	45	6	45	"Climatic surprises": I do not like very much the word "surprises". Maybe "unexpected climatic events" sounds better. Among these events, it may be cited the prolonged minimum solar activity during the past 2-3 years. [Claudio Cassardo, Italy]	Noted. We have restructured this section and no longer use the term surprises. The recent solar minimum is discussed in 11.3.6.2.2
11-127	11	6	48	6	49	It would be helpful to refer to other chapters of the report, where these “climatic surprises” are explained in more detail. To get further information about “explosive volcanic eruptions” a reference to FAQ 11.2. is recommended. An analogical reference for the solar output would also be helpful. [Claudia Mäder, Germany]	Taken into account. The relevant pointers are included in 11.4.7. A reference to FAQ 11.2 has been added to the Executive Summary.
11-128	11	7	1	7	17	I would consider rephrasing the first sentence of the introduction, since one can not asses the current scientific understanding of a future climate. One can most predict or project the future evolution of the climate system, but not understand something that have not yet happen. The same goes for “understanding of near-term climate” on line 4. Or if the field of “near-term” climate predictions/projections is meant instead of “near-term” climate, the text should be accordingly changed. [Daniela Matei, Germany]	Agreed. Sentence has been replaced with:" This chapter describes current scientific expectations for “near-term” climate - including atmospheric composition and air quality - and it assesses the scientific basis for the expectations.
11-129	11	7	1	9	41	As this is the first time that near-term predictions and projections have been discussed, I think it is important to spend some space on introducing all the various terms; precitability, prediction, projection, perfect ensembles, initial value vs boundary conditions, etc. as the author team have done. Nice job. However what I don't get from this draft introduction is a strong message that, based on current evidence, there is a large component of natural climate variability that is in no way predictable and probably never will be. For example, we will never be able to predict an ENSO event 5 years into the future. Probabilistically there is a lead time at which even a perfectly initialised ensemble prediction system will have a spread which is indistuingishable from the spread from a long control experiment or a no-assim ensemble. I suppose the point is not to raise expectation that everything can be predicted, either through initilaisation or because of boundary forcing. [Matthew Collins, United Kingdom]	All of these points are now meant to be treated in the Box. One of the figures is meant to indicate how the initial probability distribution spreads and becomes indistinguishable from that of a forced simulation and another figure to show how for longer leads and averages the forced component accounts for the predictability.
11-130	11	7	1	19	33	I found the discussions in sections 1, 2 and 3 to to be very informative particularly since this is somewhat outside my area of expertise. Since non-scientists are likely to prowl these pages these material goes a long way to providing an honest assessor a mechanism to understand what the predicts that occur later mean to the science world. It is a valuable bridge. [Larry Thomason, United States of America]	Noted. No action required.
11-131	11	7	3	7	44	Essentially extrapolation from the past [VINCENT GRAY, NEW ZEALAND]	This chapter is primarily concerned with projections and predictons of the future using e.g. complex coupled general circulation models. These models are forced using information from the past, and then the model equations - which represent the physics of the

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							climate system - are then integrated forward in time using numerical methods. The integrations then provide the projections and predictions described. Hence 'extrapolation' is not an accurate description of the major focus in Chapter 11.
11-132	11	7	17	7	19	Reword clumsy sentence. [Robert Colman, Australia]	agreed, rewritten.
11-133	11	7	21	7	28	How are the short-lived species accounted for In the near term projections of CMIP5? Are results from the ACCMIP presented in this chapter? [Hong Liao, China]	Sentence added: ". The near-term climate responses may be particularly sensitive to short-lived climate forcing agents (UNEP/WMO, 2011; Shindell et al., Science, 2012).". Also, added to point (iii), ", including new findings from the Atmospheric Chemistry and Climate Model IntercomParison (ACCMIP) initiative"
11-134	11	7	30	7	44	The distinction between points (ii) and (iv) would benefit from being more explicit [Eric Guilyardi, France]	done
11-135	11	7	31	7	44	I suggest to put point "iv" before point "ii", as in the logical consecutio of the index. [Claudio Cassardo, Italy]	done
11-136	11	7	36	7	36	TYPO: "is" instead of "in" [Ladislav Metelka, Czech Republic]	done
11-137	11	7	41			"and" at end of line is not needed for the list. [David G. DeWitt, USA]	corrected
11-138	11	7	42	7	44	Here point (iv) of the list should become point (ii) for sake of consistency with the progressive order of the related subsections. [Susanna Corti, Italy]	done
11-139	11	7	49	9	11	Box 11.1: "Climate Prediction, Projections and Predictability" should include a description of the techniques used to increase predictability. Such as ensemble runs and the use of multi-model ensembles. [Øyvind Christophersen, Norway]	This is discussed in Section 11.3.1
11-140	11	7		9	11	Nice high order overview of what predictability really means, maybe drags just a bit and could be shortened [Larry Thomason, United States of America]	Thank you. We have tried to be a terse as possible and don't really see how to condense further. Readers with less expertise than the reviewer may need this additional information.
11-141	11	7		9		While I appreciate the effort of the authors to give detailed definitions of Predictions, Projections and Predictability, I don't think it has clarified everything, see first general comment. [Antje Weisheimer, UK]	The box gives an operational definition which seems to be reasonably clear. Need a more specific understanding of the difficulty that the comments reflect.
11-142	11	7				As in comment no.8: I don't think the near term prediction community expects to see this described as 10-30y rather it is more usually taken as 1-10y as this is the major period when initial conditions appear to play some role. Again suggest broadening the definition to 2-30y or similar. Otherwise there will be very little to discuss on the effect of initial conditions! [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	In terms of near-terms predictions, the SOD focuses on 1-10 year time scale and this is referenced in the introduction: "The need for near-term climate information has spawned a new field of climate science, decadal climate prediction (Meehl et al., 2009a; 2012a). Reflecting this new activity, the Coupled Model Intercomparison Project phase 5 (CMIP5) experimental protocol includes, as one of its foci, near-term predictions (1-10 years), where there is an emphasis on the initialization of the climate system with observations."
11-143	11	8	1	8	1	Please delete "and", in accordance to the legend of fig. 11.1. [Claudia Mäder, Germany]	agreed. Will do
11-144	11	8	2	8	2	"difference from the 1901–1950 average": in other parts and in most references, it is used the word "anomaly" instead of difference. I am also curious to know why it has been used here the period 1901-1950 as reference period, instead of using the periods 1961-90, or 1971-2000, or 1981-2010, frequently used in the literature (the	This Figure is now replaced by a new Figure 11.1 which now illustrates the points more directly by plotting the obs, the ensemble mean of a set of

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						latter only very recently, for evident reasons). I think that this report would constitute a reference benchmark for future analyses, so it would be important to suggest a standard reference period also to be used in future analyses. [Claudio Cassardo, Italy]	simulations and, as well, a forecast mean and an ensemble of forecasts.
11-145	11	8	3	8	3	This is just a little comment on the assumption that T(t)=Tf(t)+Ti(t). This relation implicitly assumes a linear relationship and a non-interference (or uncoupling) between the forced and the internal generated variability. However, even if this assumption can be reasonable as a first-order approximation, we do not know what is the range of validity of it. For example we couldn't assess (so far) to what extent Tf(t) may be a function of Ti(t) and vice versa (i.e.: T(t)=Tf(t, Ti(tau))+Ti(t,Tf(tau))). Therefore I would mention somewhere in the text that this is a first order assumption useful to distinguish the two components of temperature variability. [Susanna Corti, Italy]	This is now addressed explicitly in the revised text.
11-146	11	8	3			Use of this format of the temperature equation for a temperature change is probably not the best choice. Why not use a format that talks about delta temperature. People may be confused by the choice of equation to represent the point. [David G. DeWitt, USA]	This is a matter of taste. Equation is accurate.
11-147	11	8	3			Perhaps this ought to at least point out that linearity is assumed here between natural and anthropogenic effects - this may not always be the case although I agree that no very convincing examples have yet been found. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	This is now addressed in revised text.
11-148	11	8	8	8	13	Here it is not mentioned that since climate projections attempt to determine the evolution of the forced component Tf(t) only, they are not dependent on initial conditions (i.e. uncertainties in such projections may arise from the accuracy in the external forcing itself, or from uncertainties in model formulation). The independency of projections from the initial conditions should be clearly stated because this is the main (practical) difference between projections and predictions: in the second case models are initialised. [Susanna Corti, Italy]	This is now stated more explicitly./
11-149	11	8	9	8	13	A "projection": depnds entire;y on initial assumptions, which in this case is a continuity of the past [VINCENT GRAY, NEW ZEALAND]	We do not understand this comment. A climate model will respond to external forcing which is different from the past for instance.
11-150	11	8	9		23	In addition to the arguments advanced above, the attempt to differentiate projection and prediction founders because the quantity shown in Figure 11.1 is temperature _anomaly_ which already has a departure from a mean condition (which differs from model to model) subtracted out. There would be a better claim to _prediction_ if the quantity plotted were temperature, not temperature anomaly. If the base state is taken as the initial period, it would seem that the two quantities become one and the same. [Stephen E Schwartz, USA]	I am afraid we don't see the contrast between taking the difference from the base state and the difference from some other average as far as distinguishing between prediction and projection.
11-151	11	8	12	8	13	The authors should make clear that such an ensemble does not necessarily cover the full range of possible evolutions. As stated in Chapter 11, page 21, line 13, in Chapter 11, page 44, line 4/5 and also in Chapter 9, page 20, line 1, it is possible that the climate evolves in a way that diverges from any simulation result so far obtained. An additional sentence along those lines should be added here. [Gregor Betz, Germany]	This is certainly correct. The text now reads "a range of possible evolutions" rather than "the range of possible evolutions". I have avoided an explicit statement to the effect that all possible evolutions are not represented, since this seems inherent (and also would seem to suggest that other important outcomes are somehow missing).
11-152	11	8	15	8	44	This subsection should include an additional paragraph on "possibilistic climate prediction", i.e. descriptions of climate evolutions which are only claimed to be possible (or plausible), without assigning probabilities. This seems to be particularly appropriate as this chapter does discuss possible evolutions of the climate as well as climate scenarios. [Gregor Betz, Germany]	Not sure I understand this comment. The point of climate prediction/forecasting is to be quantitative and to provide skill measures. This is very different from speculation as to possible climate variations and changes. One could state this directly but it seems implicit.
11-153	11	8	16	8	16	Having introduced the T(t) formalism, make explicit that prediction is attempting to determine also Ti and therefore T (or at least statistics of them) [Robert Colman, Australia]	The reference to climate projections has been removed in order not to confuse prediction with projection.
11-154	11	8	16	8	19	Again, it is important to state here that near-term climate prediction depend on the initial conditions. They are	Based on the comment, the description of the nature

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						conceptually identical to seasonal predictions (which can be considered as very short-term climate predictions). The only difference is related to the components of the climate systems which it is necessary to include (and initialise) in the models to make the predictions and in the main features of the statistics of which we want to predict the evolution (the average climate over a month or a season for seasonal predictions, the average climate over 5/10 years for decadal predictions). [Susanna Corti, Italy]	of a prediction has been expanded to include seasonal to decadal timescales and and local to global space scales. Initial conditions are now mentioned explicitly.
11-155	11	8	16	8	25	A prediction and forecast are not much use unless they are successful [VINCENT GRAY, NEW ZEALAND]	Skill is assessed in Ch 11.
11-156	11	8	19	8	25	Here it is mentioned that both predictions and projections are made with numerical models etc.. The it is specified that a prediction proceeds by integrating the governing equations from initial conditions. Then one expects to have an equal information on climate projection (i.e. what does one do with numerical models in order to produce climate projections?). If climate projections are mentioned in the sentence above, an explanation is needed here. By the way, also climate projections can be made using statistical models. [Susanna Corti, Italy]	The reference to climate projection is confusing in this section (as noted also in comment 154) and has been removed.
11-157	11	8	21	8	22	"using the equations of fluid mechanics, thermodynamics, cloud physics, radiative transfer etc.": here there is mention of only atmospheric processes, while a climate model include also other components. The new generation of Earth system models used in this WG1 includes also many other components, as ocean, ice, land surface, ... I thus suggest to include also other processes in the list, as ocean dynamics, ice shelf processes, ... [Claudio Cassardo, Italy]	Have avoided a more extensive list since fluid mechanics, thermodynamics, radiative transfer are all part of ocean, ice and land components for instance.
11-158	11	8	33	8	33	"A deterministic forecast (such as a weather forecast for the next day or two)": actually a deterministic forecast can extend also for several days (see for instance the GFS maps reported on www.wetterzentrale.de, available for 16 days), but its similarity with the true evolution decreases rapidly in a time normally comprised between 2 and 5-6 days. [Claudio Cassardo, Italy]	The intent was to illustrate the idea in terms of the familiar weather forecast but this is now replaced by a more formal statement of the form of the forecast as a numerical or categorical value.
11-159	11	8	33	8	44	Weather forecasts, however deterministic, are not always right, so are also probabalistic [VINCENT GRAY, NEW ZEALAND]	Please see the response to comment 159.
11-160	11	8	36	8	37	"Under some circumstances…" - please clarify. [Antje Weisheimer, UK]	The wording has been adjusted to "Under suitable circumstances" but there is no room here for a discussion of just what these conditions would need to be.
11-161	11	8	43	8	43	Instead of "uninitialized climate projection" (which is tautological) I would write: "uninitialized climate prediction (i.e. a climate projection)". [Susanna Corti, Italy]	The approach here is that a climate projection deals with the externally forced component while a climate prediction deals with both externally forced and internally generated. Would like to avoid the "uninitialized climate prediction" terminology which might confuse.
11-162	11	8	46	9	4	You might consider clarifying that predictability, as introduced here, depends crucially on the model that is used to describe the system, and hence is a characterization of the model rather than of the system itself. [Gregor Betz, Germany]	The comment is appreciated but the current wording is intended to indicate this. Thus predictability is an aspect of the system under study and also model predictability studies give "insight" into system behaviour (rather than delineating it).
11-163	11	8	46	9	4	Branstator and Teng (2010) distinguish between two different sources of predictability: The rate at which the forecast distribution spread and the pace at which the ensemble mean signal weakens. Additionally, the warming trend may bear predictiability. [Holger Pohlmann, Germany]	The displacement and broadening of the pdf are now referred to in order to incorporate this aspect.
11-164	11	8	47	8	57	Need a definition of predictability, not just what measures of it are. Need to also say somewhere here that it provides an upper limit on prediction skill. [Robert Colman, Australia]	This is a delicate task but an attempt to more clearly characterize predictability and its nature as a limit to forecast skill is now incorporated.
11-165	11	8	47	9	4	To test predictability you have to wait to see if it is successful. You cannot do it beforehand. [VINCENT GRAY, NEW ZEALAND]	Noted. This comment confuses "predictability" as defined in Ch 11, with predictive skill. Please see Box and text for detailed description of the diffeerence

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							between the two. No further action required.
11-166	11	8	47		51	Is this roughly 'How predictable is it under the best of circumstances' apart from model and or data deficiencies? [Larry Thomason, United States of America]	The displacement and broadening of the pdf are now referred to in order to incorporate this aspect.
11-167	11	8	54	9	4	In my opinion, this paragraph does not clarify sufficiently an important concept: the difference between meteorological predictability and climatic predictability. Since each meteorological model loses its skill of predicting realistically the true conditions after few days of simulation, it is clear that, from a climatic point of view, the concept of predictability is rather different. This is true also if we consider the "near term" predictions of climate models, or Earth cliamte systems. Climatic predictability is related to the statistical description of the results, and this is also one of the reasons for which the climatic predictability depends on the variable type. This concept is well deepened later, but I think it will be better to deepen also here, as this is an introductive box. Another point is that, here, the argument is the predictability of meteorological variables: other systems variables (e.g. ocean) have different predictabilities. [Claudio Cassardo, Italy]	The chapter is already quite complex and so we have not explained the subtle differences between weather predictability and climate predictability. We have, however, described in detail the difference between climate prediction and weather prediction in FAQ 11.1 and in Box 11.1.
11-168	11	9	3			what kinds of variables are high predictability and how about our ability of predict about these variables?(Qiyong Liu, China CDC) [Qiyong Liu, China]	This is treated subsequently in (renumbered) Section 11.2.3
11-169	11	9	4	9	4	Chapter 11, page 9,line 4, after "forecasts "When comparing stochastic dynamical systems, one need to quantify predictability with a metric, we need to measure, at the mathematical sense of the measure theory, the quantity of information contained in a model, in order to use a relation of order. "The information theory give a mathematical road map, with the concept of relative entropy. After the seminal work of SrinisanVaradhan: in 1982, 1984, Andrew Majda and his colleagues had published a series of milestones: [Andrew Majda, Richard Kleeman, David Cai: A mathematical frame work for quantifying predictability through relative entropy; methods and applications of analysis ; Vol. 9, N 3,pp. 425-444, September 2002]. [Robert DAUTRAY, France]	Although there are references of relative entropy-based measures to estimate both predictability and skill, none of them has been used in decadal forecasting yet. These measures of forecast quality will be introduced in the chapter if the corresponding literature is found, which is not the case yet for SOD.
11-170	11	9	4	9	4	Chapter 11, page 9,line 4, after "forecasts "To quantify predictability with a metric, we need to measure, at the mathematical sense of the measure theory, the quantity of information contained in a model. "The information theory give a mathematical road map, with the concept of relative entropy. After the seminal work of SrinisanVaradhan: in 1982, 1984, Andrew Majda and his collegues had published a series of milestones: [Andrew Majda, Richard Kleeman, David Cai: A mathematical frame work for quantifying predictability through relative entropy; methods and applications of analysis ; Vol. 9, N 3,pp. 425-444, september 2002] using "relative entropy as a measure of predictive information content". [Robert DAUTRAY, France]	as above
11-171	11	9	4			"skiflul" should be "skillful" [David G. DeWitt, USA]	'skillful' is U.S. spelling. IPCC reports use U.K. English, i.e. 'skilful' and 'skilfully'
11-172	11	9	6	9	6	Clarify that forecast is a synonym for prediction. [Robert Colman, Australia]	Page 8, ln 23 indicates this but have replaced "forecast" by "prediction" here since the para discusses "predictability".and is perhaps a better construction.
11-173	11	9	6	9	11	A section on "reliability" of the forecast is needed here. A system can have high predictability but low reliability. Assessing reliability requires a large sample and this is a real issue for decadal forecasts. [Eric Guilyardi, France]	Discussion of the meaning of reliability and an assessment of reliability of the prediction is included in the SOD
11-174	11	9	6	9	11	The term "hindcast" should not be used here since it has different meanings. [Holger Pohlmann, Germany]	Since the term is used in the literature and elsewhere the intent here is to make the meaning explicit.
11-175	11	9	6			Why do you introduce “forecast quality”. In order to be more consistent with the definitions above I would use “prediction quality” [Christof Appenzeller, Switzerland]	Only because it is the more usual term in practice but the two are the same and this is now indicated.
11-176	11	9	7	9	10	Might it also be worth acknowledging that forecast skill will vary with epoch so we should take overal statistics	Agreed. The following text has been added to the

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						with a pinch of salt when thinking about an individual forecast? [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Forecast Quality section: "Forecast skill and other measures of forecast quality are usually calculated as an average over the sequence of hindcasts. Note that the brevity of the hindcast record and inhomogeneities in the initialisation data generally leads to large uncertainty in the estimation of forecast quality measures. Additionally, the skill of seasonal predictions can vary from generation to generation (Power et al. 1999), highlighting the possibility that the skill of decadal predictions might also vary from one period to another. Certain initial conditions might precede more predictable near-term states than other intial conditions, and this has the potential to be reflected in predictive skill. FROM PACO: Meehl et al. (2012, submitted) mentions the issue in the context of decadal forecasting, while Balmaseda et al. (1995) shows its relevance for seasonal time scales.
11-177	11	9	14	9	16	It is incorrect to say that the red line represents the forced part of the observed temperature evolution (only true in the unlikely case that the forced component in the real word agrees exactly with the multimodel mean). [Jouni Räisänen, Finland]	The wording has been changed to indicate that the model results are "estimates".
11-178	11	9	37	11	19	You cannot calculate uncertainty theoretically. It can be found only by success in future prediction [VINCENT GRAY, NEW ZEALAND]	This comment confuses skill with predictability. This is understandable because laypeople do not make the distinction that climate scietists make between these two terms. Please see Box for detailed discussion of this issue.
11-179	11	9	39			what main future situation can be considered as uncertainties? give some examples.(Qiyong Liu, China CDC) [Qiyong Liu, China]	Examples are given in the subsequent subsections.
11-180	11	9	41	9	41	are essential [Robert Colman, Australia]	accepted
11-181	11	9	43	10	51	Because the multi-models have been used in CMIP5, some models have the same (or similar) physical processes. Climate model genealogy should be mentioned in this section. Reference: Masson and Knutti, GRL, 2011 [Zong-Ci Zhao, China]	The issue of model dependence is now expanded in response to this comment: "Model-to-model dependence is evident: certain model subsets e.g models developed by the same institution, sometimes tend to cluster together in their ability to simulate past climate (e.g., Pennel and Reichler, 2011; Masson and Knutti, 2011; Power et al. 2012). Interdependency has the potential to reduce the range of possible future states. It should be noted, however, that the degree of inter-dependency in simulating the past might overestimate co-dependency in projected changes (Power et al. 2012)".
11-182	11	9	46			This sentence suggests that internal variability is superimposed to the externally forced component. I believe that "may be thought of as superimposed" is more accurate. Although this additive viewapplies mostly to temperature, as Box 11.1 indicates. [Ramon de Elia, Canada]	No suggestion of linear addition is implied by this rather general statement, the natural variability will in general be modified by the forcing as well as the mean state.
11-183	11	9	48			Both IPO and PDO are used - is this OK? Are they really different? [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Yes the IPO and PDO are different. The PDO is centred in the North Pacific. It represents a convolution of variabiltiy linked to El Niño-Southern Oscillation, with variabiltiy linked to the Aleutian Low

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							(amongst other things). The IPO is, on the other hand, a basin-wide phenomenon. Having a signal in which Aleutian Low variability is important is not particularly useful for studies in e.g. the South Pacific. Hence both are used here and in other chapters. The interdecadal signal in the PDO t-series is very similar to the IPO t-series, but the PDO has interannual variabilty that the IPO signal does not, and they exhibit somewhat different spatial structures.
11-184	11	9	49	9	51	I suggest you indicate that this has not normally been done explicitly, although has perhaps been included through the use of multi model ensembles, which sample variability to a degree. [Robert Colman, Australia]	This comment refers to following text: "With projections, no attempt is made to predict the evolution of the internal variability. Instead the statistics associated with the variability based on observations or simulations of the past are sometimes included as a component of the uncertainty associated with the projection". So the fact that this aspect of uncertainty is sometimes included is in fact mentioned. Regarding the suggestion, this will take quite a bit of space to describe accurately as there is a lot of subtelty associated with assigning uncertainty associated with internal var only. These issues are covered elsewhere in the Chapter.
11-185	11	9	54	9	54	The response uncertainty is the model uncertainty. I know that the term model uncertainty is a bit old fashion, but using response uncertainty it appears that this uncertainty is intrinsic to the earth system itself (which it is true to some extent) and not to the insufficient representation of it in the state-of-art Earth system models. [Susanna Corti, Italy]	Taken into account. This section has been revised to clarify terminology.
11-186	11	9	54			“The third is uncertainty concerning the response of the climate system to forcing” To which category would you put “model uncertainty” I guess to the third. Please add it. [Christof Appenzeller, Switzerland]	Taken into account. See response to Comment 11-185.
11-187	11	10	1	10	1	Related to previous point: a definition of "reliable estimate" is needed [Eric Guilyardi, France]	The term has been changed from "reliable" ro robust, which is understood in its usual statistical meaning.
11-188	11	10	7			what kinds of uncertainties about present force arise from?(Qiyong Liu, China CDC) [Qiyong Liu, China]	Factors impacting estimates of present forcing are similar to those impacting past forcing.
11-189	11	10	25	10	25	Avoid 'dimension' as sounds like the uncertainties are of a different nature, or fundamentally worse. [Robert Colman, Australia]	Replaced 'a further dimension' with 'an additional source'
11-190	11	10	25			Dynamical downscaling also introduces uncertainty through internal variability, especially for large domains and noisy variables such as precipitation (Alexandru et al (2007) Internal Variability in Regional Climate Downscaling at the Seasonal Scale. Monthly Weather Review 135:3221-3238.) [Ramon de Elia, Canada]	Accepted - text revised.
11-191	11	10	28	10	28	our recent work (Hu et al. 2012) clearly demonstrated that signal to noise ratios depend on variables, geographic location, and forcing intensity. may add foloowing reference after "Meehl et al., 2007" [Zeng-Zhen HU, USA]	Noted. However, this is a basic and well established point and does not require support with further references.
11-192	11	10	28	10	28	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 (in press and published online). [Zeng-Zhen HU, USA]	This is a reference only. This "comment" is actually part of the previous comment by same reviewer. This comment is addressed above. No further action required.
11-193	11	10	39	10	39	add citation "Hu et al. (2012)" [Zeng-Zhen HU, USA]	Same as above.

