Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1	10	0	0	0	0	This chpater whilst very interesting seems way over size. Section 10.5 is written in a much more succinct style that the other sections should emulate. [Philip JONES, UK]	The length of the chapter has been reduced and written more succinctly
10-2	10	0	30	8	30	Should "signal-in-noise" be "signal-and-noise"? [Dian Seidel, USA]	Changed to signal-to-noise in line with terminology used by Hegerl and Zwiers, 2011
10-3	10	0				This is the most even presentation of pros and cons of D&A that has yet been produced by IPCC. Congratulations!! [Tim Barnett, USA]	Thank you
10-4	10	0				The result is the quantitative case for D&A has not, in my view, been strenghten. Just the opposite [Tim Barnett, USA]	We make a new assessment of the strength of the evidence from across the chapter as summarised in the Executive Summary
10-5	10	0				Text is far too long,redundant and often off message [Tim Barnett, USA]	The length of the chapter has been reduced and written more succinctly
10-6	10	0				e.g. Most of the material on pg 7-13 could go in an Appendix,be presented elsewhere as a separte paper or omitted [Tim Barnett, USA]	We have shortened this section and strengthened it ensuring that this section provides a standalone guide to the methodological basis of detection and attribution
10-7	10	0				e.g. cross references to Chapter 2/3 often redundant [Tim Barnett, USA]	We need to make references to Chapters 2/3 where appropriate since these chapters do the observational assessments
10-8	10	0				e.g. If it doen not have to do with a summary D&A statement, leave it out.. This act alone would greatly shorten the Chapter [Tim Barnett, USA]	We have tightened up the text by ensuring that each subsection ends with a short synthesis summarising the assessment of that part. The preceding text then forms the necessary supporting material for this synthesis statement.
10-9	10	0				Unfortunately, this report has been rushed to meet bureaucratic time scales; else we would see much more from the CMIP5 ensemble which has apparently not been fully analyzed. So we fall back on 'old' info from CMIP3 [Tim Barnett, USA]	There is new literature using CMIP5 available to us in the SOD which we now assess
10-10	10	0				Numerous citings where models fail to simulate the real world. In that case just say so; no D&A possible. But the authors go ahead and make baseless conclusions as to the reality of GHG impacts. [Tim Barnett, USA]	Assessment based on evidence presented and peer reviewed methodologies which are assessed. As discussed errors in a model's climate sensitivity of net forcing does not disqualify it from being used in detection and attribuioin since spatial patterns of model fingerprints may still be well represented and adjustable using scaling factors from optimal detection.
10-11	10	0				Please remove the words 'broadly consistent' from the text. As generally used here,the terms are used to summarized a scientific discussion that is confused, based on questionable data and models. It is an outstanding example of 'IPCC bias' [Tim Barnett, USA]	We have addressed this in the text.
10-12	10	0				The text, as far as I've gotten, seems to have a level of increased uncertainty and contradictory results than AR4. So I don't really seeing this Chapter as pushing on past where we were with AR4. [Tim Barnett, USA]	The assessment is drawn together in the ES. There is more evidence from a wider range of the climate system. There is more regional level information. An important advance is an increased understanding of ocean heat content varibility than at the time of AR4 thanks to having identified the impact of instrumental errors on records of ocean heat content. At the same time there are important uncertainties which inform the assessment as summarised in Table 10.1

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-13	10	0				Subsections 10.5.1.1,10.5.2,10.5.3, 10.6.2 and 10.7 do not contain a substantive statement re-D&A. Further much of these sections looks like a rehash of material already presented in other chapters.. Suggest they be shortened (to say no quantitative D&A are possible) or deleted