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11-194	11	10	48	10	48	add citation "Hu et al. (2012)" [Zeng-Zhen HU, USA]	Same as above.
11-195	11	10	53	10	53	Please insert “Figure 11.4” here. It belongs to chapter 11.2.1. To clarify, please submit to the figures “a” – “d”. [Claudia Mäder, Germany]	accepted
11-196	11	10	53	11	19	Relating present day climate to future changes should be narrated. The metrics are based on the present day climate. But some corelations of variables between present day and future across models might be small. See: Knutti et al., J.Climate, 2010 [Zong-Ci Zhao, China]	this issue is now discussed in section delaing with model inter-dependence
11-197	11	11	1	11	2	It is worth noting that the aim of initialisation is not only to "exploit the predictability of internal variability" but also to correct errors in the forcing datasets and the model response to those forcings. By initialising our models with observed temperatures we can correct any shortcomings in historical forcing datasets. This is particularly important where either historical/present emissions are uncertain and also when there is a large response between models to that forcing - for example, in the case of the model response to aerosols via the aerosol indirect effects. [Nick Dunstone, United Kingdom]	Accepted. This is a good point, and the text has been amended.
11-198	11	11	6			besides qulification of these uncertainties, how to improve our understanding about these uncertainties(Qiyong Liu, China CDC) [Qiyong Liu, China]	There is a secion called 'Realising Poterntial' which deals with such issues
11-199	11	11	9	11	19	Delete “Figure 11.4”, in accordance to the comment “page 10, line 53”. [Claudia Mäder, Germany]	accepted
11-200	11	11	25	15	7	Predicting tomorrow is difficult enough, Decadal and seasonal makes it virtually impossible [VINCENT GRAY, NEW ZEALAND]	The skill of seasonal predictions is discussed in Chapter 11. The skill of decadal systems is discussed at length, and actual estimates of skill are provided. Estimatess of predictabiltiy are also provided. The issue: "If You cannot Predict the Weather Next Month, How can You Predict Climate for the Coming Decade"? is specifically addressed in FAQ 11.1.
11-201	11	11	27	11	27	"and land" should be added. [Holger Pohlmann, Germany]	accepted
11-202	11	11	29	11	30	The concepts of calibrating and veryifying should probably be explained. Perhaps in an Annex. Also, the reference given WMO (2009) does not appear in the reference list. [David G. DeWitt, USA]	The reference has been removed. The concepts of calibration and combination have been described in the SOD. Combination tries to improve the forecasts by merging information from as many skilful sources as possible, while calibration aims at increasing the forecast reliability as is relevant to climate forecast information users. The verification section has been revised in the SOD.
11-203	11	11	29			It is correctly stated that decadal predictions are of interest for socio-economic reasons. However Fig. 11.6 shows that the internally generated potential predictability over land is very limited, thereby reducing its socio-economic value substantially. It is still a challenge to demonstrate that decadal predictions, apart from predicting the trend, have useful skill over land. May be the new CMIP5 results are better. If not I think one should be clear about this. [Reindert Haarsma, Netherlands]	Agreed, and currently achievable skill is discussed in later Sections.
11-204	11	11	32	11	52	It should be noted that while idealised experiments (that use model control runs) are extremely useful for studying internal predictability, in the real world changes in external forcings could dominate (superimpose and/or mask) the processes seen in model control runs. So it should be noted that the processes goverening predictability in idealised experiments using model control runs give do not necessarily provide a guide for predictability in the real world. (see my 4th comment) [Nick Dunstone, United Kingdom]	The intent here is to explain prognostic and diagnostic approaches but it is certainly true that model-based results depend on the verisimilitude of the model. While this has been mentioned earlier, it perhaps doesn't hurt to re-emphasis it as is now done.
11-205	11	11	40	11	52	There are more CMIP5 studies now (eg Swingedouw et al. op cit) [Eric Guilyardi, France]	Now referenced
11-206	11	11	41	11	52	an important paper that is missing from this discussion is Dunstone et al 2011, which shows predictability of the tropical Atlantic atmosphere [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	This paper is now published in GRL and a reference to it is now included here.

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11-207	11	11	47			A number of papers are cited but nor included in the reference list: eg Pohlmann et al. 2004 (J Climate), others are cited twice e.g. on page 16, line 25. [Daniela Matei, Germany]	Corrected
11-208	11	11	48	11	52	The impression given by the first part of the first sentence (There is a broad indication of predictability of sea-surface temperature …) seems to contradict the following sentence. [Jouni Räisänen, Finland]	This is now reworded.
11-209	11	11				Sec. 11.3.1.1: The order of 11.3.1.1.1 and 11.3.1.1.2 should be exchanged. [Holger Pohlmann, Germany]	The ording of the sub-section has been considered and the chapter team asserts that the ordering is correct for the intended message.
11-210	11	11				Still on predictability. There is discussion here about predictability of the AMOC. It seems that inclusion of the phase of the AMOC in is used to distinguish prediction from projection. As if starting the model with knowledge of the phase of the AMOC leads to prediction whereas startng the model absent that knowledge is projection. If this is all that the authors (and the community) can find to distinguish prediction from projection, the whole distinction hardly seems worth the effort. I continue to recommend eliminate any discussion of the distinction. Its all projection; it is all contingent on assumptions on forcings, so that makes it projection. [Stephen E Schwartz, USA]	Section 11.3.1.1 discusses the predictability of the AMOC but doesn't mention "projection" so it is difficult to make the connection here. From a broader viewpoint the usage is that a projection does not attempt to trace out the actual evolution of some quantity but only the "externally forced" part. (GJB)....(THIS IS FROM GAV): I think we need to be very clear on what the distinction between prediction and projection is in our chapter, and make sure it is consistent with other chapters and previous IPCC reports. I think the distinction is more subtle than just "forced" vs. "initial value". Those are the methods. My understanding is that projections are conditional predictions - "if so and so happens then this is liable to happen".
11-211	11	12	7	12	7	Please quantify "small" but predictable changes… [Eric Guilyardi, France]	"small" has now been quantified (<1degC, < 1m)
11-212	11	12	10			"internally generated in" should be "internally generated variability in" [David G. DeWitt, USA]	corrected
11-213	11	12	14	12	15	"several years or more compared to other variables" is unclear. [Jouni Räisänen, Finland]	Yes, poorly stated and now improved. The idea is that predictability timescales are longer for ocean variables.
11-214	11	12	22	12	28	This caption is not clear. The figure as well has too many panels. I would propose to split the figure in two (a, b, c) and (d, e) and to insert more informations and explanations both in the captions and in the related text. [Susanna Corti, Italy]	Yes - Figure has been revamped.
11-215	11	12	24			Msadek et al. reference does not have a year. [David G. DeWitt, USA]	Yes - Figure has been revamped.
11-216	11	12	26			Teng et al. reference does not have a year. [David G. DeWitt, USA]	Yes - Figure has been revamped.
11-217	11	12	28			Missing a period at the end of the sentence. [David G. DeWitt, USA]	fixed
11-218	11	12	30	12	52	Sec. 11.3.1.1.2: The third paragraph explains diagnostic potential predictability hence should be the first of this section. [Holger Pohlmann, Germany]	Diagnostic "potential predictability" is not meant to encompass all measures of diagnostic predictability and the text has been modified with the intention to make this clearer.
11-219	11	12	31	12	32	remove comma after predictability [Philip Rasch, United States of America]	done
11-220	11	12	43	12	52	The "potential predictability" discussed here and visualized in Fig. 11.6 has never been defined, thus the values in the figure can just be used for having an idea about the zones with the highest predictability, without understanding the exact reasons. [Claudio Cassardo, Italy]	This is a good point. The text defines the potential predictability as the ratio of variances and the discussion of (now) Fig 11.4 now indicates it is the variance of decadal means that is referred to.
11-221	11	12	43	12	52	Other studies exist: eg Persechino et al. (2012). Decadal predictability of the Atlantic Meridional Overturning	References now included once preprints became

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Circulation and Climate in the IPSLCM5A-LR model. Clim. Dyn., in revision [Eric Guilyardi, France]	available.
11-222	11	12	46	12	46	Pohlmann et al. (2004): This citation is wrong (2 n's) and missing in the list of references. [Holger Pohlmann, Germany]	Fixed.
11-223	11	12	47	12	47	Pohlmann et al. (2004): This citation is wrong (2 n's) and missing in the list of references. [Holger Pohlmann, Germany]	Fixed.
11-224	11	12	54			Should this figure (11.6) have a reference? [David G. DeWitt, USA]	The Figure (now Figure 11.4) has been updated with available CMIP5 results and the caption rewritten.
11-225	11	12	55	12	57	Which period does the forced signal in Fig. 11.6 represent and how is the baseline defined? [Jouni Räisänen, Finland]	Now noted that the results apply to the early 21st century.
11-226	11	12	55	13	8	There are no units in the caption nor in the figure [Susanna Corti, Italy]	Text changed to make clearer
11-227	11	13	2	13	2	"coupled models": I imagine atmosphere-ocean coupling? [Claudio Cassardo, Italy]	Now reads "coupled climate" models with the assumption that this is understood to include coupling with the ocean and with other components.
11-228	11	13	7	3	7	remove "is" before decreases [Philip Rasch, United States of America]	done
11-229	11	13	7	13	7	"is" should be deleted. [Holger Pohlmann, Germany]	done
11-230	11	13	7			Word "is" at beginning of the line should be deleted. [David G. DeWitt, USA]	done
11-231	11	13	8	13	8	"4-9 years" This statement needs a citation. [Holger Pohlmann, Germany]	Citation given earlier in text. This sentence is part of summary. Citations therefore kept to minimum - especially if already given.
11-232	11	13	14	13	18	the term "dynamical systems" is awkward. It should be explained or alternate words used. Some like "Models described by evolution equations" might work. The following sentence is also unclear. You might mean "Statistical methods alone are (ALSO or SOMETIMES) used to product forecasts" [Philip Rasch, United States of America]	the paragraph has been reworded in the light of the several comments below.
11-233	11	13	16	13	16	alone'? I think you mean that in some centres purely statistical methods are used? [Robert Colman, Australia]	Yes, now reworded.
11-234	11	13	16			Please add the following: “however, they are typically post-processed by statistical METHODS TO REDUCE THE SUBSTANTIAL MODEL BIASES AND MODEL OVERCONVIDENCE”. As a reference please add “Weigel, A. P.; Liniger, M. A. & Appenzeller, C. (2008), 'Can multi-model combination really enhance the prediction skill of probabilistic ensemble forecasts?', Quarterly Journal of the Royal Meteorological Society 134(630), 241-260.” [Christof Appenzeller, Switzerland]	This is felt to be too general a statement. The references given are meant to be examples rather than to be exhaustive. This is, however, probably one of the best references in multi-model forecasting.
11-235	11	13	16			"Statistical methods alone are used" should be "Statistical methods alone are also used" [David G. DeWitt, USA]	Yes, now reworded. (GJB).
11-236	11	13	17	13	18	Similar to what? Mixtures of stastical/dynamical, or purely statistical? [Robert Colman, Australia]	Yes, now reworded. (GJB).
11-237	11	13	17			typo: produce some forecasts [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Yes, now reworded. (GJB).
11-238	11	13	18			more details for decadal prediction should be added.(Qiyong Liu, China CDC) [Qiyong Liu, China]	Yes, now reworded. (GJB).
11-239	11	13	24	13	24	It is not "sparseness, secular change etc" that is a challenge (which Is implied by the sentence), but that those things "make prediction a challenge" [Philip Rasch, United States of America]	Yes, reworded. (GJB).
11-240	11	13	27	13	27	SST-only initialisation is also done for the IPSL model (Swingedouw et al. op cit) [Eric Guilyardi, France]	OK, reference now available (GJB).
11-241	11	13	27			forcing an ocean model with atmospheric observations: one should cite here the studies of Matei et al. (2011) and Yeager et al. (2011), both in revision for Journal of Climate. [Daniela Matei, Germany]	Text subsequently changed to refer to Table 1 rather than to provide a long list of references here (GJB)

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11-242	11	13	32	3	7	Dunstone and Smith (2010) is a predictability study hence should be cited there (11.3.1.1.1). [Holger Pohlmann, Germany]	Meaning of comment unclear. No action taken.
11-243	11	13	32	13	33	As shown by Swingedouw et al. (op cit), the fact that Dunstone and Smith were using a non-physical relaxation term for SST (30 times the observed value) is a serious caveat of their study. Caution should be used here. [Eric Guilyardi, France]	Changed l 25-27 "Approaches to ocean initialization range from assimilating only SSTs and relying on ocean transports to initialize the sub-surface ocean indirectly (Keenlyside et al. 2008)" to "Approaches to ocean initialization range from assimilating only SSTs and relying on ocean transports to initialize the sub-surface ocean indirectly (Keenlyside et al. 2008), although the suitability of this approach is contentious (Dunstone and Smith, 2010) and studies suggest that the amplitude of the relaxation term is a key unknown".(this has been reinserted in the new version)
11-244	11	13	32	13	37	Dunstone and Smith (2010) also showed that assimilating just SST failed to initialise or predict the AMOC in their model [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Changed l 25-27 "Approaches to ocean initialization range from assimilating only SSTs and relying on ocean transports to initialize the sub-surface ocean indirectly (Keenlyside et al. 2008)" to "Approaches to ocean initialization range from assimilating only SSTs and relying on ocean transports to initialize the sub-surface ocean indirectly (Keenlyside et al. 2008), although the suitability of this approach is contentious (Dunstone and Smith, 2010) and studies suggest that the amplitude of the relaxation term is a key unknown".(this has been reinserted in the new version)
11-245	11	13	32	13	53	Within the European FP7 project COMBINE, special attempt has been made to investigate the impact of sea-ice initialization on the decadal predicatbility. Also within the COMBINE project with one model (EC-EARTH) full-field and anomaly initialization decadal predictions have been performed. This is a clean experiment where the only difference is the initialization. The results are now analyzed and can hopefully serve the revised version of this chapter. [Reindert Haarsma, Netherlands]	There is now text referring to the initialization of sea-ice and other quantities and some references given.
11-246	11	13	34	13	34	The crossreference 1.3.2 seems to be wrong. [Holger Pohlmann, Germany]	Yes, should have been 11.3.2. Corrected.
11-247	11	13	35	11	37	Disagree with this statement. Several groupds are in fact initializing sea ice and soil moisture and potentially snow cover. These include NCEP CFSv2, ECMWF, and UKMO. [David G. DeWitt, USA]	The statement referred to decadal prediction only. Changed "The initialization of sea ice, snow cover, frozen soil and soil moisture may all contribute to predictive skill beyond the seasonal timescale although direct initialization of these variables has not yet been attempted" to "The initialization of sea ice, snow cover, frozen soil and soil moisture have all shown to contribute to the forecast quality in seasonal forecasting (Koster et al., 2011; Chevallier and Melià, 2012), although an assessment of their positive benefit beyond the seasonal forecast time scale has not yet been attempted."
11-248	11	13	35	13	37	Sea-ice is currently initialised in many seasonal and decadal prediction systems - albeit perhaps crudely and only preliminary studies have been undertaken as to the benefit! [Nick Dunstone, United Kingdom]	Corrected. Added Chevallier and Melià (2012) and Wang et al. (2012); Matthieu Chevallier, David Salas-Mélia (2012). The Role of Sea Ice Thickness Distribution in the Arctic Sea Ice Potential Predictability: A Diagnostic Approach with a Coupled GCM. JCli Vol 25 No 8, 3025–3038; Wang, W., M.

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							Chen and A. Kumar (2012). Seasonal prediction of Arctic sea ice extent from a coupled dynamical forecast system. Mon. Weather Rev., submitted.
11-249	11	13	35	13	37	The authors could cite here Koster et al. (2010, Nature Geoscience) and Koster et al. (2011, Journal of Hydrometeorology), as well as Hirschi et al. (2011, Nature Geoscience). [Sonia Seneviratne, Switzerland]	Would prefer to leave the statement as is, in the absence of clear evidence of increased skill on s2d timescales.
11-250	11	13	39	15	40	The issue of model drift is treated twice in slightly different context (page 13, line 40-53 and page 15 line 12-40). I think this can be treated in a more concise way. [Reindert Haarsma, Netherlands]	Yes, this is now done.
11-251	11	13				Fig. 11.6: What are the units? [Holger Pohlmann, Germany]	See response to 227.
11-252	11	14	12			The reference Doblas-Reyes et al., 2009 does not appear in the list of references [Antje Weisheimer, UK]	Now added.
11-253	11	14	13			I would suggest to add another recent paper who analyses the impact of different appraoches to represent model uncertainty on seasonal forecasts. Weisheimer, A., T.N. Palmer and F.J. Doblas-Reyes (2011), Assessment of representations of model uncertainty in monthly and seasonal forecast ensembles, Geophys. Res. Lett., 38, L16703,doi:10.1029/2011GL048123. [Antje Weisheimer, UK]	The intent is not to be exhaustive here so the refs are relatively early examples. Nevertheless, added "Weisheimer et al., 2011"
11-254	11	14	17			Please add several more references e.g. "Krishnamurti TN, Kishtawal CM, LaRow TE, Bachiochi DR, Zhang Z, Williford CE, Gadgil S, Surendran S. 1999. Improved weather and seasonal climate forecasts from multimodel superensemble. Science 285: 1548–1550." "Weigel, A. P.; Liniger, M. A. & Appenzeller, C. (2008), 'Can multi-model combination really enhance the prediction skill of probabilistic ensemble forecasts?', Quarterly Journal of the Royal Meteorological Society 134(630), 241-260." [Christof Appenzeller, Switzerland]	The intent is not to be exhaustive here so the refs are relatively early examples. Nevertheless changed "Multi-model approaches (Weigel et al., 2008) are used across timescales ranging from seasonal-interannual (eg. DEMETER, Palmer et al. 2004), to seasonal-decadal (e.g., ENSEMBLES, van der Linden, and Mitchell, 2009), in climate change simulation (e.g., IPCC2007, Chapter 10, Meehl et al., 2007) and in the ENSEMBLES and CMIP5-based decadal predictions assessed in Section 11.3.2." to "Multi-model approaches are used across timescales ranging from seasonal (eg. DEMETER, Palmer et al. 2004, ENSEMBLES, Weisheimer et al., 2009), in climate change simulation (e.g., IPCC2007, Chapter 10, Meehl et al., 2007) and in the ENSEMBLES (van Oldenborgh et al., 2012) and CMIP5-based decadal predictions assessed in Section 11.3.2." Weigel, A. P.; Liniger, M. A. & Appenzeller, C. (2008), 'Can multi-model combination really enhance the prediction skill of probabilistic ensemble forecasts?', Quarterly Journal of the Royal Meteorological Society 134(630), 241-260; Weisheimer, A., T.N. Palmer and F.J. Doblas-Reyes (2011), Assessment of representations of model uncertainty in monthly and seasonal forecast ensembles, Geophys. Res. Lett., 38, L16703,doi:10.1029/2011GL048123; Weisheimer, A. et al. (2009), ENSEMBLES: A new multi-model ensemble for seasonal-to-annual predictions - Skill and progress beyond DEMETER in forecasting tropical Pacific SSTs, Geophys. Res. Lett., 36, L21711, doi:10.1029/2009GL040896.
11-255	11	14	18			Instead of the very general reference to the ENSEMBLES project, I would suggest to use another reference for the ENSEMBLES multi-model ensemble: Weisheimer, A. et al. (2009), ENSEMBLES: A new multi-model ensemble for seasonal-to-annual predictions - Skill and progress beyond DEMETER in forecasting tropical Pacific SSTs, Geophys. Res. Lett., 36, L21711, doi:10.1029/2009GL040896. [Antje Weisheimer, UK]	The intent here is not to be exhaustive but to give some indication of the general ENSEMBLES effort so that the reader can consult the published results if interested.

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11-256	11	14	32	14	36	more examples about predictions of other climatic variables using dynamical models besides temperature.(Qiyong Liu, China CDC) [Qiyong Liu, China]	Precipitation is included in figure 11.8.
11-257	11	14	33	14	33	based on the fundamental physical principles'. Clarify this, or preferably drop, unless you want to get into discussion of what these are, what role parametrisations etc. [Robert Colman, Australia]	Both empirical and dynamical prediction systems have been considered in Ch 11. The dynamical systems are referred to as dynamical forecast systems and are identified as those based on ocean-atmosphere coupled models. The sentence referring to fundamental physical principles has been removed for the SOD
11-258	11	14	33	14	36	Reword -- the subject of this sentence is 3 lines long. [Robert Colman, Australia]	Changed l 33-36 from "Evidence for skilful interannual to decadal temperature predictions using dynamical models forced only by previous and projected changes in anthropogenic greenhouse gases and aerosols and natural variations in volcanic aerosols and solar irradiance was reported by Lee et al. (2006), Raisanen and Ruokolainen (2006) and Laepple et al. (2008)" to "Lee et al. (2006), Raisanen and Ruokolainen (2006) and Laepple et al. (2008) provided evidence for skilful interannual to decadal temperature predictions. Those studies used dynamical models forced inly by previous and projected changes in anthropogenic greenhouse gases and aerosols and natural variations in volcanic aerosols and solar irradiance".
11-259	11	14	38	14	39	First sentence: I thought that the initialisation was implicit in climate predictions. If so this sentence is redundant here, maybe it should be moved to a previous introductory section. [Susanna Corti, Italy]	A more general view of climate prediction has been considered, where positive skill is obtained even without initialization.
11-260	11	14	41			Similar to the above comment, I would suggest to use a more specific reference here, e.g. van Oldenborgh et al., 2011 [Antje Weisheimer, UK]	van Oldenborgh et al. (2012) has been introduced.
11-261	11	14				Doblas-Reyes et al, 2009 reference is not in the reference list. [David G. DeWitt, USA]	corrected.
11-262	11	14				"skilful" should be "skillful" [David G. DeWitt, USA]	IPCC uses UK English. Spelling provided by reviewer is U.S. spelling. No change required.
11-263	11	15	2	15	2	TYPO: "Table 11.1." instead of "Table 11.3.2.1." [Ladislav Metelka, Czech Republic]	corrected
11-264	11	15	2			Should this be Table 11.1? [Thomas Delworth, USA]	yes. Corrected.
11-265	11	15	4	15	5	The article by van Oldenborgh et al. has been accepted. Because it discusses extensively the decadal prediction skill of the CMIP3 ensemble and ENSEMBLES it should be treated more extensively in chapter 11 (see Figures 5-7 therein) . It also stresses the impact of volcanos. [Reindert Haarsma, Netherlands]	corrected
11-266	11	15	12	15	40	This part should be shortened and merged with section 11.2.1.2.3. [Holger Pohlmann, Germany]	Corrected.
11-267	11	15	12			I think this is a very important paragraph. The problem of model error and bias or drift should get more attention in the discussion of near-term predictions. [Antje Weisheimer, UK]	Agreed on the importance of the drift issue. In the limited space, we tried to describe drift behavior and some measures to avoid it during the conduct of decadal prediction experiments.
11-268	11	15	15			Doblas-Reyes et al., 2010 reference is not in the reference list which has as 2010a. Should this be 2010 since there is only one reference in that year for Doblas-Reyes et al.? [David G. DeWitt, USA]	Corrected

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11-269	11	15	16		17	The obvious limitations with this approach need to be discussed critically. [Antje Weisheimer, UK]	Added the following "However, there is usually non-linear relationships between the mean state and the anomalies, which is one of the main limitations of this simple, linear approach". A reference is still needed. A reference from Kharin et al. (under review) has been added.
11-270	11	15	19	15	27	I'm not sure that this discussion accounts for the fact that the a linear a-posteriori bias correction would automatically remove any linear trend within the forecast and that this could seriously harm a decadal prediction where linearly increasing boundary forcing is important. Of course I may have misunderstood though! [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	The idea here is to adjust the trend rather than remove it. This is now expanded on to clarify.
11-271	11	15	19	15	29	bias correction has already been explained (with slightly different equations) on page 13 [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Page 13 discusses inialization and the possible avoidance of bias correction.
11-272	11	15	24			"that estimates" should be "which estimates" [David G. DeWitt, USA]	yes.
11-273	11	15	29			Please add "if the predicted temperature trend differs from the observed trend (Fyfe et al., 2012 -accepted). Similar defining an averaged model drift might be difficult due to the limited number of independent hindcasts available (Gangstø et al, submitted) Gangstø, R., Weigel, A. P., Liniger, M. A., and Appenzeller, C. (2012) 'Comments on the evaluation of decadal predictions', Climate Research, submitted [Christof Appenzeller, Switzerland]	The statement has been modified to indicate this. It is one of the reasons that CMIP5 now calls for decadal forecasts initialized every year. The suggested reference has been provisionally added (there is another reference to this elsewhere). (GJB2)
11-274	11	15	40			It might be worth noting that current model biases are several times larger than some of the signals that are being predicted here so it will be some time before this is achieved. Interannual timescales have larger anomalies of course so it will happen there first. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Since forecasts are ultimately judged by their quality we avoid prejudging the effect of biases here.
11-275	11	15	47	15	47	NRCNA (2010): This citation is missing in the list of references. [Holger Pohlmann, Germany]	National Research Council of National Academies (NRCNA), 2010. Assessment of Intraseasonal to Interannual Climate Prediction and Predictability. The National Academies Press, Washington, 192 pp.
11-276	11	15	47	16	16	Both validation and predictability cannot be assessed in any other way than by successful or otherwise future prediction. [VINCENT GRAY, NEW ZEALAND]	Forecast quality is estimated using hindcasts for the past, which are systematically confronted with the best available observations. This is current practice in weather and seasonal forecasting too. Of course, these are estimates, but they always come along with estimates of their uncertainty.
11-277	11	15	47			Should the reference "NRCNA" be "NRC"? [David G. DeWitt, USA]	National Research Council of National Academies (NRCNA), 2010. Assessment of Intraseasonal to Interannual Climate Prediction and Predictability. The National Academies Press, Washington, 192 pp.
11-278	11	15	47			NRCNA - please explain. [Antje Weisheimer, UK]	National Research Council of National Academies (NRCNA), 2010. Assessment of Intraseasonal to Interannual Climate Prediction and Predictability. The National Academies Press, Washington, 192 pp.
11-279	11	15	53	15	55	describe briefly methods to imporve accuracy and reliability of forecasting.(Qiyong Liu, China CDC) [Qiyong Liu, China]	Modified to "The reliability, which is a property of the specific forecast system, measures the trustworthyness of the predictions, i.e.measures , how well the predicted probability distribution matches the observed relative frequency of the forecast event. In other words, a probabilistic prediction is considered

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							reliable if a user can rely on it to make a decision, even if the prediction is not skilful. 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 in a multi-model. Furthermore, the reliability can be increased by statistical post-processing of the predictions."
11-280	11	15	55	15	55	I would propose to add here something about the importance of reliability of a probabilistic forecast. The forecast skill it is always stressed, however, it is reliability that represent the necessary prerequisite for a prediction. My point is essentially the following: a forecast can be useful even if its skill is low (compared for example to the climatology), providing that this forecast is reliable. A forecast is defined reliable if the user can rely on a forecast signal when it occurs, even if the signal is only forecast intermittently. To evaluate reliability we need to produce a large number of retrospective predictions, which are supposed to be used (also) to calibrate a posteriori the real time predictions. [Susanna Corti, Italy]	Modified to "The reliability, which is a property of the specific forecast system, measures the trustworthyness of the predictions, i.e.measures , how well the predicted probability distribution matches the observed relative frequency of the forecast event. In other words, a probabilistic prediction is considered reliable if a user can rely on it to make a decision, even if the prediction is not skilful. 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 in a multi-model. Furthermore, the reliability can be increased by statistical post-processing of the predictions."
11-281	11	15				Tab. 11.1: The reference to table 11.1 is wrong and and the table seems to be incomplete. [Holger Pohlmann, Germany]	corrected
11-282	11	16	12			Palmer et al., 2006 is not a refereed paper [Antje Weisheimer, UK]	The correct reference is Palmer et al. (2007): Palmer, T. N., R. Buizza, R. Hagedon, A. Lawrence, M. Leutbecher, and L. Smith (2007), Ensemble prediction: A pedagogical perspective, ECMWF Newsletter, 106, 10-17.
11-283	11	16	13	16	14	any example about staticstical inference in forecast quality assssment?(Qiyong Liu, China CDC) [Qiyong Liu, China]	Added the references García-Serrano and Doblas-Reyes (2012) and Guémas et al. (2012).
11-284	11	16	15			Please replace: "…degrees of freedom are taken into account." BY "…degrees of freedom are taken into account. For example the widely used leave-one-out cross-validation strategy may lead to biased skill estimates for decadal prediction (Gangstø et al, submitted)" Gangstø, R., Weigel, A. P., Liniger, M. A., and Appenzeller, C. (2012) 'Comments on the evaluation of decadal predictions', Climate Research, submitted [Christof Appenzeller, Switzerland]	Changed "…degrees of freedom are taken into account." by "…degrees of freedom are taken into account, which creates problems such as the negative bias in correlation induced by the commonly used leave-one-out cross-validation strategy (Gangstø et al, submitted)"; Gangstø, R., Weigel, A. P., Liniger, M. A., and Appenzeller, C. (2012) 'Comments on the evaluation of decadal predictions', Climate Research, submitted
11-285	11	16	16			The Joliffe (2007) referenence is not in the reference list. [David G. DeWitt, USA]	Corrected for the SOD
11-286	11	16	20	16	27	The paragraph is awkward. You start out discussing the potential for additional predictability from initialization, and then say that use of multiple start dates "indicated generalized temperature skill". Don't you need to also say that use of fewer start dates in the same studies indicated no skill or something like that? I dont think it fair to cite the earlier studies with fewer start dates as they were for different models with different potential for skillful forecasts. [Philip Rasch, United States of America]	Experiments with two start dates don't allow to estimate the forecast quality. More recent experiments with nine or ten verifiable start dates (such as those planned in the CMIP5 experiment) offer the possibility of estimating the different facets of forecast quality, although with large confidence intervals. A historical overview requires describing how the early experiments addressed the problem and the limitations encountered.

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11-287	11	16	20	19	33	All these "predictions" are phony because your "experiments" do not include any attempt to find out whether they can successfully predict anything [VINCENT GRAY, NEW ZEALAND]	The experiments address the issue of estimating the forecast quality of the predictions for several variables using standard methods commonly employed in weather and climate forecasting.
11-288	11	16	29	16	46	The balance between technical detail (too much) and results (none) is wrong in this paragraph. [Jouni Räisänen, Finland]	Paragraph has been re-written.
11-289	11	16	42			The authors are using the abbreviations Assim and NoAssim to differentiate between the initialized and uninitialized forecast experiments. I would rather suggest Init and NoInit. The Assim notation can be very confusing since the decadal prediction community is using it to denote the experiments that assimilate observations and therefore create initial conditions for the hindcast/prediction experiments. [Daniela Matei, Germany]	SOD uses Init and NoInit for all CMIP5 initialized experiments. However, for all references to DePreSys_PP results, which in the literature is referred to widely as Assim and NoAssim for the initialized and non-initialized experiment, the FOD still uses the previous nomenclature.
11-290	11	16	48			a simple table about comparation of main features between the two experiments should be better.(Qiyong Liu, China CDC) [Qiyong Liu, China]	The section on pre-CMIP5 results has been shortened in CMIP5, so including a table giving equal weight to both pre-CMIP5 and CMIP5 was not considered to be a balanced way of presenting the results, especially as appropriate summarizing references exist for the pre-CMIP5 experiments.
11-291	11	16	50	16	52	If the point of this is to show that the initialized systems have better predictions than the unitialized systems, then there is a need for a measure of whether the differences in the correlations between the initialized and unitialized predictions are statistically significant given the limited sample size of the hindcasts. This is a crucial point, and without this assessment I do not see how any statements can be made comparing the unitialized and initialized predictions. [Thomas Delworth, USA]	Section 11.2.3.2 of the SOD includes a detailed assessment of the literature regarding forecast quality assessment techniques. For the Atlantic the SOD notes "The impact of the initialization on the skill, though robust (as shown by the agreement between the different CMIP5 systems), is small. This fact linked to the limited sample available (nine or ten start dates, depending on the forecast time), make the correlation differences between Init and NoInit not statistically significant with 90% confidence. Although some single forecast systems show larger skill improvements, the lack of agreement in the spatial distribution of the skill differences reduces the positive impact of the initialization. " For the Pacific the SOD notes "IPO is barely significant for all forecast systems and shows no consistent impact of the initialization." Moreover, the SOD argues that "The small amount of statistically significant differences found between the initialized and non-initialized experiments does not necessarily mean that the impact of the initialization does not have a physical basis. A comparison of the global-mean temperature and AMV forecast quality using one- and five-year intervals between start dates (Fig. 11.6) suggests that, although a five-year interval sampling allows to estimate the level of skill, local maxima along the forecast time might well be due to poor sampling of the start dates (García-Serrano and Doblas-Reyes, 2012; Goddard et al. – accepted; Doblas-Reyes et al. – submitted). Several signals, such as the skill improvement for temperature over the North Atlantic, are robust as it is found in a large fraction of forecast systems (more than 75%). However, it is difficult to obtain statistical significance

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							with these limited samples. The low start date sampling frequency is one of the limitations of the core CMIP5 near-term prediction experiment. Results estimated with yearly start dates are clearly more robust than with a five-year start date frequency, and offer an increased number of statistically significant results. However, even with one-year start date frequency, the impact of the initialization is similar (Fig. 11.10; note that the CMIP5 multi-model ensemble in this case includes five individual systems instead of eleven). The spatial distribution of the skill does not change substantially with the different start date frequency. The skill and the initialization impact are both slightly reduced in the results with yearly start dates, but at the same time the spatial variability is substantially reduced. Apart from the low sampling of the start dates, the length of the forecasting period is limited to the period over which reasonably accurate estimates of the ocean initial state can be made, which starts around 1960. This fact also limits the sample size to estimate the forecast quality."
11-292	11	16	50			I would find it most natural to start this section with a discussion of the model biases which are normally removed from the data before the skill is computed. Unfortunately, besdie of the notion on page 15, line 12 that the climate drift is "one of the most serious difficulties in climate prediction",I cannot find any discussion of model biases and model drift which is very disappointing giving its relevance. To my opinion, this needs to be addressed in the next version of the report. [Antje Weisheimer, UK]	An illustration of the systematic error and drift in temperature will be included in the SOD (Paco). My response would be that we agre that biases are a problem but here we dealing with scores which speak for themselves. Not clear we need more discussion but this could be debated. A figure to illustrate model drift and systematic error has been added to the SOD.
11-293	11	17	11			"one-side" should be "one-sided" [David G. DeWitt, USA]	Taken into account - text revised.
11-294	11	17	21	17	25	There is some ambiguity here. On the one hand, we have the statement that (the differences) "… are not statistically significant ". On the other hand, we have the statement that "… an overall larger skill is found for the Assim experiment …". This is not clear to me. Perhaps one statement refers to local significance, and the other to field significance, but this is not obvious from the text. [Thomas Delworth, USA]	The sentence has been left as the CMIP5 results confirm it. The results are shown in Figure 11.7 and 11.8.
11-295	11	17	28	17	37	The colour scale in the figure 11.8 is not clear. The yellows are too similar. What about introducing other hues, like red, green and so on... As it is is really difficult to distinguish colours in the panels showing the diffferences between initialised and non-initialised predictions (or should I say projections?) [Susanna Corti, Italy]	We have a new version of these plots. In any case, red and green should be avoided together because colour-blind people can't distinguish them.
11-296	11	17	34	17	35	Should there be a description of how this multi-source surface temperature data set is generated? [David G. DeWitt, USA]	We followed the methodology described in van Oldenborgh et al. (2012).
11-297	11	17	39	17	42	Why is a figure needed here if there is no signal for precipitation? [Thomas Delworth, USA]	Mainly to clarify what is meant for users' reference.
11-298	11	17	56	17	56	Where is the skill improved through teleconnections? Please clarify. [Thomas Delworth, USA]	The sentence has been removed as appropriate examples are not yet available.
11-299	11	17	56	18	31	A new paper that has just been accepted for publication in Nature is: (Ben B. B. Booth, Nick J. Dunstone, Paul R. Halloran, Timothy Andrews and Nicolas Bellouin, Aerosols implicated as a prime driver of 20th century North Atlantic climate variability, 2012, Nature, In Press). This work suggests that North Atlantic SSTs (AMO/AMV) have largely been forced by anthropogenic emissions of aerosols and their radiative impact on shortwave radiation via aerosol indirect effects. This work uses the HadGEM2-ES earth system model which, like other CMIP5 generation models, are the first to include comprehensive representation of cloud microphysical effects (such as the 1st and 2nd aerosol indirect effects). If other CMIP5 models give similar	We now include a discussion of the Booth et al. (2012) paper, along with Chang et al. (2011) and others, to highlight the potential importance of radiative forcing changes in North Atlantic variations.

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						results then the fact that the AMO is largely a forced response to anthropogenic emissions, and not simply an internal mode of climate variability, has profound impilcations for our understanding of past and future North Atlantic variability and it's associated climate impacts. In particular, the paradigm seen in idealised studies using unforced control runs where variability in the AMOC is seen to control North Atlantic SST variability may not apply in the presence of strong external forcing. This new work could be cited as an alternative theory in this section and elsewhere in this chapter. Careful consideration of future aerosol pathways could lead to predictions of important climate impacts. [Nick Dunstone, United Kingdom]	
11-300	11	17	59	18	1	Please provide a reference for this statement as I am not aware of any verified (using observations) proxies for the AMOC. At present, oceanographic observations are the only way to make reliable estimates of the meridional overturning circulation, and existing records are short (~10 years or less, see chapter 3, figure 3.12). Otherwise, some clarification is required regarding the results of the model experiments being described. [Chris Roberts, Uk]	Text on this issue has been carefully re-crafted and improved to address this and other issues.
11-301	11	17	60	17	60	"five years" This statement needs a citation. [Holger Pohlmann, Germany]	Pohlmann et al. (2012) added.
11-302	11	17	60	17	60	Here is also a citation missing: Matei, D., J. Baehr, J. H. Jungclaus, H. Haak, W. A. Müller, and J. Marotzke, 2012: Multiyear prediction of Monthly Mean Atlantic Meridional Overturning Circulation at 26.5°N. Science, 335, 76-79, DOI:10.1126/science.1210299. [Holger Pohlmann, Germany]	accepted.
11-303	11	17				Fig. 11.8: The quality of this figure should be enhanced (I cannot see any black dots). [Holger Pohlmann, Germany]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-304	11	17				Fig. 11.9: The quality of this figure should be enhanced (I cannot see any black dots). [Holger Pohlmann, Germany]	Figure 11.9 has been redrawn, and moved to the supplementary material, using the latest CMIP5 results available. The quality has been substantially improved.
11-305	11	18	1	18	1	Is there a citation for this statement? [Thomas Delworth, USA]	Pohlmann et al. (2012).
11-306	11	18	2	18	2	The sentence is unclear. In the previous sentence you indicate initialized models are skillful and uninitialized are not (presumably you have initialized them with observations). Then you say lack of observation might limit the initialization of the predictions. What observations were used in the skillful forecasts, and why would those observations not be used? I am assuming that with "hindcasts" that you are trying to predict observed behavior of the climate system, and not trying to simulate the behavior of another model [Philip Rasch, United States of America]	The sentence has been clarified to indicate that the observations available are subsurface temperature and salinity, and their sparcity prior to the ARGO era appears to limit the ability of a model to successfully predict the AMOC.
11-307	11	18	4	18	4	The North Atlantic does not really stand out (to me) in Fig. 11.8. There are equally large correlations over Africa, the Indian Ocean, and parts of Asia. [Thomas Delworth, USA]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-308	11	18	4	18	19	it should be made clear that the CMIP5 assim is expected to be the most skilful since it includes future volcanoes [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	agreed and revised accordingly.
11-309	11	18	10			"and is" should be "and has" [David G. DeWitt, USA]	accepted.
11-310	11	18	12	18	12	If the differences between the predictive skills of the different systems are not significant, what does this imply about the significance of differences in the predictive skills of the initialized and unitialized predictions, since these differences are of comparable size as the differences among the various prediction systems? [Thomas Delworth, USA]	The assessed literature indicates where the initialized prediction are statistically more skillful than the projections, although the definition of statistical significane (i.e., confidence interval) are subjective. Nevertheless, the initialized prediction tend to significantly more skillful than the projections in limited regions (e.g., North Atlantic). Please see Doblas-Reyas et al (2012, submitted)

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11-311	11	18	21	18	31	This discussions seems too premature and qualitative to include. I suggest deleting this paragraph and associated figure. [Thomas Delworth, USA]	The SOD has been greatly reduced in this discussion.
11-312	11	18	22			"sampling allows to estimate" should be "sampling allows one to estimate" [David G. DeWitt, USA]	accepted
11-313	11	18	34	18	38	Figure 11.10 has 4 panels, but only panels b) and c) are described both in the text and in the caption. [Susanna Corti, Italy]	Figure 11.10 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-314	11	18	34			Should "centred" be "centered"? Also, it might be a good idea to describe what an ensemble-mean centered correlation is. [David G. DeWitt, USA]	No. IPCC reports are written in U.K. English. Second point to be revsited by GB. (Paco's figure).
11-315	11	18	49	18	50	I don't think Van Oldenborgh et al show convincing evidence of significant skill of the IPO [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	We modified the expression from significant to weak (IPO skill) for the Van Oldenborgh study, and added another sentence to state that IPO predictive skill is barely significant in the CMIP5 ensemble.
11-316	11	18				Fig. 11.10: This figure is problematic since no significance test was applied. [Holger Pohlmann, Germany]	Figure 11.10 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-317	11	18				the discussion of AMV should also include Booth et al (2012, Aerosols Implicated as a Prime Driver of 20th century variability within the North Atlantic, Nature, in press) which highlights the role of anthropogenic aerosols [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	We now include a discussion of the Booth et al. (2012) paper, along with Chang et al. (2011) and others, to highlight the potential importance of radiative forcing changes in North Atlantic variations.
11-318	11	19	1	19	3	Is the intiitiave being described that organized by Doug Smith and Adam Scaife? I think that they have a weblink describing the project? [David G. DeWitt, USA]	The SOD has a paragraph and a figure (Fig.11.10) for the most recent prediction results, based on Smith et al. (submitted).
11-319	11	19	1	45	1	Section 11.4 on near-term projections is strongly overlapping with chapter 12, section 12.4 and, to a certain extent, with chapter 14. Firstly this is inefficient, although I think we have come to the conclusion that it is inevitable if we want chapters 11 and 12 to stand in their own right. More seriously though there is potential for inconsistencies between figures, numbers, notaion and conclusions. Some very careful cross-chapter collaboration is going to be required to avoid this. [Matthew Collins, United Kingdom]	Greater coordination is now occurring.
11-320	11	19	18	19	21	The suggestion about the utility from new or planned satellite missions seems odd. One of the primary drawbacks in decadal prediction is the short length of th eopbservational record, and yet it willa long time before such satellites produce long records. [Thomas Delworth, USA]	agreed, text removed.
11-321	11	19	39	19	52	All these "predictions" are phony because your "expeiiments" do not include any attempt to find out whether they can successfully predict anything [VINCENT GRAY, NEW ZEALAND]	This is incorrect. Hindcast skill is discused in detail in Chapter 11.
11-322	11	19	39			"section (Section 11.3) a" should be "section (Section 11.3), a" [David G. DeWitt, USA]	corrected
11-323	11	19	40			"this section we" should be "this section, we" [David G. DeWitt, USA]	first person dropped. Comment no longer relevant.
11-324	11	19	41	19	41	"the internal variability has not been initialized": could you please explain here how the internal variability of a model can be initialized? [Claudio Cassardo, Italy]	The internal variability is initialized in the sense the the prediction use observational estimates of the initial conditions, and as such are attempting to predict the total variability (i.e., internal plus externally forced variability).
11-325	11	19	50			Lobell and Burke 2008 is absent from references. [Ramon de Elia, Canada]	corrected
11-326	11	19	54	46	17	All of these are mere "pojections" based on whether you believe the initial asumptions. Since they depend on the highly unbelievable climate models it will be no surprise that none of them will be as successful as the weather forecasts, , which are actually tested against reality [VINCENT GRAY, NEW ZEALAND]	The fidelity of initialized predictions depends on fidelity of models, observing systems and how the imperfect models are intiailized. Climate models exhibit

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							strengths and weaknesses in simulating past climate. See Chapter 9. The suggestion that models are not evaluated is incorrect. The hindcast skill of seasonal and decadal predcitions are discussed in the chapter. Simulations of past climate are evaluated in Chapter 9.
11-327	11	20	14			11.4.2.1.1 It would be useful to include a figure of the spread in trends (e.g. like in fig 3 in Easterling GRL doi:10.1029/2009GL037810) [Gareth S Jones, UK]	The addition of these kinds of figures, which are indeed very useful, was discussed but it was not thought worth the extra space as the current graph already contains the most important information.
11-328	11	20	20	20	20	"11.25 (a)": I think it is intended "Figure 11.11a". [Claudio Cassardo, Italy]	corrected
11-329	11	20	20	20	20	Please correct the number, it seems to be “11.11 (a)” instead of “11.25 (a)”. [Claudia Mäder, Germany]	corrected
11-330	11	20	20	20	20	TYPO: "11.11. (a)..." instead of "11.25 (a)..." [Ladislav Metelka, Czech Republic]	corrected
11-331	11	20	21	20	25	What is the interest to publish such a wide range of uncertainty ? By comparing with the weight of an adult person, is it 40 kg or 120 kg or even lighter or even heavier ? Should the person be treated for anorexia or for obesity ? [François GERVAIS, France]	The estimated range of uncertainty is wide for this particular projection. This is a valid estimate. We agree that this level of uncertainty reduces the extent to which community will find the projections useful. However, our task is to provide a sober assessment of the signal and its associated uncertainty, no matter how large or small these things are actually estimated to be. It is important that readers are aware of the high degree of uncertainty in this particular projection
11-332	11	20	27			The concept of "True uncertainty" has several definitions and some people believe is meaningless in this context (and the fact that has been used in past IPCC reports does not change this). I propose that it should be either carefully defined or eliminated from the sentence by changing it to something less controversial such as "A trustworthy estimation of uncertainty... ". In any case, the issue is complex and I think it should be circumvented as much as possible in a text like this. For further discussion see for example page 46 of Best Practice Approaches for Characterizing, Communicating, and Incorporationg Scientific Uncertainty in Climate Decision Making by Granger Morgan et al eds, 2009, available on line at downloads.globalchange.gov/sap/sap5-2/sap5-2-final-report-all.pdf. (this text is also used as a reference in Mastrandrea et al 2010) [Ramon de Elia, Canada]	Accepted - text revised.
11-333	11	20	48			Lean and Rind (2009) reference is not in the reference section. [David G. DeWitt, USA]	accepted.
11-334	11	20	54	20	54	This paragraph could also discuss the possibility of rapid methane and/or BC mitigation [William Collins, United Kingdom of Great Britain & Northern Ireland]	This placeholder paragraph has been expanded, but focus retained on RCPs. Some discussion of alternative scenarios is now included.
11-335	11	20				Figure 11.11. In my opinion, this figure is likely to suggest to readers that the statistical methods do an excellent job of producing the same result as the full global models, implying we could just as well use statistical models rather than physical models. To make the point that this result in fact has little to do with the statistical models, I suggest that a simple linear extrapolation of the 1975-2010 observations should be added. Those show about 0.6 C increase over 35 years. You could also use 1975-2000, with a more rapid rate. Extrapolating those two rates from 2015 forward would show that these also fall within the CMIP5 range I expect, making clear that the statistical methods are not greatly adding to our knowledge at the global scale for surface temperature and hopefully preventing misleading interpretations of this figure. Or the statistical model results might simply be removed. [Drew Shindell, USA]	Noted. However, we disagree that this result implies we could just as well use statistical models. The consistency of the statistical model with the dynamical models is of interest. The proposed comparison with a linear extrapolation is open to the criticism that the choice of start date (here 1975) is arbitrary.
11-336	11	21	1	21	6	Does the range of projected warming include an estimate of internal climate variability? If yes then it should be explicitly stated here again. [Gareth S Jones, UK]	Yes. Text amended.

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11-337	11	21	3	21	5	"it is likely …": this is a general question, but in this specific point makes in me additional doubts. The choice of using this adjective in this situation ("likely" and not "very likely", for instance) is related in some ways to the use of the 5-95 percentiles in the ASK results? How? In Table 1.1 "likely" is associated with 66-100% probability, while "very likely" with 90-100% probability. It is not clear to me how the two things are related with each other. In other words, to allow a conclusion "very likely" instead of "likely", how Fig. 11.11b should be? [Claudio Cassardo, Italy]	Taken at face value the ASK results would support a "very likely" statement. However, as indicated in the text, there are caveats associated with the potential influence of model and forcing biases on the ASK results. For this reason, the "likely" statement has been judged most appropriate.
11-338	11	21	9	21	14	it might be worthwhile stating that the CMIP5 projections produce a "lower bound" on uncertainty, since the real uncertainty must be larger. [Philip Rasch, United States of America]	Rejected. As discussed in the text the ASK results suggest that - for global mean temperature - those CMIP5 models that warm most rapidly may be inconsistent with observations, so for this variable the CMIP5 range may not be a lower bound.
11-339	11	21	11	21	12	Note that the agreement between models does not give any clue about likelihoods whatsoever. I concur with the IPCC Good Practice Guidance Paper on Assessing and Combining Multi Model Climate Projections: "Model agreement is not necessarily an indication of likelihood." (p. 10) AR5 should not say otherwise. [Gregor Betz, Germany]	xx draft response: your first statement - and in particular your use of the word "whatsoever" - is not consistent with what the reference is saying. the difference arises from the inclusion of the words "not necessarily". The degree to which models agree (e.g. on the sign of change) is a single line of evidence that needs to be considered in assessing likelihood (see e.g. Power et al. J. Climate (2012). The wording is therefore correct and does not need to be changed.
11-340	11	21	37	21	46	"This feature (polar amplifaction) is found in virtually all coupled model projections, but the CMIP3 simulations generally appeared to underestimate this effect in comparison to observations…" The paper, Jacobson, M.Z., Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health, J. Geophys. Res., 115, D14209, doi:10.1029/2009JD013795, 2010 shows that fossil-fuel and biofuel soot can account for a significant portion of polar amplification over the Arctic. [Mark Z. Jacobson, U.S.A.]	reference added
11-341	11	21	54	21	54	After “RCP4,5 relative to the” replace“2086” by ”1986”. [Claudia Mäder, Germany]	The caption has been corrected
11-342	11	21	54	21	54	please verify the following afirmation “ relative to the 2086–2005 period” .I suppose that have to be eliminated.The changes from figure 11.12 concern the period 2016-2035 with respect to 1986-2005 [RODICA TOMOZEIU, Italy]	The caption has been corrected
11-343	11	21	54			REPLACE: 2086-2005 WITH 1986-2005 [Christof Appenzeller, Switzerland]	The caption has been corrected
11-344	11	21	54			"2086-2005" should be "1986-2005" I think. [David G. DeWitt, USA]	The caption has been corrected
11-345	11	22	1	22	12	I undestand what is said here but I wonder if it could be made clearer that we see (or will see) significant warming at low latitudes first not so much because it is bigger or occuring faster but because the variability is smaller so changes are more easily observed. I think this is important and is worth be very clear. [Larry Thomason, United States of America]	Agreed. Sentence has been changed to: The regional variation in the (spatially varying) magnitude of the signal relative to the (spatially varying) magnitude of the internal variability has implications for the emergence of the climate change signal.
11-346	11	22	9	11	22	Fig. 11.13 is based on comparison of the forced signal with the interannual standard deviation of temperature (so TOE is only reached when the warming exceeds the interannual standard deviation by a factor of two, or when about 97.5% of all individual years are warmer than the baseline mean). If the same comparison were made by using (e.g.) the interdecadal standard deviation, which would be more relevant for climate change detection, the TOEs would be substantially earlier. If this difference is not clarified in the text, the figure will be most likely missinterpreted. [Jouni Räisänen, Finland]	Accepted - text revised.
11-347	11	22	10			Figure 11.13 quantifies the Time of Emergence” (ToE) of significant warming relative to the recent past (1986–2005). I think this reference period is not adequate for the concept of “Time of Emergence” (ToE) of significant warming” since the reader expects the emergemce time relative to an undisturbed climate. Note that I very much appreciate that for all other plots the reference time 1986–2005 is used. [Christof Appenzeller,	Rejected. The text already states that "in many low latitude regions significant local warming, relative to pre-industrial climate, has already occurred". It is important that the projections in Chapter 11 are

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						Switzerland]	consistently referenced to a common time period: 1986-2005.
11-348	11	22	15			"Mahlstein et al (2011)" should be "Mahlstein et al. (2011)" [David G. DeWitt, USA]	corrected
11-349	11	22	44			Was "NT" define previously. I assume you mean Near-term so phrase should be Near-Term (NT). [David G. DeWitt, USA]	NT' replaced by 'near-term'
11-350	11	22	45	22	46	"Stratospheric cooling extends nearly into the upper troposphere in the high southern latitudes in JJA": actually the lower border of the cooling zone is close to 200 hPa, but polar tropopause during wintertime can be as low as 6 km, corresponding roughly to 450 hPa, thus I think that the blue area does not intrude into the troposphere, but remain in the lowest portion of the stratosphere, or better the tropopause. [Claudio Cassardo, Italy]	Accepted - text revised.
11-351	11	22	48	22	53	Why the discussion about surface relative humidity is joined with that on the free troposphere temperature? In my opinion, discussion on surface relative humidity should be better joined with that on the surface temperature. [Claudio Cassardo, Italy]	This figure has been dropped and the discussion of RH now appears later in the water cycle section
11-352	11	23	11			Missing period at end of sentence. [David G. DeWitt, USA]	corrected.
11-353	11	23	17	23	17	Should be Lambert and Webb (2008)! [Francis Hugo Lambert, United Kingdom of Great Britain & Northern Ireland]	corrected
11-354	11	23	26	23	31	Initially it is said that there is consistency at large scale and large uncertainty at smaller scales. Later, it is specified that there are some large regions (tropics + subtropics) in which all (or most of) models agree in the sign of the signal, while over other smaller regions located at the border between the above mentioned large regions (and, I imagine, some other mid-latitude regions) the models disagree in their predictions. Thus, the division is not between large and small regions, but between specific geographical areas. In fact, generically in a small tropical region, the models are consistent. I think this paragraph need to be rephrased. [Claudio Cassardo, Italy]	This has been clarified by insertion of following text: 'Large uncertainties in the sign of projected change were seen especially in regions located on the borders between regions of increases and regions of decreases. More recent research has highlighted the fact that if models agree that the projected change is small relative to internal variability in some sense, then agreement on the sign of the change is not expected (Tebaldi et al. 2011; Power et al. 2012). This recognition led to the identification of subregions within the border regions, where models agree that projected changes are either zero or small (Power et al. 2012). This, and other considerations, also led to the realisation that the consensus amongst models on precipation projections is more widespread than might have been inferred on the basis of the projections described in AR4 (Power et al. 2012)'.
11-355	11	23	55	24	5	There is, obviously, a clear link between the energy and hydrological cycle, which consists in the latent heat-evapotranspiration flux, a common component in the two cycles (although it is normally expressed in different units). Over land, the sensible heat flux helps in determining the value of the latent heat flux, while over the oceans the role of sensible heat flux is less important. [Claudio Cassardo, Italy]	We agree, some changes have been made to the text, so as to bring out the point more clearly.
11-356	11	23	56	23	56	changes in precipitation [Robert Colman, Australia]	Editorial. P was changed to precipitation
11-357	11	24	1			Add sentence at end of sentence ending on this line. Sentence to add is:"Giannini (2010) explains the disagreement in sign of the project precipitation change in the Sahel region of Africa with the relative dominance of local/terrestrial processes related to the direct radiative effect of GHGs on the energy balance and evaporation, versus the remote/oceanic warming and associated influence on global tropical vertical stability." Reference for this paper is: Giannini, A., 2010: Mechanisms of climate change in the semiarid African Sahel: The local view. J. Climate., 23, 743-756. [David G. DeWitt, USA]	Accepted. Simplified version of proposed sentence has been added.
11-358	11	24	11	24	11	add citation "Hu et al. (2012)" [Zeng-Zhen HU, USA]	Accepted. Sentence added.

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11-359	11	24	12	24	15	Please mention the size (~2500 km) of the regions used. On smaller scales, internal variability of precipitation is much stronger, and would thererefore dominate the near-term uncertainty even more strongly (e.g., Fig. 10.27 in IPCC WG1 AR4). [Jouni Räisänen, Finland]	Accepted - text revised.
11-360	11	24	17			Which Power et al. (2011) there are two in the reference list. [David G. DeWitt, USA]	corrected.
11-361	11	24	18			Tebaldi et al (2011) reference is not in the reference list. [David G. DeWitt, USA]	accepted.
11-362	11	24	24		29	It isn't clear to me how this paragraph links to the preceeding or following material. [Larry Thomason, United States of America]	Taken into account. This paragraph follows directly from the previous in discussing sources of uncertainty and the extent to which they are understood. However, the paragraph break has been moved to emphasise continuity.
11-363	11	24	39	25	4	Which is the physical meaning of the high values of standard deviation over the desertic area (from Sahara to Arabia, and generally in the two tropical belts)? [Claudio Cassardo, Italy]	The new version of this Figure includes many more CMIP5 models. As the standard deviation is provided in relative units, regions that have small absolute (but large relative) changes dominate. A sentence referring to the maxima over desertic areas is included now.
11-364	11	24	42	24	42	The reference to Fig. 11.29 seems not correct here. Repeated again in line 51, same page. [Claudio Cassardo, Italy]	agreed and will be corrected.
11-365	11	24	42	24	42	should be fig 11.17 [Robert Colman, Australia]	Accepted. Figure number amended
11-366	11	24	51	24	51	should be fig 11.17 [Robert Colman, Australia]	Accepted.Figure number amended
11-367	11	24	51	24	51	Please check the number of the figure. [Claudia Mäder, Germany]	Accepted. Figure number amended
11-368	11	25	15	25	17	The description of the box and whisker diagram should be re-written as it is not clear enough. [David G. DeWitt, USA]	It has been rewritten
11-369	11	25	26	25	34	The evapotranspiration over land can be also limited, during drought conditions, by the unavailability of soil moisture, caused by an excess of evapotranspiration in the previous days. These conditions depend basically on three factors: the history of the previous meteorological conditions, the soil/vegetation types (these two are related to local scale characteristics), and the large-scale meteorological conditions (e.g. anticyclonic patterns, blocking situations, ...). Since these conditions could occur only during a limited time (e.g. part of the summer), their effect can be masked (and thus difficult to establish and quantify) when one looks only at the seasonal mean values. The zones in which these phenomena can occur are zones in which, actually, the seasonal hydrological cycle is already close to arid conditions at least in one season (e.g., generically speaking, the areas between mid latitudes and tropics - some of these areas are mentioned at lines 43-44 later). [Claudio Cassardo, Italy]	The text has been revised and now provides additional references on the topic.
11-370	11	25	31	25	34	This evidence is very much disputed, in particular because of the underlying runoff data and its suitability for the derivation of global scale trends. See AR5 Chapter 2, page 34, lines 32 and following, IPCC SREX (2012, Chapter 3, Section 3.1.5), as well as Legates et al. (2005, Advances in Water Resources). [Sonia Seneviratne, Switzerland]	Text has been revised
11-371	11	25	31			"inclusion of a DGVM" - explain context within a earth system model or coupled to a GCM [Ruth Doherty, UK]	Taken into account. An explanation is included.
11-372	11	25	39	25	44	The decrease of the soil moisture in those areas is generally not uniform during the year, but is large in some seasons (e.g. the summer in the Mediterranean area) and small, null or even reversed (=increment of soil moisture) in other seasons or in other areas (e.g. the winter in the Alpine region). This is important because, in certain zones (e.g. Alpine area), the annual signal can be negligible or small, while the seasonal signal, especially in some seasons (namely, summer) can be relevant, with large repercussions on some activities (e.g. agriculture, but not only). [Claudio Cassardo, Italy]	Text revised

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11-373	11	25	41	25	41	Seneviratne et al. (2010) is not in the reference list of the chapter. [Sonia Seneviratne, Switzerland]	Accepted - Reference added
11-374	11	25	44			"significant" should be "statistically significant". [David G. DeWitt, USA]	Accepted - Text revised
11-375	11	25	50	25	51	Saying that 6-8% is comparable to or even larger than 5-17% is an unbalanced statement. [Jouni Räisänen, Finland]	Accepted - Text revised and changed to "...6-8% is comparable to that simulated in response to radiative forcing changes (11 ± 6%)"
11-376	11	25	53			"increase" should be "increased" [David G. DeWitt, USA]	Corrected
11-377	11	26	3			"Over the oceans evaporation" should be "Over the oceans, evaporation" [David G. DeWitt, USA]	Corrected
11-378	11	26	7			What you mean by natural variability in this context and why isn't total variability the relevant quantity? [David G. DeWitt, USA]	The standard deviation presented in Fig. 11.18 represents the natural variability obtained computing for each model with >3 ensemble members the intra-model variability in these 20-yr means and next averaging these estimates
11-379	11	26	12			"and northern North Atlantic" should be "and the northern North Atlantic" [David G. DeWitt, USA]	Corrected
11-380	11	26	26	26	53	In a recent article Haarsma and Selten argue, by analyzing CMIP3 models, that the weaking of the Walker circulation significantly affects the Northern Hemisphere extra-tropical circualtion, by altering the Rossby wave propogation from the tropics into the extra-tropics. The reference is: Haarsma, R.J. en F.M. Selten (2012) Anthropogenic changes in the Walker Circulation and their impact on the extra-tropical planetary wave structure in the Northern Hemisphere. Climate Dynamics. DOI 10.1007/s00382-012-1308-1 It will appear mid-February online. [Reindert Haarsma, Netherlands]	Manuscript is now cited.
11-381	11	26	38	26	38	I think Scaife et al (Climate Change and Stratosphere-Troposphere Interaction. Clim. Dyn., DOI 10.1007/s00382-011-1080-7) should also be included here [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	Manuscript is now cited.
11-382	11	26	39			The recent paper by Scaife et al 2011, Clim. Dyn. Demonstrates a different northern hemisphere climate change response in high vertical resolution models compared to the AR4 and presents a mechanism. CMIP5 contains several such models. Ref: Climate Change and Stratosphere-Troposphere Interaction. Scaife A.A., T. Spangehl, D. Fereday, U. Cubasch, U. Langematz, H. Akiyoshi, S. Bekki, P. Braesicke, N. Butchart, M. Chipperfield, A. Gettelman, S. Hardiman, M. Michou, E. Rozanov and T.G. Shepherd 2011. Clim. Dyn., DOI 10.1007/s00382-011-1080-7. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Manuscript is now cited.
11-383	11	26	41	26	53	This paragraph states that "that natural variability could dominate the anthropogenically forced response in the near-term" and that the NAO related variability is a key for understanding and quantifying this uncertainty. In my opinion an explicit assessment on the expected near term changes in NAO (or alternatively the AO) variability as for example derived with the CMIP5 models would be helpful for the reader. I am aware of the results in chapter 9, where “it is concluded that here is medium evidence and medium agreement that the NAO is well simulated”, but I strongly believe that such a paragraph should never the less be added. Similar CHAPTER 14 addresses many NAO aspects, but is not providing a corresponding graph. [Christof Appenzeller, Switzerland]	Lacking literature to cite on such a projected near term change, we are limited in what we can assess.
11-384	11	26	44			Some caveat should be added here to say that many models do not easily reproduce the multidecadal shift in the NAO/AO e.g. Scaife et al 2009, Clim. Dyn. [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Manuscript is now cited
11-385	11	26	46			"over coming few decades" should be "over the coming few decades" [David G. DeWitt, USA]	Fixed
11-386	11	26	56			"A key issue for in" should be "A key issue in" [David G. DeWitt, USA]	Fixed
11-387	11	27	9	27	9	"expected until 2070": I suggest to be less precise, as it is not yet sure we have reached already the minimum.	Have adopted the range "2060s-2070s"

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						Furthermore, see page 5 line 11 in which the year 2060 is mentioned: it is better to standardize. [Claudio Cassardo, Italy]	
11-388	11	27	15	27	21	Which is the exact period over which the mean values of Fig. 11.20 are evaluated? In the caption, is mentioned only the period used as reference (1986-2005), while in the text it is said "the later decades of the 20th century". [Claudio Cassardo, Italy]	The text refers to the range over which there has been an observed extension, while the period 1986-2005 is the reference for evaluating projected changes.
11-389	11	27	15		32	As well as GHGs and ozone it has been demonstrated (Haigh, Science, 1996; Haigh et al, J. Clim., 2005) that increases in solar activity result in expansion of the Hadley cells. [Joanna Haigh, UK]	References included.
11-390	11	27	29	27	31	The suggestion in this sentence that internal variability has amplified the recent trends should be spelled out explicitly. Otherwise, internal variability could either accelerate or decelarate the future trends. [Jouni Räisänen, Finland]	We now separate the discussion of internal variability (which should add spread), from that of ozone recovery.
11-391	11	27	34	27	53	This section should also mention the impact of future aerosol forcing on the monsoons. [William Collins, United Kingdom of Great Britain & Northern Ireland]	The monsoon circulations are now further discussed.
11-392	11	27	35	27	35	add 'in the vigour' before 'of atmospheric circulation' [Robert Colman, Australia]	Changed to "strength of atmospheric circulation"
11-393	11	27	40	27	40	Zhan => Zahn [Matthias Zahn, United Kingdom]	not a review comment.
11-394	11	28	22	30	21	I'm not sure where this comment should go (Chapter 11 or 12), but there appears to be no discussion at all about modelled changes in severe thunderstorm activity. Given the relatively large number of papers published since AR4, this seems inconceivable. There is disagreement in the two papers about Australia about the change, but all of the North American studies have found similar results-an increase in convective available potential energy east of the Rockies and a decrease in vertical wind shear, with the former outweighing the latter, leading to an increase in the number of environments supportive of severe thunderstorms. The references follow in the next lines of comments. [Harold Brooks, USA]	This is a useful comment. There is now a brief mention of some of this literature, in particular the Lenderink and van Meijgaard (2008) study.
11-395	11	28	22	30	21	Del Genio, A. D., M.-S. Yao, and J. Jonas, 2007: Will moist convection be stronger in a warmer climate? Geophys. Res. Lett., 34, L16703, doi:10.1029/2007GL030525 [Harold Brooks, USA]	accpeted.
11-396	11	28	22	30	21	Leslie, L.M., M. Leplastrier, and B.W. Buckley, 2008: Estimating future trends in severe hailstorms over the Sydney Basin: A climate modelling study. Atmospheric Research, 87(1), 37-57. [Harold Brooks, USA]	accepted.
11-397	11	28	22	30	21	Niall, S., and K. Walsh, 2005: The impact of climate change on hailstorms in southeastern Australia. International Journal of Climatology, 25(14), 1933-1952. [Harold Brooks, USA]	accepted.
11-398	11	28	22	30	21	Trapp, R. J., N. S. Diffenbaugh, and A. Gluhovsky, 2009: Transient response of severe thunderstorm forcing to elevated greenhouse gas concentrations. Geophysical Research Letters, 36, L01703, doi:10.1029/2008GL036203 [Harold Brooks, USA]	accepted.
11-399	11	28	22	30	21	Trapp, R. J., N. S. Diffenbaugh, H. E. Brooks, M. E. Baldwin, E. D. Robinson, and J. S. Pal, 2007: Changes in severe thunderstorm frequency during the 21st century due to anthropogenically enhanced global radiative forcing. Proc. National Acad. Sci., 104, DOI: 10.1073/pnas.0705494104. [Harold Brooks, USA]	accepted.
11-400	11	28	22	30	21	Van Klooster, S. L., and P. J. Roebber, 2009: Surface-Based Convective Potential in the Contiguous United States in a Business-as-Usual Future Climate, J. Clim., 22, 3317-3330. doi: : 10.1175/2009JCLI2697.1 [Harold Brooks, USA]	accepted
11-401	11	28	22			Please consider elaborating more on SREX in this chapter or other relevant chapters, including the methodology, downscaling approaches, results on global and regional projections (e.g. return periods and uncertainties, etc.). Please also consider providing all WMO Members and/or research community a full and unrestricted access to the CMIP5 projection data for conducting downscaling to assess plausible national / local changes in extreme events in the 21st century. These study results will contribute important inputs for the IPCC AR5 WG1, WG2,	Discussion in this section needs to be brief and cannot cover the theme as detailed as SREX (the respective SREX Chapter 3 is 121 pages, substantially longer than the AR5 Chapter 11). As regards CMIP5 – access to this data set is unrestricted to the full research and WMO community.

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						and WG3 on projected extreme event characteristics and related impact assessments at national and local levels. This also well aligns with the initiative of WMO's GFCS in handling the upsurge in the demand of climate change projections and related information for climate change adaptation and mitigation assessments at national and local levels. [Tsz-cheung Lee, Hong Kong]	We also agree with the thrust of the comment on additional downscaling studies, albeit this research is unlikely to be reflected in the AR5 WGI, due to the current publication schedule that requires submission of peer-reviewed publications in summer 2012.
11-402	11	28	41	28	43	Sillman et al. (2011) also show a regional decrease (over Europe) of cold nights and cold waves in winter associated with blocking. Sillmann, Jana, Mischa Croci-Maspoli, Malaak Kallache, Richard W. Katz, 2011: Extreme Cold Winter Temperatures in Europe under the Influence of North Atlantic Atmospheric Blocking. J. Climate, 24, 5899–5913. doi: http://dx.doi.org/10.1175/2011JCLI4075 [Anthony Lupo, USA]	Paper is now referenced.
11-403	11	28	47	28	53	It seems odd that one has to look at the CMIP5 projections to conclude that "changes in temperature extremes are to be expected already for the near term" when the previous chapter has painstakingly showed that we have detected and attributed changes in extreme temperatures in the historical record. If chagnes have already happened surely we should expect them to continue to change without the results of CMIP5? [Simon Brown, UK]	Good point. Replaced text as follows: "None, of the aforementioned studies has specifically addressed the near term, but detection and attribution studies show that temperature extremes already increase in many regions consistent with climate change projections, and analyses of CMIP5 global projections show that this trend will continue and become more notable."
11-404	11	28	51	28	51	I couldn't see Sillman and Kharin (2011) in the references [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	There are now references to Sillmann et al (2011) and Sillmann et al. (2012, submitted).
11-405	11	28	56	28	59	It would be better to have the same units (either exceedance rate or relative change) in both panels of Fig. 11.22, so to show that the increase in extremely high temperatures is actually much stronger than the increase in the frequency of extreme precipitation. [Jouni Räisänen, Finland]	The main point of this figure is to assess whether the changes in extremes experience a similar increase as changes in mean, for this purpose the current display is better suited. The point that changes in extreme temperatures are more pronounced is evident e.g. from the information about statistical significance (as evident from the figure from the stippling, see also caption).
11-406	11	29	4	29	4	please change "(Figure 11.4.2.1b a-b)" with "(Figure 11.23 a and b)" [RODICA TOMOZEIU, Italy]	Thanks, corrected.
11-407	11	29	5	29	5	"highest changes in the Mediterranean": actually not over the sea, but on land, especially southern Europe. [Claudio Cassardo, Italy]	Suggestion followed, revised text now states "with highest changes over the land portion of the Mediterranean."
11-408	11	29	6	29	6	please change "(Figure 11.22b)" with "(Figure 11.23 b)" [RODICA TOMOZEIU, Italy]	Thanks, figure number was mislabeled.
11-409	11	29	6	29	7	… are projected to warm substantially faster than the mean temperatures - in southern and central, but not northern Europe. [Jouni Räisänen, Finland]	Good suggestion! Text clarified.
11-410	11	29	6			Daytime extreme temperatures (Figure 11.22b) are projected to warm substantially faster than mean temperatures (compare panels (a) and (b)). In my printout figure 11.22b is on very wet days and not on mean temperature. [Christof Appenzeller, Switzerland]	The text should address Fig.11.23b, the text used a wrong figure number. Sorry!
11-411	11	29	6			Fig. 11.23 instead of 11.22 [Ramon de Elia, Canada]	Thanks, figure number was corrected.
11-412	11	29	7	29	7	The maxima affect a belt starting from Spain and Northern Italy up to southwestern Russia, with values larger than 1.5°C. [Claudio Cassardo, Italy]	That's correct, but not implemented due to space restrictions.
11-413	11	29	7	29	9	Regarding the scaling between changes in mean climate and changes in extremes, refer also to chapter 3 of IPCC SREX report (2012; Section 3.1.6). [Sonia Seneviratne, Switzerland]	Added reference to SREX in line 9 of page 29
11-414	11	29	9	29	9	I suggest to change" daytime winter temperatures" with "daytime extreme winter temperatures" [RODICA TOMOZEIU, Italy]	To make things even clearer: high-percentile winter temperatures (see also comment #103)
11-415	11	29	9	29	9	As in the case of JJA mean temperature projections, I suggest to include also for DJF mean temperature	Good point, suggestion followed.

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						projections, a short description concerning the winter pattern of warming and it's intensity. I suggest a following description" In terms of DJF temperature, projection shows a warming of 0.3-1.8°C, with high intensity in the N-NE part of Europe. This configuration of warming tends to persist also to the end of century (ENSEMBLES project, van der Linden and Mitchell, 2009)". [RODICA TOMOZEIU, Italy]	
11-416	11	29	9			Unclear description of image (name of variables). [Ramon de Elia, Canada]	We did not understand this comment, it might relate to some other portion of text or some other figure.
11-417	11	29	10	29	10	"Highest winter temperatures" recommended instead of "daytime winter temperatures" (as much of the difference to the change in the mean temperature likely reflects reduced irregular variability, rather than reduced night-to-day differences). [Jouni Räisänen, Finland]	Text improved using the term "high-percentile winter temperatures", see also comment #418
11-418	11	29	15	29	24	It would be useful to also include the change in the 10th percentile of daily minimum temperatures in Fig. 11.23. In winter, this is probably more interesting than the 90th percentile of the daily maxima. [Jouni Räisänen, Finland]	Is now included in the respective figure.
11-419	11	29	26			An important issue in the cold season is whether precipitaion will be rain or snow. The locations of freezing precipitation are also critical. There have been some recent attempts to assess alteration of freezing precipitation such as the article by Lambert, S. and B. Hansen, 2011: Simulated changes in the freezing rain climatology of North America under global warming using a coupled climate model. Atmos.-Ocean, 49, 289-295. [Ronald Stewart, Canada]	A brief paragraph on this aspect is now included. This is clearly important to many of the precipitation-related impacts.
11-420	11	29	32	29	34	This sentence is hard to follow: In SREX, heavy precipitation was projected to also increase in some (but not all) regions with projected decreases of total precipitation (medium confidence). Can you clarify? [Philip Rasch, United States of America]	The text has been revised.
11-421	11	29	40	29	41	"In general models have difficulties to represent these variations, particularly in the tropics (see Section 9.4.4.2)." This sentence seems an understatement. I think it important to discuss how much confidence we have in GCMs ability to represent the statistics of extreme events (either precipitation or tropical cyclone intensity or frequency). There is little discussion of these issues here that provides insight into why would should have confidence in the model predictions. If models cannot characterize these things accurately today, how much confidence should we have in their ability to predict changes in these quantities in the future. Perhaps this is discussed in other chapters and the special report, but I think it deserves at least a few sentences in this section on extremes. [Philip Rasch, United States of America]	We agree with the concern of the reviewer, but twill need to be addressed post-SOD.
11-422	11	29	43	29	52	Please provide, in addition to the results for Europe, more detailed and quantitative assessment (with diagrams) on the near-term projections of the changes in the probability of occurrence of extreme rainfall events in different regions by CMIP5 model simulations, in particular for those densely populated sub-regions. [Tsz-cheung Lee, Hong Kong]	This comment applies to Europe, and I am not aware of a study that shows this on a global scale. The prime objective of the text is to provide an indication of the type of changes that one needs to anticipate (at the example of Europe). No evidence provided that (justified) comment of the reviewer applies more generally. Publications on CMIP5 will be monitored correspondingly, otherwise no action taken for now.
11-423	11	29	46	29	47	I propose to mention that the north-south gradient of projected precipitation, that tends to keep the configuration also to the end of the century (Ensembles project), is more intense especially during winter season. So, maybe just to include "…a pronounced north-south gradient in the extratropics, especially during winter season, with precipitation increases..." [RODICA TOMOZEIU, Italy]	Good proposal, suggestion followed.
11-424	11	29	49	29	50	"(ii) the large-scale circulation changes": an example of this is the decrease of winter precipitations (mean and extreme) in NW Alps, caused by a change in the regime of circulation. [Claudio Cassardo, Italy]	The main point is that changes in extreme precipitation will be dominated in the near-term by interannual and decadal variations (as is already the case for mean precipitation), while changes in temperature extremes are driven to a significant extent by a large-scale temperature changes modulated by other factors. The example mentioned by the reviewer is relevant, but can be dealt with in the

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							current assessment.
11-425	11	29	50	29	52	The qualitative similarity (same sign of change) also applies to temperature. Thus, the last sentence of the paragraph may not be valid (although it is likely that both the changes in the mean and the variability have a lower signal-to-noise ratio for precipitation than for temperature). [Jouni Räisänen, Finland]	The sentence ("This indicates that changes in variability are less important for precipitation than temperature extremes.") has been removed.
11-426	11	29	54	30	21	This paragraph is one-sided to the point of irresponsibility. It speaks almost entirely to only one global metric… overall storm frequency.. which has been shown repeatedly to be largely irrelevant to society, and omits important references to detection and projections of high intensity events, which are socially relevant. For example, it omits reference to the Elsner and Kossin study of increasing incidence of high-intensity events, and to the work of Emanuel et al. (BAMS, 2008) which projects an increased frequency of high intensity events in many places. In referring to the work of Bender et al. (2010), the paragraph only emphasizes a 60-year emergence time scale at 95% confidence, which is socially irrelevant as no authority charged with planning would ever wait for 95% confidence before acting. This paragraph is not an accurate representation of the consensus of published information, and emphasizes the wrong metrics. [Kerry Emanuel, United States of America]	The SOD includes further discussion of tropical cyclone intensity changes. However, literature on the intensity changes over the next few decades (relative to the late-20th century) is limited. The long-term projections of tropical cyclones are primarily addressed in Chapter 14, this chapter focusses on near-term (up to mid-21st century) changes.
11-427	11	29	54	30	21	Please give more emphasis on the findings of the projections of tropical cyclone trends / changes in other basins, in particular the western North Pacific which is the most active tropical cyclone basin. This Section disproportionally inclined to the research results in Atlantic. In 2009, the ESCAP/WMO Typhoon Committee formed an expert team to assess the impacts of climate change on frequency and intensity in the Western North Pacific and the South China Sea. The first assessment report has been published in 2010. On tropical cyclone projections in the western North Pacific, most of the climate model studies project a reduction in the number of tropical cyclones in the western North Pacific in the 21st century. While there are fewer studies on the change of tropical cyclone intensity, some of the model projections suggest an increase in the number of intense tropical cyclones in the region in a warmer climate. Please also note that the ESCAP/WMO Typhoon Committee has also tasked an expert team to conduct the second assessment on the impact of TC activity in the Typhoon Committee region with a focus on the possible changes in TC track and impact areas, including landfalling statistics/trends. More updated summaries on the projections on the tropical cyclone frequency, intensity and precipitation in the western North Pacific will be incorporated in the 2nd assessment report which will be published later in 2012. References : - Lee, T.C., W.J. Lee, T. Nakazawa, J.C. Weyman, and M. Ying, 2010: Assessment report on impacts of climate change on tropical cyclone frequency and intensity in the Typhoon Committee region, ESCAP/WMO Typhoon Committee, TC/TD-No. 0001. - Lee, T.C., 2012 : A review on the long term variations of tropical cyclone activity in the Typhoon Committee Region, to be published in the Tropical Cyclone Research and Review. [Tsz-cheung Lee, Hong Kong]	The long-term projections of tropical cyclones are primarily addressed in Chapter 14, this chapter focusses on near-term (up to mid-21st century) changes. We will have expanded our discussion of the discussion of other basins, although literature on near-term changes in these other basins is limited.
11-428	11	29	55	30	21	cross reference Box 14.3 and Section 10.6.1.5. [George Kiladis, USA]	will be addressed post-SOD once exact locations known
11-429	11	30	19	30	21	I think this statement is open to some misinterpretation and should be elaborated a little. My interpretion is that there is a 90% chance that future TC frequency, intensity, and spatial distribution will be statistically indistinguishable from past variability, at least during the next decade or two. This seems probably OK, but my suggestion is to put in the sentence an explicit reminder that it applies to "the next few decades". [Thomas Knutson, U.S.A.]	We now include "the next few decades" in this sentence, and will further modify the entire section for the SOD.
11-430	11	30	26	30	26	I woud suggest to add "biogenic" to "chemical-physical processes" [Xuemei Wang, China]	No, the sentence is correct as is, the number of biogenic processes in the atmosphere are limited.
11-431	11	30	33	30	35	Multiple brackets are a little confusing [Ruth Doherty, UK]	Parentheses dropped and full sentences used.
11-432	11	30	48	30	49	In line 48/49, "Technology shifts in agriculture sectors …" are identified as one of the possible influences, but what is omitted is potential changes in diet and food production, which may have a much more profound impact on agricultural emissions (e.g. reduced methane emissions if meat consumption was to change). The focus on agricultural technology seems too narrow and addressing the potential large impact of a wider range of changes in agricultural production (technology, food production, biofuel production etc.) would be beneficial. This potential effect of a change in diet is underpinned by figure 6.3.4 in chapter 6 of the 5AR draft. [Stefan	Sentence rephrased to, "In addition, changes in diet, production of food or biofuels (Chapter 6), technology shifts in the agricultural or energy sectors…"

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						Reis, United Kingdom of Great Britain & Northern Ireland]	
11-433	11	31	19	31	22	Explain why CIMP5 model ensembles are not based on current best understanding of natural and anthropogenic emissions atmospheric chemistry and biogeochemistry, and radiative forcing of climate.What species are involed here? [Hong Liao, China]	This discussion has been overhauled and clarified in SOD. The CMIP5 use a simple, outdated box model to project chemistry (discussed here), as well as carbon cycle feedbacks on CO2 (see Chapter 6).
11-434	11	31	33	31	33	"Y2010": it means year 2010? [Claudio Cassardo, Italy]	Yes, this editorial choice has been made uniform throught the SOD - term Y2010 omitted.
11-435	11	31	33			define Y2010 [Ruth Doherty, UK]	Yes, this editorial choice has been made uniform throught the SOD - term Y2010 omitted.
11-436	11	31	34	31	35	"In our projections" -"our" is confusing. It would be really useful to have a paragraph here or earlier that outlines separate CMIP5 and ACCMIP anlaysis done specifically for this chapter. Are the "RCP&" scenarios only for CH4 abundances? Is the key difference that the ACCMIP simulations are time-slice experiments and "our" simulations are long-term transient simulations. If so that would also be useful to point out. [Ruth Doherty, UK]	This is explained more thoroughly in the SOD, and it is documented in the Annex II
11-437	11	31	40	33	4	In executive summary, “Near term” is defined as future decades up to mid-century. The projected changes in atmosphric components until 2100 are mentioned a number of places in the text. May need to be put more efforts on near term changes. [Hong Liao, China]	This chapter deals with near term climate, but is the only chapter covering changes in atmospheric chemistry and composition (not CO2) through to 2100. Thus we spend equal time on the entire century for composition changes.
11-438	11	31	56	31	57	If possible, it would be helpful to name those factors that account for most of the range of OH trends in the CMIP5 simulations for a single scenario. [Loretta Mickley, USA]	Sorry, this analysis is not done for CMIP5, we are including latest information from ACCMIP papers just now being submitted.
11-439	11	31	57			ranges between +/15%- is this the methane lifetime change or OH mixing ratio change? First mention of CMIP5 ensemble? See comment 15 [Ruth Doherty, UK]	This confused sentence was meant to refer to the CH4 abundances - it is totally rewritten for the SOD, thanks.
11-440	11	32	3	32	7	"Projected CH4"- this sentence is lengthy and rather confusing- "for the same reasons" isn't clear [Ruth Doherty, UK]	Entirely rewritten in the SOD.
11-441	11	32	10	32	11	As comment 15 above "this assessment" is rather hidden in the text, and a separate paragraph to outline the strategy to produce new estimates for this chapter at the start of the section would be useful [Ruth Doherty, UK]	Good idea, the SOD has been totally revised and clarifies the published vs assessed RCPs.
11-442	11	32	18	32	19	"the uncertainty in future abundances is much smaller than the difference between scenarios": this for all scenarios but the couple RCP4.5&-RCP6.0&. [Claudio Cassardo, Italy]	Revised to: "Uncertainty in projecting future abundances is much smaller than the difference between scenarios"
11-443	11	32	37			Link these values to those in chapter 8 [Ruth Doherty, UK]	Done. Also Annex II.
11-444	11	32	45			I'm not sure the example explains "dominant" as there are increasing and decreasing trends noted [Ruth Doherty, UK]	revised to, "Tropospheric O3 changes are driven by anthropogenic emissions of CH4, NOx, CO, NMVOC (AII.2.2.16–18) and are projected to follow these emission trends over the next few decades (e.g., decreasing in all RCPs by 2100 as global NOx declines, but increasing in RCP8.5 due to CH4 increases despite falling NOx emissions)."
11-445	11	32	48			"these" species- could remind the reader what species [Ruth Doherty, UK]	Revised to, "Natural emissions of NOx, particularly lightning NOx, and biogenic NMVOC are also important "

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11-446	11	32	53	32	54	see comments 15 and 18- was some analyses done specifically for this chapter need to be referenced here (or a figure)? [Ruth Doherty, UK]	Results have been updated with submitted papers from CMIP5/ACCMIP
11-447	11	33	26			could add TFHTAP 2010 also [Ruth Doherty, UK]	Accepted. Now we group all references together rather than separating those for emissions from climate since indeed, HTAP 2010 addresses both.
11-448	11	33	42			How do the inherent correlations of many meteorological variables hamper statistical downscaling? Some forms of statistical downscaling use these large-scale relationships and relate these to "local" observations of meterological variables [Ruth Doherty, UK]	Accepted. "inherent correlations" point cut but replaced with "and spatial variations", phrase added: "although downscaling based on air quality relationships with synoptic conditions is likely a more robust approach [e.g., Tai et al., 2012; Appelhans et al., 2012]. "
11-449	11	33	48	33	55	I thought this paragraph well characterized the difficulties associated with making reliable air quality projections and future research topics to be addressed. I was under the impression that differences in ventilation and mixing of chemical tracers between models was also a significant source of variation between models, and if so I think it makes sense to mention it at this point in the manuscript. [William Landuyt, United States of America]	Accepted. Opening sentence now reads, "Reliable air quality projections require confidence in the regional climate responses, including precipitation, convection, mixing depths, subtropical high pressure systems ,and the positioning of mid-latitude storm tracks with associated frontal passages"
11-450	11	34	1			Section 11.4.3.2.1 Isaksen et al. Atmos. Env. 2009 discusses the many processes involved in Climate-driven changes from meteorology and natural emissions. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Now cited in first paragraph.
11-451	11	34	3	34	8	"The observed correlation of high-O3 events with temperature in polluted regions is well documented…" Please include Jacobson, M.Z, On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101, 2008 and Jacobson, M.Z., The enhancement of local air pollution by urban CO2 domes, Environ. Sci. Technol., 44, 2497-2502, doi:10.1021/es903018m, 2010. [Mark Z. Jacobson, U.S.A.]	Accepted.
11-452	11	34	3	34	31	The section on climate-driven changes on ozone well characterizes the primary importance of water vapor abundance (and its increase due to climate change) and how it will drive many of the effects on ozone. While many studies do see increases in ozone levels or pollution events, the subsection doesn't clearly allude to the regional variation differences between many studies (e.g. U.S. EPA, Assessment of the impacts of global change on regional U.S. air quality (2009)). The model comparisons presented in the 2009 EPA publication highlight that many modeling results have contradictory estimates of ozone levels in the same region for future climate simulations, though some areas consistently show elevated levels of ozone (e.g. Northeast U.S.). The lack of reproducibility of spatial variations in future ozone levels under climate change suggests research shortcomings (e.g. downscaling of GCM results, modeling of isoprene chemistry, etc..) that are mentioned in the section, but the effect on the consistency in model output is not clear. [William Landuyt, United States of America]	Accepted. Sentence inserted, "On smaller regions, models are sometimes consistent in the sign of the O3 response to a warming climate (e.g., Northeastern United States) but they often disagree (e.g., the Midwest, Southeast, and Western United States) [Weaver et al., 2009; Jacob and Winner et al., 2009], reflecting variations in the regional climate responses as well as in emissions and chemical feedbacks. "
11-453	11	34	6	34	8	Could add Racherla and Adams (2008) (as given on line 10), Katragkou et al 2011 (as in line 16) and Doherty et al. 2012. Racherla and Adams note "In contrast to the cited earlier work, we do not find unambiguous evidence of synoptic-scale circulation changes driving the increase in O3 episodes." Lang and Waugh 2011, http://www.agu.org/pubs/crossref/2011/2010JD014300.shtml debate whether summertime synoptic frequencies really change due to climate change. [Ruth Doherty, UK]	Accepted additional references. This sentence speaks to observed relationships so does not make any statements regarding the response of stagnation events to a warming climate so those points seem out of place here. We do, however, incorporate these points later, "Models are inconsistent in their attribution of O3 increases to specific processes; for example, changes in summertime cyclones, which ventilate the polluted boundary layer, are model-dependent in many regions [Lang and Waugh, 2011]. "
11-454	11	34	8	34	10	Is this statement based on the direct effect of temperature on ozone chemistry, or on the observed interannual correlation between temperature and ozone? The latter might not have predictive power, because interannual	Taken into account; sentence reviwed as, "Taken together, these multiple lines of evidence indicate that

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						temperature variations are largely circulation-induced (hence the correlation between stagnation episodes and temperature) but the projected long-term warming is not. [Jouni Räisänen, Finland]	surface O3 will increase in some polluted regions as climate warms, in the absence of emission changes. " since the original statement is based on all of the processes that tend to cause O3 to increase with temperature, over many temporal scales. Circulation-induced changes in a warmer climate would indeed influence surface ozone as pointed out by several of the cited references
11-455	11	34	10	34	12	"Surface O3 levels in unpolluted regions are very likely to decrease in a warmer climate because higher water vapor abundances enhance O3 destruction in low-NOx regions." Please clarify that, in unpolluted regions, the higher temperature has virtually no effect itself on the ozone when chemistry alone is considered, and it is only the higher water vapor at low pollution levels that decreases ozone (Figure 1 of Jacobson, M.Z, On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101, 2008). [Mark Z. Jacobson, U.S.A.]	Accepted. Sentence now reads, "Surface O3 levels in unpolluted regions are very likely to decrease in a warmer climate because higher water vapor abundances enhance O3 destruction in low-NOx regions of the atmosphere where the increase in temperature itself has little impact on O3 chemistry". Jacobson reference added.
11-456	11	34	12	34	12	Please clarify that, in polluted air, higher water vapor increases ozone (as do higher temperatures, independently (Figure 1 of Jacobson, M.Z, On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101, 2008) [Mark Z. Jacobson, U.S.A.]	Taken into account. While this is true in terms of specific chemistry under high-NOx conditions (such as illustrated in the cited paper), it is not found to be universally true in all polluted regions - e.g., Camalier et al. Atmospheric Environment, 2007, show negative correlations with RH at eastern U.S. urban sites-which presumably reflects correlation with synoptic conditions which are apparently contributing more to the O3 response than the direct chemical effects. See also the discussion in Jacob and Winner 2009 on this point.
11-457	11	34	14	34	16	Do any of these refences specifically refer to changes in intercontinental transport? [Ruth Doherty, UK]	Yes, Wu et al. 2008b (policy-relevant background). Rather than repeat references multiple times since several processes are discussed in each paper, we group them here.
11-458	11	34	19			Figure 11:25 THTAP 2010 which gives results from 3 CCMs for 2100-2000 of annual average changes in lowest model level O3 are not different to the 2050 ranges given here (e.g., -2.2 %, -1.0%, -1.6%, -3.8% for the HTAP NA, EU, EA and SA regions respectively). Does seperating by metric not give narrower ranges? Note also the pink bars for the A1 scenarios have the widest range. Does this reflect a publications bias? If so this should be noted. Not sure I can follow the lettering at the bottom of the figure. [Ruth Doherty, UK]	This discussion refers only to the blue lines (climate changes) as stated in the first sentence of this paragraph. The A1 bar mentioned refers to emission-driven changes only. Yes "reported O3 statistics" are listed as a contributor to the wide range here - not sure this is addressing the points adequately. Appropriate to follow up with Ruth directly? Will try to improve font etc for final version. Finally, we added a sentence addressing the publication bias point in Section 11.4.3.2.2: "The caveats associated with the climate-only results (Section 11.4.3.2.1) apply here. In addition, the A1 scenario has been applied most frequently for near-term air quality projections (pink bars, Figure 11.25), as reflected in the larger range of estimates as compared to other scenarios."
11-459	11	34	25			figure 11.25 and text, is it possible to comment on spatial resolution differences? For example, do the regional models report higher O3 increases in polluted regions? [Ruth Doherty, UK]	Accepted. Phrase inserted in final sentence of "ozone" section of 11.4.3.2.1, ". The inclusion of several studies reporting spatial ranges in summer daytime statistics, and higher resolution regional models which tend to simulate a wider range of potential changes (e.g., Zhang et al., JGR, 2008),

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							contributes to the wider range of climate-driven changes plotted for 2050 and are not an assessment of changes in uncertainty"
11-460	11	34	33	35	6	The effects of warming due to CO2 on aerosol and ozone pollution and mortality was investigated in Jacobson, M.Z, On the causal link between carbon dioxide and air pollution mortality, Geophysical Research Letters, 35, L03809, doi:10.1029/2007GL031101, 2008 and Jacobson, M.Z., The enhancement of local air pollution by urban CO2 domes, Environ. Sci. Technol., 44, 2497-2502, doi:10.1021/es903018m, 2010, who found an increase in mortality due to increased particulate matter due to warming from CO2. The three main sources of PM increases at the surface were increases in surface stability (increased air temperatures relative to ground temperatures, reducing pollution dispersion), increased biogenic gas emissions and resulting gas-to-particle conversion, and increases in near-surface RH in locations of high humidity, causing aerosol particles to swell and absorb more gases. Increased precipitation offset some of the increased atmospheric PM2.5, but not sufficiently to cancel the gains. An additional source of PM2.5 due to a warmer climate is the increased occurrence of wildfires. [Mark Z. Jacobson, U.S.A.]	Taken into account. Added sentence, "PM in surface air will also increase in locations where surface stability increases, or where particles swell under higher humidity, permitting additional uptake from the gas phase [Jacobson, GRL, 2008]." and revised biogenic sentence: "The biogenic secondary organic aerosol contribution to PM is generally expected to grow as temperatures rise due to enhanced emissions and subsequent gas-to-particle conversion, at least in some regions (Tagaris et al., 2007; Heald et al., 2008; Liao et al., 2007) [insert Jacobson, GRL, 2008]." While changes in precipitation will not offset these positive feedbacks everywhere, the studies cited indicate that in many regions, increases in precipitation are likely to decrease PM so we retain the current phrasing. Wildfires are indeed mentioned as a positive feedback on PM.
11-461	11	34	34	34	44	I suggest adding one recent study as reference for dust-climate connections: Yue X., H. Wang, H. Liao, and K. Fan, Simulation of dust aerosol radiative feedback using the GMOD. Part II: dust-climate interactions, J. Geophys. Res., 115, D04201, doi:10.1029/2009JD012063, 2010. [Hong Liao, China]	Accepted.
11-462	11	34	36	34	39	However, using a combination of observations and results from a chemical transport model, Tai et al. [2012] found that the observed correlations of sulfate aerosol with temperature across the United States do not arise from direct dependence, but rather from covariation with synoptic transport. [Loretta Mickley, USA]	Taken into account. These "direct" effects are expected to play a role but we note that some of the correlation may reflect co-variance with synoptic conditions and cite this reference: "Rising temperatures and water vapor enhance SO2 oxidation relative to surface loss, thus increasing sulphate aerosol and decreasing nitrate aerosol (e.g., Racherla and Adams, 2006; Hedegaard et al., 2008; Unger et al., 2006a; Pye et al., 2009; Liao, Chen and Seinfeld, 2006; Aw and Kleeman, 2003; Kleeman, 2008) though one study suggests synoptic transport is the underlying driver of observed regional correlations between sulphate and temperature (Tai et al., ACPD, 2012)."
11-463	11	34	36	34	39	Tai, A.P.K., L.J. Mickley, D.J. Jacob, E.M. Leibensperger, L. Zhang, J.A. Fisher, and H.O.T. Pye, Meteorological modes of variability for fine particulate matter (PM2.5) air quality the United States: implications for PM2.5 sensitivity to climage change, submitted to Atmos. Chem. Phys., 2012. http://acmg.seas.harvard.edu/publications/tai_2011.pdf [Loretta Mickley, USA]	Accepted.
11-464	11	35	8	35	27	The role of intercontinental transport of ozone (e.g. Holloway et al (2003) or Zhang et al (2011)) seems to be an important point for both global and local air quality and wasn't given much mention in the section, though it was mentioned in the executive summary. I would suggest including a sentence or two regarding the role of intercontinental transport of ozone in local and regional air quality events. [William Landuyt, United States of America]	Accepted. Inserted sentence, "Intercontinental transport of O3 has also been shown to contribute to individual high-O3 events [e.g., Yienger et al., JGR, 2000; Li et al., JGR, 2002; Holloway et al., 2003; Zhang et al., ACP, 2008; TF HTAP, 2010; Lin et al., JGR, 2012].
11-465	11	35	11			could add HTAP 2010 here also. [Ruth Doherty, UK]	Accepted.
11-466	11	35	13	35	14	This text explaining the CMIP5 and ACCMIP simulations is useful, but would also be useful earlier in section	The text now clarifies what is being discussed: i.e.

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						11.4 as in comment 15. [Ruth Doherty, UK]	transient CMIP5 or time-slice ACCMIP simulations.
11-467	11	35	13			figure 11.26- can the RCP& CH4 scenarios be included here? [Ruth Doherty, UK]	Not clear what this means. RCP scenarios are included in 11.26. For consistency, only ACCMIP and CMIP5 models are included here, other studies are shown in Figure 11.25 (including MFR and CLE if that is what reviewer means)
11-468	11	35	29	35	37	Intercontinental transport of particulates (e.g. Liu et al (2009) and Ramanathan and Feng (2009)) is important for understanding global and local air quality and wasn't addressed or mentioned in this section. I suggest including a sentence or two to address the role of intercontinental transport of particulates (possibly in association with ozone as well) in air quality issues. [William Landuyt, United States of America]	Accepted. Now reads, "Seasonal dust events increase aerosols in regions downwind of major source regions, including trans-oceanic transport [Prospero, JGR, 1999; Grousset, GRL, 2003; Huang, JGR, 2008; Chin, ACP, 2007; HTAP 2010]. Other studies document intercontinental transport of aerosols, which can degrade downwind air quality [e.g., Liu et al., 2009; Ramanthan and Feng; HTAP 2010]. One study (Leibensperger et al., Atmos Environ, 2011) suggests that intercontinental influences of NOx and CO emissions on PM can exceed those from SO2 emissions [Liu et al., 2009] particularly in regions with high PM pollution."
11-469	11	35	29	35	37	There is little mention of the role of either organic or elemental carbon individually in this subsection on aerosols. Is this addressed elsewhere, or just not included in AR5? I would suggest an inclusion of black carbon and organic carbon in the discussion of particulates, similar to the extent mentioned regarding sulfate and nitrate. [William Landuyt, United States of America]	Accepted. Oversight. Climate impacts are discussed elsewhere (11.4.6); their projected response to climate is in previous section; here we add emission point: "...generally sulphate follows SO2 emissions and carbonaceous aerosols follow the primary elemental and organic carbon emissions". Related note - Fiore et al., 2012 include PM2.5 projections from CMIP5/ACCMIP models over next century - that figure can be incorporated for SOD and mentioned in this discussion here to help balance discussion of PM with O3.
11-470	11	35	29	35	37	There is no discussion of dust and other natural aerosols in this section, and I suggest adding a sentence mentioning the role of climate change would have on dust and minerals and subsequently on air quality issues (Selin et al., MIT Joint Program Report, 2009. [William Landuyt, United States of America]	Taken into account. Note we are encouraged to avoid citing "grey" literature, so we substitute with peer-reviewed literature - if reviewer could send submitted (by July 31 2012) version of this work, we can cite. This section focuses on the role of emissions but we do now address dust (climate impacts were noted in the previous section but briefly due to space - we now include a reference to a new review (submitted) that discusses these in more detail [Fiore et al., 2012]): "Seasonal dust events increase aerosols in regions downwind of major source regions, including trans-oceanic transport [Prospero, JGR, 1999; Grousset, GRL, 2003; Huang, JGR, 2008; Chin, ACP, 2007; HTAP 2010]."
11-471	11	35	29	35	37	Suggest mentioning the effect of climate change on mercury cycling, though the projections are rather premature at this stage (e.g. Jacob and Winner, 2009) [William Landuyt, United States of America]	Taken into account. The Jacob and Winner discussion of climate change on mercury is referred to in the introduction to 11.4.3 opening paragraph.
11-472	11	35	32	35	32	insert "the" after "of" [Philip Rasch, United States of America]	Accepted.
11-473	11	35	40	35	40	"stagnation events, often concurrent with heat waves": it depends how is defined a heat wave, in particular if	Taken into account. Very important point. We

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						we consider as heat wave a period in which the temperature anomaly with respect to the average value in that period exceeds a determinate threshold. It depends also on the pollutant considered: for instance, O3 peaks are reached during summertime, while particulate and NOx maxima are normally reached during wintertime. Stagnation events are normally (but O3) more numerous during wintertime, thus if a heat wave is defined as stated above, it can occur also during wintertime. [Claudio Cassardo, Italy]	rephrase to say, "sometimes" rather than "often" since "often" reflected a bias towards summertime conditions.
11-474	11	35	40	35	50	Biomass burnings and wildfires also play an important role in ozone pollution events (Jaffe (2011)), and its role could either be cited or mentioned here. [William Landuyt, United States of America]	Taken into account. Added "wildfires" and Flannigan et al., 2009 and Jaffe and Wigder 2012 references: "Positive feedbacks from vegetation (higher emissions and lower stomatal deposition), wildfires, and urbanization may further worsen air pollution during heat waves "
11-475	11	35	42			If appropriate to add more text describing the relevant processes - Vieno et al. 2010 also examines the processes causing high O3 during the 2003 heatwave. Reduced dry deposition and import seem to have a largest influence, other studies suggest isoprene to be important. [Ruth Doherty, UK]	Taken into account. In the next paragraph the sentence discussing feedback processes now reads, "Positive feedbacks from vegetation (higher emissions of O3 and aerosol precursors and lower stomatal O3 deposition) wildfires, urbanization, and shifts in prevailing wind directions, may further worsen air pollution during heat waves" and Vieno reference is added.
11-476	11	35	46	35	47	This is misleading because the simulations by Meleux were done for the last decades of the 21st century. [Twan Van Noije, Netherlands]	Taken into account. Process information is relevant here, we clarify by saying, "by the end of the century".
11-477	11	35	56	35	57	"Positive feedbacks from vegetation (higher emissions": emission of what? [Claudio Cassardo, Italy]	Accepted. Added "O3 and aerosol precursors".
11-478	11	37	10	37	11	"Stechnikov et al. (2010) reference is missing and lead authors name is misspelled I think. Shouldn't it be Stenchikov. [David G. DeWitt, USA]	Corrected spelling. Reference is 2009.
11-479	11	37	16	37	18	The possible occurrence of a Maunder minimum is not considered here. The progressive decrease of sunspots numbers in successive 11 years-cycles, the decrease of associated magnetic field and AP index, point towards this possibility. [François GERVAIS, France]	Text has been amended to include a pointer to Section 11.4.7 on the potential for surprises.
11-480	11	37	34	37	41	A link/consistency check with corresponding section in Chap 9 is needed here [Eric Guilyardi, France]	Further interaction with Ch 9 will occur post-SOD.
11-481	11	37	36			"caused the" should be "caused by the" [David G. DeWitt, USA]	Modified.
11-482	11	37	39			"for recent past" should be "for the recent past" [David G. DeWitt, USA]	Modified.
11-483	11	37	39			"term is an" should be "term an" [David G. DeWitt, USA]	Modified.
11-484	11	37	43	37	49	The walker circulation is not the only factor that can change the thermal structure of the tropical oceans. Meridional transport via Sub-tropical cells for instance, can also play a role. [Eric Guilyardi, France]	Text now mentions the potential roles of heat fluxes and meridional heat transport.
11-485	11	37	49			Missing period at end of sentence. [David G. DeWitt, USA]	Fixed.
11-486	11	38	35	38	38	A link/consistency check with corresponding section in Chap 9 is needed here [Eric Guilyardi, France]	Further interaction with Ch 9 will occur post-SOD.
11-487	11	38	44	38	46	The fresh water flux is not the only factor affecting the AMOC. Can references be provided here to strengthen this suggested dominance ? [Eric Guilyardi, France]	Now mention explicitly the role of temperature changes in AMOC.
11-488	11	39	23			Nothing on glaciers? [Larry Thomason, United States of America]	Glaciers and ice sheets are covered in Ch. 13, and this is now mentioned
11-489	11	39	32		34	“As with all projected quantities for the near-term, there is considerable interannual and decadal variability that confounds the emergence of a forced signal above the noise.” I think this statement needs to be crafted with care, lest it be manipulated in a way the authors do not intend. “Trends in many observed quantities seem to	suggested wording mostly incorporated

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						show evidence of anthropogenic forcing, however, for many of these, the trend exists alongside considerable interannual and decadal variability that hampers our ability to make specific/precise short-term projections". [David Vaughan, UK]	
11-490	11	39	38	39	54	Section 11.4.5.1: CMIP 3 simulations failed to capture the acceleration of sea ice drift observed over the last 3 decades. This has strong consequences in terms of modelled sea ice mass balance (the role of ice export out of the Arctic basin on negative mass balance is strongly underestimated), and so in terms of projected sea ice thinning and decline (Rampal et al. , JGR-C, 116, C00D07, 2011). A first order correction of this effect allows a much better agreement between simulations and observations, and implies that average CMIP3 projections for an ice-free summer in the Arctic by 2100 are much too conservative - i.e. that such event may likely happen much before. [Jerome WEISS, France]	this aspect of the CMIP3 simulations, and the improvement in the CMIP5 models, is now mentioned, and cross-referenced to the more detailed discussion in 12.4.6.1
11-491	11	39	39	39	54	The issue of the disappearance of the summer Arctic sea ice is discussed in detail in Chapter 12. This discussion should not be repeated here. [Thierry Fichefet, Belgium]	this discussion focuses more on the near-term aspects, and is now cross-referenced to discussion and figure in Ch. 12
11-492	11	39	56	40	3	some discussion of the recent increase in Antarctic sea ice, and whether it is simulated by models, should be included to set the context for future projections [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	this aspect of the Antarctic sea ice decreasing recently in the models compared to observed slight increases is now noted
11-493	11	40	7			This sentence doesn't seem to make sense to me. [David Vaughan, UK]	sentence has been re-worded
11-494	11	40	7			It might be worth pointing out that projections of snow-cover duration depends on both precip and temperature being satisfactory within rather narrow limits. So it is inherently not an easy ask. [David Vaughan, UK]	agreed, clarification added and cross-reference to 12.4.6.2 added as well
11-495	11	40	32	40	52	This material is already included in Chapter 12. [Thierry Fichefet, Belgium]	this is now cross referenced to 12.5.5.3, and is focused on the near term
11-496	11	40	34			The rapid loss of sea ice…" this phrase makes it sound like the sea ice was continuously there, and then was lost. The phrase doesn't reflect seasonal fluctuations in sea ice. "The retreat of sea ice to new summer minima…" [David Vaughan, UK]	the word "retreat" is now used as opposed to "loss", as suggested
11-497	11	40	37	40	38	Later research does not support the idea that the loss of Arctic summer sea ice would be irreversible (e.g., Stranne and Björk 2011, Climate Dynamics, DOI 10.1007/s00382-011-1275-y) [Jouni Räisänen, Finland]	reference added
11-498	11	40				Paragraph 11.4.5.1 - The projection of ice-free Arctic before mid-century seems to ignore the 60 years-oscillation and simply extrapolates the warming observed during the ascending sequence of the 60 years-period oscillation observed since 30 years. If a change of tendency is observed in the next fifteen years, it will be a good test of the impact of this oscillation, provided the role of ocean circulation is not larger than that of global temperature. [François GERVAIS, France]	the reviewer does not provide evidence/references for a 60 year oscillation
11-499	11	41	21			Is there any feedback from climate change to land use (required by climate change) and then back to further change? [Larry Thomason, United States of America]	though the CMIP5 models include land use change, it is specified, and there is no conclusive model evidence yet using dynamic land use change to explore this issue
11-500	11	41	39	41	43	It would be very useful to have a table somewhere in the report which describes which CMIP5 models have these effects and which have not. Chapter 11 could possibly link to chapter 6 on this. [Olivier Boucher, France]	table 9.1 provides model characteristics
11-501	11	41	45	41	45	I've seen CH4 described as long-lived, medium-lived, and short-lived in different contexts throughout the report. It would be best to have a consistent way to categorize methane's lifetime. In addition, a rapid response of methane to changes in emissions is debatable and depends on the context. Here, it is being grouped with ozone which has a much more rapid response. Can this be made more clear? [Susan Anenberg, USA]	This section has been overhauled. For the contrast with LLGHGs, we continue to group together here, but rephrase as, "Two major anthropogenic greenhouse gases, CH4 and tropospheric O3, respond rapidly, within a decade, to changes in emissions"
11-502	11	41	48	41	50	Add "while black carbon aerososl produce a net positive RF." [Susan Anenberg, USA]	Taken into account. The net sign from BC is not well established due to uncertainties in the aerosol indirect effect. We move a sentence later in the text

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							expressing this point here.
11-503	11	41	51	41	54	True if you're talking about the total response of global-mean precip, not so true for the slow response that relates to warming itself (see Andrews et al., GRL, 2010). [Olivier Boucher, France]	reference added
11-504	11	42	25	42	36	This section should compare RCP6.0 (not 2.6) and RCP8.5 as 6.0 has the higher aerosol loading, and so will show the greatest regional temperature difference. RCP6.0 is colder globally than RCP2.6 up to and including 2040. [William Collins, United Kingdom of Great Britain & Northern Ireland]	the general issue of aerosol loading in the RCPs, related to recent aerosol measurements, is now being dealt with in terms of greater uncertainty on the lower end of the near-term projections (i.e. the RCP aerosol loading is generally lower than recent observations, thus implying that the RCP simulations could be somewhat warmer than what could actually happen
11-505	11	42	26			You mean less 'warming' rather than less 'cooling', no? [Drew Shindell, USA]	Accepted.
11-506	11	42	38	42	57	The importance of co-emitted species (ie cooling agents emitted with warming agents and vice-versa) has been ignored by many authors so it is easy to make overstatements. See Bond et al, in preparation, 2012 on BC mitigation. [Olivier Boucher, France]	Bond et al cannot yet be cited, as the authors have not released the submitted version to the IPCC review. Nevertheless we have totally rewritten this section and explicitly acknowledge uncertainties with co-emitted aerosols.
11-507	11	42	47	42	49	Indeed, most control strategies since air pollution control began have reduced SO2 and NOx preferentially over BC. Although few studies have examined the climate impacts of health-driven air pollution controls, it could be noted that air pollution controls have historically targetted SO2 and NOx reductions from power plants, and that while these mitigation efforts have successfully reduced acid rain and air pollution health impacts, they likely contribute to warming. Could reference Leibensperger, E. M., L. J. Mickley, D. J. Jacob, W.-T. Chen, J. H. Seinfeld, A. Nenes, P. J. Adams, D. G. Streets, N. Kumar, and D. Rind, Climatic effects of 1950-2050 changes in US anthropogenic aerosols - Part 1: Aerosol trends and radiative forcing, submitted to Atmos. Chem. Phys. [Susan Anenberg, USA]	Accepted. Revised sentence reads, "For example, one study concludes that emission controls on maritime shipping are predicted to improve air quality but increase near-term climate forcing (Collins, Sanderson and Johnson, 2009; Eyring et al., 2010); another study finds that past controls on NOx and SO2 power plant emissions for acid rain and air pollution abatement may have contributed to recent U.S. warming trends (Leibensperger et al., ACPD, Part I)."
11-508	11	42	49	42	52	Multiple modeling approaches indicate that corollary reductions in sulphate-nitrate aerosols occurring for possible aggressive CO2 stabilization or air pollutant mitigation scenarios will produce a rapid rise in surface temperatures." For this result, please see also Streets, D. G., K. Jiang, X. Hu, J. E. Sinton, X.-Q. Zhang, D. Xu, M. Z. Jacobson, and J. E. Hansen, Recent reductions in China's greenhouse gas emissions, Science, 294, 1835-1836, 2001 [Mark Z. Jacobson, U.S.A.]	This reference shows that pollutant controls over China could have slight warming effects over the next century but does not point out the potential for rapid warming which is the point being made here.
11-509	11	43	2	43	2	"aerosol indirect effects" rather than "cloud feedbacks". Mention "co-emitted species" as well. [Olivier Boucher, France]	Accepted.
11-510	11	43	15	43	17	This is a speculative statement. [Olivier Boucher, France]	will be examined post-SOD
11-511	11	44	4	44	5	Sentence beginning "It is possible…": Add a comment to the effect that examples of such possible scenarios were mentioned in section 11.4.6. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted - text amended.
11-512	11	44	36			11.4.7.1 Is it worth mentioning those studies that have included (in one way or another) volcanic eruptions in their projections of future climate change (e.g. Kettleborough et al., Journal of Climate, 2007; Hansen et al., JGR, 1988; Feulner and Rahmstorf ,GRL, doi:10.1029/2010GL042710, 2010) [Gareth S Jones, UK]	accepted, text modified
11-513	11	44	38	44	53	Swingedouw et al. op cit also identifies the Agung volcanic eruption of 1963 as a cause of decadal predictability of the AMOC in a model, i.e a AMOC maximum in ~1975. [Eric Guilyardi, France]	accepted, text modified
11-514	11	44	47	44	47	Suggest add Church et al, 2005, Nature, 438, as reference on this. [Robert Colman, Australia]	accepted, text modified
11-515	11	45	1	45	2	"RCP scenarios assume … no underlying trend beyond 2005" Is this true? Why not an RCP scenario with a weaker future Sun? [Terje Wahl, Norway]	Yes, it is true. At the time the future scenarios were designed the deeper solar minimum in 2009 was not clearly evident, so 'business as usual' was considered

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							the most appropriate strategy. Therefore the recommended solar scenario for the 'future' i.e. 2005-2100 was to take the average of the most recent 3-4 cycles and to repeat this into the future wih constant period and no underling trend. There may be studies in addition to the Feulner and Rahmstorf (2010) paper that examine the impact of a weaker future sun in future, and results from these will be included in future drafts.
11-516	11	45	1	45	16	Para 11.4.7.2 on Future Solar Forcing must be rewritten and expanded. Some examples: [Terje Wahl, Norway]	The reviewer has not been explicit about what he/she believes is missing. However,additional text has been included, in response to other comments, with more information on regional effects and the recent Harder et al measurements.
11-517	11	45	2	45	3	Outdated: "… the Sun is in a grand solar maximum …" [Terje Wahl, Norway]	It is not yet evident that this statement is outdated; the sentence following outlines the possibility that we are leaving the grand solar maximum.
11-518	11	45	6	45	9	I had a quick look at the TSI values used in Jones 2012 and the reduction in mean TSI (for the Lean 2009 reconstruction of TSI) between the two periods is nearer 0.45Wm-2. [Gareth S Jones, UK]	Text amended (0.6 changed to 0.45).
11-519	11	45	10	45	10	"…only an 8%..." I think an 8% probability of entering a MM is significant!! [Nick Dunstone, United Kingdom]	Text amended (the word 'only' has been removed).
11-520	11	45	11	45	12	The reduction radiative forcing in the estimated mean TSI variations by 2050 is approx -0.1Wm-2. This is not taking into account the adjustment that was applied to estimate the AR5 past TSI radiative forcings (see section 8.3.1) [Gareth S Jones, UK]	Accepted. A clarification and reference to 8.3.1 has been added.
11-521	11	45	12	45	12	The term "climate sensitivity" usually refers to temp change with doubling of CO2 (9-64 L10). [Gareth S Jones, UK]	Accepted. Text amended ("climate sensitivity" has been changed to "climate sensitivity parameter").
11-522	11	45	12	45	12	0.5 K Wm-2 is a low value of climate sensitivity (0.8 K Wm-2 is more typical in models, at least for the equilibrium). [Jouni Räisänen, Finland]	Accepted. Text and calculation amended.
11-523	11	45	13	45	16	Last sentence assumes no significant amplification from UV or GCR, without explicitly stating this. [Terje Wahl, Norway]	Text has been amended to include a paragraph on regional effects and issues relating to the recent Hader et al UV effects.
11-524	11	45	16			While this is a reasonable summary of the global solar effects from TSI variations, there is also evidence that much larger regional and seasonal variability may be driven by solar variability (e.g. Ineson et al, Nature Geoscience, 2011) [Adam Scaife, United Kingdom of Great Britain & Northern Ireland]	Accepted - text has been added on regional effects and the recent Harder et al UV measurements.
11-525	11	45	19	46	48	FAQ 11.1: In line with the standard WG1 FAQ style, can an italicised "overview answer" paragraph be produced please, and inserted at the beginning ahead of the smoking analogy paragraph. [David Wratt, New Zealand]	Now provided
11-526	11	45	24	45	31	Ths use of an analogue is good, but I suggest a different one, which is closer to the situation under discussion, does not have connotations of health issues -- dying etc, nor the added complication that it 'might not happen at all from this cause': The analogue is that can predict with confidence that July will be around x dagrees warmer than January in ,say New York, even though we can't predict daily temperatures. Indeed there is even a chance that the occasional day in July might be cooler than Jan, or even that it might be an unusually cool January, but our average prediction is robust. Clarify that although the forcing in this case is stronger than that of GHG's , the principle is the same. [Robert Colman, Australia]	The good alternative analogue provided by reviewer has been used.
11-527	11	45	24	45	31	This is not a very good analogy. There is a substantial chance that a smoker will not get a chronic smoking related illness, but virtually no chance that increases in greenhouse gases will not lead to substantial long-term	An alternative analogy has been adopted.

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						climate change. In other words, the lifetime fate of the smoker is analogous to weather in one day, but to get an analogy for climate change, you should consider the death rate within a large group of smokers. [Jouni Räisänen, Finland]	
11-528	11	45	37			"In fact reliable" should be "In fact, reliable" [David G. DeWitt, USA]	changed
11-529	11	45				Really like Faq 11.1 and FAQ 11.2 (wonder if 11.2 is really necessary though)…. [Larry Thomason, United States of America]	Thank you. These questions are commonly asked, and so FAQs will be appreciated by some non-climate science readers.
11-530	11	45				FAQ 11.1: For the chapeau, we would strongly encourage the use of an analogy from the 'natural sciences' rather than staying into the 'health' sector. For example, one might think of an analogy involving the forecasting of risk from 'avalanches' or other natural hazard. [Thomas Stocker/ WGI TSU, Switzerland]	An alternative analog along these lines has been adopted.
11-531	11	45				FAQ 11.1: The current 4th paragraph should come earlier, introducing the term 'climate prediction' before the term is subsequently used. [Thomas Stocker/ WGI TSU, Switzerland]	FAQ has been rearranged
11-532	11	45				FAQ 11.1: Include some examples in support of the final paragraph that illustrate what can be predicted for some climate variables in some locations, i.e., end with a more positive message. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Changed. Now includes: 'What does the research tell us about our current ability to predict climate over the coming decade? Despite their infancy, decadal prediction systems developed in recent years exhibit statistically significant (though imperfect) skill in predicting near-surface temperature over much of the globe out to at least 9 years. The bulk of this skill is thought to arise from external forcing'.
11-533	11	45				FAQ 11.1: We consider a useful figure in the context of this FAQ would be one similar to the Schaer et al. 2004 (Nature), showing the statistical distribution of summer temperatures for the past and future. [Thomas Stocker/ WGI TSU, Switzerland]	Time did not allow this comment to be investigated.
11-534	11	46	4	46	7	I suggest to eliminate "… or the frequency of days below 5°C". In my opinion, when we speak about "statistics of weather conditions over a long period of time" we include also statistics of extreme weather events (namely, frequency of days below 5°C, or above 90th percentile…).I propose to change " This includes long-term averages of e.g., air temperature and rainfall, as well as the statistics of the variability about their long-term averages e.g., the standard deviation of year-to-year rainfall variability from the long-term average, or the frequency of days below 5°C" with "This includes averages and variances of mean and extreme weather events defined over a long period of time." [RODICA TOMOZEIU, Italy]	The problem with text is not made clear by review comment. The provision of specific examples seems useful. No change made.
11-535	11	46	20	46	30	the discussion of predictability in the FAQ is at a substanitally different level from that of the previous FAQ paragraphs. In previous sections they took time to explain what "weather" is, and what "climate" is. Then in the predicability section they blithely mention "internally generated natural variability" and "external forcing", and "radiative forcing" and "teleconnections" etc, without bothering to define these things at all. In my opinion, these FAQs are for non-technical readers, and if they dont understand "weather" they are certainly not going to understand "radiative forcing". Those technical terms are actually not required to answer the FAQ anyway, and a much more basic approach is possible. If you want advice on approaches, dont hesitate to ask :-) [Philip Rasch, United States of America]	FAQ 11.1 has been further simplified.
11-536	11	46	20	46	46	Since none of these projections have been shown to be capable of successful prediction, the whole exercise is futile [VINCENT GRAY, NEW ZEALAND]	The skill of the predictions are discussed at length. Skill is evident for some variables in some locations. Please see text. No change required.
11-537	11	46	22			The standard WG1 FAQ style does not include references to chapters, since FAQs are designed to be read "stand-alone". Can the reference to Sections 11.2.1 and 11.2.2 be replaced by a list of external forcings ? [David Wratt, New Zealand]	References to chapters have been removed.
11-538	11	46	27	46	30	Rather than a reference to Chapter 11, I suggest a brief summary of the relevant points be provided here, in line with the standard WG1 FAQ stuyle of making FAQs "stand-alone". (From the placeholder text I assume in	References have been removed.

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						fact that this is the intention for the next draft). [David Wratt, New Zealand]	
11-539	11	46	32	46	46	It is true that a decadal signal provides little if any enhancement in our ability to forecast weather. However, the same decadal signal could be related for example to an enhanced/diminished frequency of specific local weather events. It has been show (see for example Cassou et al 2005 - JClim 18 2805-2811) that persisting large scale conditions (which might be of the same amplitude of a decadal climate signal) can affect the frequency of weather regimes which in turns can be related to very anomalous regional weather patterns. Therefore, even if the decadal signal cannot directly impact on our ability to forecast weather, it can provide the information of a change in the statistics of weather events. [Susanna Corti, Italy]	For the sake of brevity, and because this is not directly addressing the question asked in title, this topic has been dropped. No furthr change therefore required.
11-540	11	46	38	46	46	In line with the standard WG1 FAQ style I suggest removing the references to particular chapter sections in this paragraph - and provision of sufficient detail for the text to be understandable "stand-alone" without these references. [David Wratt, New Zealand]	Accepted. Done.
11-541	11	46	51	48	47	FAQ 11.2: The first paragraph "overall summary answer" should be italicised. [David Wratt, New Zealand]	Accepted - will be revised according to suggestions.
11-542	11	46	55	47	57	I suggest that the text be revised to a less technical level. It isn't necessary to refer to "radiative forcing". One can merely say the aerosols scatter sunlight back to space and shade the Earth's surface. It might be worth qualifying the statement about CO2 emissions by indicating it is from one volcano the size of Pinatubo, and compared to present day anthropogenic emissions from 1 year (or whatever the appropriate qualifiers are. E.g. surely the volcanic eruption is as large as the emissions for 1 second, to carry the argument to an extreme). [Philip Rasch, United States of America]	The FAQ has been completely re-written for the non-expert.
11-543	11	46				FAQ 11.2: Please remove the 'Frankenstein" sentence, and remove the paragraph on geoengineering (page 48, lines 12-15) - both are not central to this FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	removed as suggested.
11-544	11	46				FAQ 11.2, Fig 1: We suggest this figure is updated using the latest set of temperature reconstructions from chapter 5. [Thomas Stocker/ WGI TSU, Switzerland]	No longer relevant.
11-545	11	47	12	47	31	This paragraph contains various technical terms (e.g. troposphere, stratosphere, Antarctic Oscillation, advection, …) which I suggest should ether be explained or replaced with a few words of explanation, for the benefit of the general reader. [David Wratt, New Zealand]	No longer relevant
11-546	11	47	22	47	22	Typo: "lower tropospheric" should be "lower stratospheric" [Jouni Räisänen, Finland]	Corrected.
11-547	11	47	38	47	38	"the unknown 1809 eruption": it is better to call "unidentified" than "unknown". And please add these references: Title: Ice core evidence for an explosive tropical volcanic eruption 6 years preceding Tambora. Authors: Dai, Jihong; Mosley-Thompson, Ellen; Thompson, Lonnie G. Publication: Journal of Geophysical Research (ISSN 0148-0227), vol.96, Sept. 20, 1991, p. 17,361-17,366. Title: Two major volcanic cooling episodes derived from global marine air temperature, AD 1807-1827. Authors: Chenoweth, Michael Publication: Geophysical Research Letters, Volume 28, Issue 15, p. 2963-2966 (GeoRL Homepage), 2001 DOI: 10.1029/2000GL012648 Cole-Dai, J., D. Ferris, A. Lanciki, J. Savarino, M. Baroni, and M. H. Thiemens (2009), Cold decade (AD 1810-1819) caused by Tambora (1815) and another (1809) stratospheric volcanic eruption, Geophys. Res. Lett., doi:10.1029/2009GL040882, VOL. 36, L22703, 6 PP., 2009. The first two describe the eruptions, but at that time there was not evidence about the fact it was a stratospheric eruption. The latter paper describes, based on isotopic data, that this eruption in 1809 was a stratospheric one. [Claudio Cassardo, Italy]	Noted - we will change it to "unidentified" but FAQs do not include references, so we cannot specifically refer to these papers.
11-548	11	47	39	47	39	I suggest to remove the citation of Frankestein… [Claudio Cassardo, Italy]	Accepted - revised according to suggestions.
11-549	11	47	46	47	47	"very large stratospheric aerosol clouds and large climatic effects": if you call "very large" and "large" these effects, how do you will call the effects of Mt. Tambora and Mt. St. Helena ruptions? [Claudio Cassardo, Italy]	Accepted - text revised.
11-550	11	48	1	48	47	FAQ 11.2 Figure 2: I suggest replacement of the "Mann hockey stick" graph in this figure with a plot which draws on past temperature estimates by several different groups and also gives some indication of	No longer relevant.

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						uncertainty, similar to the "overlap of reconstructed temperatures" shaded graph used in the bottom panel of Fig 6.14 of the WG1 volume of the AR4. [David Wratt, New Zealand]	
11-551	11	48	4	48	4	"Because volcanic aerosols normally remain in the stratosphere no more than two or three years": it depends on the eruption. If the eruption is very big, as for instance in the case of the 1815, these aerosols can remain even 10 or more years in the stratosphere, because the stratosphere is very stable and suppresses vertical movements, thus the only way to eliminate those aerosols will be for gravitational settlement. [Claudio Cassardo, Italy]	No longer relevant
11-552	11	48	5	48	5	"decadal-scale cooling, such as happened with small eruptions in the decade 2001–2010": really? This is the first time I heard such idea. Nevertheless, in the period 2001-2010 it has not been observed a decadal cooling!!! [Claudio Cassardo, Italy]	Accepted - revised according to suggestions.
11-553	11	48	5	48	7	"A series of volcanic eruptions could, however, give rise to a decadal-scale cooling, such as happened with small eruptions in the decade 2001–2010" If this is true, that a tau/AOD = 0.002 to 0.005 during 2001-2010 is responsible for the decadal cooling, it follows that the much larger (30 times larger) decrease of AOD from 0.050 to 0.150 down to 0.001 during the period 1985-2001 is reponsible for much of the warming from 1985 to 2001. The numbers are from Fig. 8.15. [Richard Keen, USA]	Accepted - revised according to suggestions.
11-554	11	48	6			The wording "decadal scale cooling" here could be interpreted by some readers to imply there was a global cooling trend from 2001 to 2010, which I don't think was the case. Can this sentence be reworded to ensure there is no ambiguity or possible misinterpretation ? [David Wratt, New Zealand]	Accepted - revised according to suggestions.
11-555	11	48	12	48	15	The standard WG1 FAQ style does not include references to chapter sections, since FAQs are designed to be read "stand-alone". So I suggest removing the references here to Chapter 7 and to Section 8.3.2. In fact I suggest the Chapter Authors consider removing this paragraph totally, since the matter is dealt with in FAQ 7.3. [David Wratt, New Zealand]	Accepted - revised according to suggestions.
11-556	11	48	20			Since the standard WG1 FAQ style does not include references to material in Chapters (to allow the FAQS to be read stand-alone), I suggest removal of the reference here to Box 11.1. [David Wratt, New Zealand]	Accepted - revised according to suggestions.
11-557	11	48	40			The standard WG1 FAQ style does not include references to chapters, since FAQs are designed to be read "stand-alone". Can the reference in this line to Chapter 9 be dropped ? [David Wratt, New Zealand]	Accepted - revised according to suggestions.
11-558	11	49	3	49	13	The Allen references should come after the Allan references. [David G. DeWitt, USA]	accepted
11-559	11	49	9	49	12	The Allan references are not in the proper format. [David G. DeWitt, USA]	accepted
11-560	11	49	13			The Allan reference is not the proper format. [David G. DeWitt, USA]	accepted
11-561	11	50	33			"J. Clim. 22:" should be "J. Clim.,22," [David G. DeWitt, USA]	accepted
11-562	11	52	4			"Journal of Climate" should be "J. Climate" [David G. DeWitt, USA]	accepted
11-563	11	52	20			Author format is wrong, i.e. full first names instead of initials. [David G. DeWitt, USA]	accepted
11-564	11	52	33	52	33	Article ID missing in reference. [Georg Feulner, Potsdam]	accepted
11-565	11	53	35			Missing period at end of sentence. [David G. DeWitt, USA]	accepted
11-566	11	53	37			Missing period at end of sentence. [David G. DeWitt, USA]	accepted
11-567	11	53	47			Missing period at end of sentence. [David G. DeWitt, USA]	accepted
11-568	11	53	63			Missing period at end of sentence. [David G. DeWitt, USA]	accepted
11-569	11	54	48	54	49	Reference is incomplete and journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted

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11-570	11	56	3	56	4	Use all listed author initials in citation: Knight, J.R., Allan, R.J., Folland, C.K., Vellinga, M., Mann, M.E., A Signature of Persistent Natural Thermohaline Circulation Cycles in Observed Climate, Geophysical Research Letters, 32, L20708, doi: 10.1029/2005GL02423, 2005 [Michael Mann, USA]	accepted
11-571	11	56	7	56	8	Author format is wrong, i.e. full first names instead of initials. [David G. DeWitt, USA]	accepted
11-572	11	56	23	56	23	Should be "Lambert, F. H." Also, citation of Lambert et al., 2008 is missing (referred to above on page 23): Lambert, F. H., A. R. Stine, N. Y. Krakauer and J. C. H. Chiang, How much will precipitation increase with global warming?, EOS trans., Vol. 89, No. 21, doi: 10.1029/2008EO210001, 2008. [Francis Hugo Lambert, United Kingdom of Great Britain & Northern Ireland]	accepted
11-573	11	56	31			Author format is wrong, ie.e last name and then initial instead of vice-versa. [David G. DeWitt, USA]	accepted
11-574	11	56	50			Journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-575	11	57	40	57	42	I can't find this reference cited anywhere in the chapter. [Michael Mann, USA]	accepted
11-576	11	58	49			Reference is incomplete and journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-577	11	59	50	59	51	All caps for authors and title. Also, journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-578	11	60	59	60	60	There is a missing initial for the author. Also, the ennd of the sentenc is missing a period. [David G. DeWitt, USA]	accepted
11-579	11	62	26			Journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-580	11	62	47			Incomplete reference and journal name sould be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-581	11	62	53			Journal name should be abbreviated instead of spelled out. [David G. DeWitt, USA]	accepted
11-582	11	62	56			What journal has this article been submitted to? [David G. DeWitt, USA]	accepted
11-583	11	63	23			"Journal of Climate" should be "J. Climate" [David G. DeWitt, USA]	accepted
11-584	11	64	44			Missing first initials for authors. [David G. DeWitt, USA]	accepted
11-585	11	64	51			"does the" should be "Does the". [David G. DeWitt, USA]	accepted
11-586	11	66		66		Table 11.1: correct IPSL resolution: AGCM: 2.5x3.75L39 and OGCM 2(0.5)L31 (equatorial refinement) [Eric Guilyardi, France]	Corrected for the IPSL model, but it seems that there are other models that has regionally enhanced resolution. The table will be reexamined after SOD to include possibly more models and to adopt a common convention to represent resolution.
11-587	11	66		66		Table 11.1: correct IPSL initialisation for Atmos/land to "model reanalysis" or "no" depending what is meant by "no" [Eric Guilyardi, France]	The entry is meant whether or not observational data of the atmosphere is assimilated to models. We put "no" in this case. The table will be updated after SOD with checks by the model groups.
11-588	11	68	5	68	5	The "observed mean temperatures" are not observed as such,; they are based on very large numbers of daily observations of maximum and minimum temperature in a varying non representive sample of land and sea sitres. They are not temperatures but "temperature anomalies", the difference between an annual mean and an average over a reference period.. They are not means but the results of subtracting one complex mean from another. The uncertaionty ranges shown indicate that there is a high chance that any "trend"is close to zero. [VINCENT GRAY, NEW ZEALAND]	The caption now refers to observation-based temperatures as the difference from a reference. One could refer to the anomaly from some reference but it seems the same. The detection of climate change is treated in a previous Chapter.

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11-589	11	68				Figure 11.1: I find this figure misleading since it compares observed temperature (which contains observational error) with model simulations (which contain model error). [Richard Allan, UK]	It is certainly true that all observations are inexact as are all models but this is understood. It does not mean that they lack information or that the figure is misleading. Uncertainties are now discussed in more detail.
11-590	11	68				Fig 11.1 Technically the red line still has some internal variability included as it is the average of different model simulations which have internal noise. Would it be possible to do a plot using a simple climate model simulation (MAGICC?) with noise from piControls added to make the point? [Gareth S Jones, UK]	The intent here is to illustrate the Box terms in a reasonably concrete way. While it would be possible to follow this suggestion the worry is that introducing modell results would tend to divorce the ideas from the actual system.
11-591	11	68				Fig 11.1 looks very much like (identical) to Fig 9.5a of AR4. Maybe it is a place holder. How is this fig meant to differ from Fig 9-10? I expect this figure or one like it will become an "icon" of this report as was the case for Fig 9.5 in AR4. However by plotting anomaly, rather than temperature itself, vs year, the figure minimizes the variation among the models. I would think that it is essential to include a plot of Global mean sfc temp itself from the models, rather than (or in addition to) anomaly. I call attn to such a plot Tredger, E. Thesis, 2009, http://cats.lse.ac.uk/homepages/edward/TREDGER_Thesis.pdf page 71. On the evaluation of uncertainty in climate models. PhD thesis, London School of Economics, London. Also Stevens, Bjorn and Stephen E. Schwartz, 2011: Observing and Modeling Earth's Energy Flows. Surveys of Geophysics, revised January 2012. http://www.mpimet.mpg.de/fileadmin/staff/stevensbjorn/Documents/StevensSchwartz2012.pdf Figure 11. All models should be identified, and the data should be made available in an on-line table. [Stephen E Schwartz, USA]	This was a place holder and is now replaced by an example which includes an actual prediction in order to illustrate the points that are being discussed in the Box.The CMIP5 data that was used for the Figure is available from the CMIP archives.
11-592	11	69	1	69	1	I do not like too much this figure. Here the trajectories diverge immediately, while at the beginning they should be parallel between each other, keeping the form of the yellow oval, and diverging only after a certain time. Even if one may question that, in a simulation of 30 years, the time of 1-2 weeks in which the behaviour can be considered as deterministic is very short, however I think that, for didactic purposes, it should be represented in the figure. [Claudio Cassardo, Italy]	This figure received a mixed review and has been dropped.
11-593	11	69	1	69	2	Change figure so that lines do not collapse to a single value downstream of start [Robert Colman, Australia]	This figure received a mixed review and has been dropped.
11-594	11	69				Fig 11.2 Would it be possible to show that the yellow and pink zones are on planes parallel to each other but seperate on the t axis. At the moment it just looks like a blob is attacking an amoeba! [Gareth S Jones, UK]	Ditto.
11-595	11	69				Fig 11.2: The figure dimensions are quite confusing, and it seems to over complicate the concepts that are clearly and sufficiently explained in the chapter text. [Thomas Stocker/ WGI TSU, Switzerland]	Ditto.
11-596	11	71				Panels of the figure are not labeled with the letters that are referred to in the abstract. [David G. DeWitt, USA]	Accepted - figure was placeholder and has been amended
11-597	11	71				Text in upper left figure (key) is too small, axis labels are too small on most frames [Larry Thomason, United States of America]	This figure has been revised and is now clearer.
11-598	11	72	6	72	8	The Msadek and Teng et al references are missing years. There also is no Teng et al. reference in the reference list. [David G. DeWitt, USA]	Fixed in text (pg 11-12) and refs.
11-599	11	72	11	72	11	The point "e" in the captions is repeated twice. [Claudio Cassardo, Italy]	Accepted and corrected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-600	11	72				labeling is too small; maybe split to 2 pages. [Larry Thomason, United States of America]	Accepted and corrected.
11-601	11	72				Fig 11.5: Consider to split this figure into multiple figures. The information provided is very important but is in our view too packed and complex for one single figure [Thomas Stocker/ WGI TSU, Switzerland]	Accepted and corrected.
11-602	11	73				This figure appears to be from a publication by George Boer and collaborators. Should there be a reference to where the figure came from? [David G. DeWitt, USA]	Accepted and corrected.
11-603	11	74	12			:one-side" should be "one-sided". [David G. DeWitt, USA]	Has been fixed
11-604	11	75	4	75	13	The difference of correlations generally does not have reasonable statistical meaning. It should be explained that the differentiation is only formal, without any reasonable statistical meaning of the differences. [Ladislav Metelka, Czech Republic]	This has been replaced by the difference of the Fisher z-tranforms, which is better-defined.
11-605	11	75	4			Figure 11.8: I would change the color scheme used on the right panels. The present one does not allow for a distinction between different values: almost all the plot is yellow, but this can mean everything from 0.1 to 0.3-0.4 improvement in COR skill. Are the results presented in this figure obtained from decadal predictions systems that were initialized every fifth year (as the initial CMIP5 setup suggested) or every year. Please specify this in the figure captions. [Daniela Matei, Germany]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-606	11	75	7	75	7	For Figure 11.8 … there is a reference in the caption to "black dots" but I do not see those. [Thomas Delworth, USA]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-607	11	75				This figure is to small and hence very hard to read. This applies to both the figure and the panel labels. The discussion of black dots is curious as it is impossible to see any dots with the present size of the figure. [David G. DeWitt, USA]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-608	11	75				labeling is too small [Larry Thomason, United States of America]	Figure 11.8 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-609	11	76	4	76	12	The difference of correlations generally does not have reasonable statistical meaning. It should be explained that the differentiation is only formal, without any reasonable statistical meaning of the differences. [Ladislav Metelka, Czech Republic]	This has been replaced by the difference of the Fisher z-tranforms, which is better-defined.
11-610	11	77	4	77	8	In the captions, is not indicated the content of parts "a" and "c" of this figure. [Claudio Cassardo, Italy]	Figure 11.10 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-611	11	77	4	77	8	Description of pictures a) and c) is missing [Ladislav Metelka, Czech Republic]	Figure 11.10 has been redrawn with the latest CMIP5 results available. The quality has been substantially improved.
11-612	11	78	4	78	12	In this figure, "Observations" is present in both plots, but for a different period and, arguably, with a different meaning (as the two curves are different), which should be explained. In the text (page 20 line 40) it is said "In this case decadal means are shown" for the figure 11.11b, but maybe these are moving decadal averages? Another point is, again, the fact that the reference period chosen here for calculating the anomalies, 1986-2005, is different from others used in the same report. Why different reference periods are used? [Claudio Cassardo, Italy]	Yes, panel b shows moving decadal averages. Caption amended. 1986-2005 is the standard reference period for the projections chapters.
11-613	11	79	1	79	2	The colour scale seems to me not appropriate, as there are many useless colours. I suggest to reduce the scale and consequently the number of colours. [Claudio Cassardo, Italy]	The colour scale has been adjusted to show the changes more clearly
11-614	11	79	5	79	5	delete "...relative to the 2086–2005 period", incorrect interval. [Ladislav Metelka, Czech Republic]	This has been corrected
11-615	11	79	5	79	5	Figure 11.12: I suggest to eliminate " relative to the 2086-2005 period" [RODICA TOMOZEIU, Italy]	This has been corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-616	11	79	5			"2086-2005" should be "1986-2005" I think. [David G. DeWitt, USA]	This has been corrected
11-617	11	80				Fig 11.13: We note that a similar concept but different modeling results are provided in FAQ 10.2, Fig 1. Coordination between chapters will be necessary to ensure no contradiction between these two Chapters and their figures. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. This figure has been discussed with the author of the Chapter 10 FAQ. Their revised figure should be referenced to pre-industrial climate, whereas the reference period for chapter 11 is 1986-2005.
11-618	11	81	1	81	2	Also here the scale is too large and many colours are unused. In addition, which is the meaning of the "dotted" areas? Statistically significant changes? [Claudio Cassardo, Italy]	This figure has been replaced by a clearer one. Stippling has been unified throughout the report and a box in Ch12 explains what it means.
11-619	11	81				Figures are very difficult to read especially the lableing for axix and color bars as well as title at top of each panel. Also, what does the stippling represent here? It is not spelled out in the text. [David G. DeWitt, USA]	This figure has been replaced by a clearer one. Stippling has been unified throughout the report and a box in Ch12 explains what it means.
11-620	11	81				nearly impossible to read… [Larry Thomason, United States of America]	This figure has been replaced by a clearer one. Stippling has been unified throughout the report and a box in Ch12 explains what it means.
11-621	11	82	1	82	2	Which is the meaning of the dots? Statistically significant changes? [Claudio Cassardo, Italy]	This figure has been replaced by a clearer one. Stippling has been unified throughout the report and a box in Ch12 explains what it means.
11-622	11	83	1	83	2	I premise I have some problems in distinguishing some colors. However, the choice of the palette seems good except for the two colours corresponding to +2.5 up to +10 - better to choose colours in the blue tonality. Also because the majority of the positive points enter in that range. [Claudio Cassardo, Italy]	The colour scale for precipitation has been modified throughout the report and checked for readability, also for colour blind people.
11-623	11	83	5	83	7	"Left panels" instead of "Upper panels", "Right panels" instead of "Lower panels". [Ladislav Metelka, Czech Republic]	This figure and caption have been replaced by clearer versions. GJ: check that this comment is addressed in the new caption
11-624	11	83				Figure panels do not correspond with the caption description. [David G. DeWitt, USA]	This figure and caption have been replaced by clearer versions. GJ: check that this comment is addressed in the new caption
11-625	11	83				Fig. 11.16 – caption does not match the figure – left/right mixed with top/bottom. [ED HAWKINS, United Kingdom of Great Britain & Northern Ireland]	This figure and caption have been replaced by clearer versions. GJ: check that this comment is addressed in the new caption
11-626	11	83				Fig. 11.16: the figure caption seems wrong. I think "upper" and "lower" actually refer to "left" and "right" panels [Doug Smith, United Kingdom of Great Britain & Northern Ireland]	This figure and caption have been replaced by clearer versions. GJ: check that this comment is addressed in the new caption
11-627	11	85	4	85	5	Units in the caption should be revised or clarified. For evaporation, precipitation and runoff, the unit is percentage. What means percentage? It means that, for each grid point, the plotted value is the ratio [period 2016–2035 minus period 1986–2005]/[period 1986-2005]*100? If yes, please specify it. For soil moisture, near the color scale it is written kg/m2 while in the caption is written percent: which one is the correct one? And finally for specific humidity (usually measured in g/kg), near the color scale it is written percent (flux of soil water), as in the caption: it must be intended as above? [Claudio Cassardo, Italy]	This figure and caption has been replaced by clearer versions. For many quantities we use relative changes wrt to the 1986-2005 climatology. This allows comparisons of areas with very mean states, and in the case of soil moistue the different definitions in the in different models. In the case of specific humidity, the percentage change can be compared with the change expected on the basis of the Clausius Clapeyron relationship. GJ to check that he caption is clear.
11-628	11	85				No color bar for the evaporation panel. Color bar says units for soil moisture are kg/sq meter while caption says it is %. Panel order in line 4 is wrong, i.e. top panel is evaporation not runoff. Finally, why is specific humidity in brackets on line 5? [David G. DeWitt, USA]	This figure and caption has been replaced by new clearer versions including units and color bars for each variable.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11-629	11	86	4	86	8	"Gray shading indicates ...": I cannot see gray shading in the figure. While I see a strange "disturbance" in the map which cross the figure along a meridian (Greenwich?), pole-to-pole, "passing" over France, well evident in southern Atlantic. [Claudio Cassardo, Italy]	This figure and caption have been replaced by clearer versions. GV: check that this comment is addressed in the new caption
11-630	11	89	1	89	1	Figure 11.22 and other figures show projections up to 2100. I presume these will either be changed to just show the near term or there could simply be a cross reference to ch12 figures? [Matthew Collins, United Kingdom]	The choice of panels to be shown in this figure has been coordinated with Chapter 12.
11-631	11	89				How exactly did you calculate the global very wet days? Did you first globally average the precipitation or did you do the analysis at each grid point and the global average? To be consistent with the chapter the figure should cover the projection time range until ~2050 only. [Christof Appenzeller, Switzerland]	The relevant paper (Sillmann et al. 2012, submitted to JGR) is now available and referenced in the text.
11-632	11	89				jpeg artifacts? [Larry Thomason, United States of America]	Figure has been improved.
11-633	11	89				Fig 11.22: Overlaps with Chapter 12, Fig 12.29. Please avoid redundancy. [Thomas Stocker/ WGI TSU, Switzerland]	The choice of panels to be shown in this figure has been coordinated with the cross-cutting group on extremes.
11-634	11	90				Very hard to make out stippling in figure. [David G. DeWitt, USA]	Figure has been improved.
11-635	11	92	20	92	24	While the caption and corresponding section in the paper clearly state that the model results for ozone levels are difficult to attribute to climate change versus climate variability, it is difficult to assess the role of climate variability in Figure 11.25. Figure 11 from Nolte et al. (2008) at least visually allows one to assess the potential role of climate variability, and it might be useful to include something of this sort into Figure 11.25 (particularly in the projections out to 2030 where the competing roles are more intertwined). [William Landuyt, United States of America]	Taken into account. The individual groups whose papers are synthesized here do not consistently report the individual years as was done in Nolte et al. 2008 making it difficult if not impossible to follow the reviewer's suggestion directly. Figure 11.26 does show interannual variability, which would be larger in any one model, but nevertheless is still evident in the multi-model mean and range shown there. We now include a statement pointing this out.
11-636	11	93				the background map confuses the plots though I can see why it is used. [Larry Thomason, United States of America]	Taken into account. We have revised the figure for SOD to a much lighter background shading so as not to distract from the AQ projections in the panels.
11-637	11	101				Figure 11.3b would be more informative if it compared RCP6.0 with RCP8.5 since these are the scenarios that are most different over the period considered. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted. We considered this suggestion, but the differences between scenarios depend on the time period considered, and we judged that the existing comparison between RCP2.6 AND RCP8.5 is most informative.
11-638	11	102	1	102	3	In this list, Pinatubo is missing. And, if you will accept my suggestion, please rename "unknown" in "unidentified" (see my note 224). [Claudio Cassardo, Italy]	accepted.
11-639	11	102				FAQ 11.2, Figure 1 - If one attaches confidence to the controversial dendrochronologic "thermometer", why hide the data after 1960 ? Because they do not longer fit CRU data ? Dendrochronology is suspected to be at least as sensitive to humidity as to temperature and is questionable, therefore, as a good proxy of global temperature. And why is this curve incorporated without any explication of the method used ? [François GERVAIS, France]	No longer relavant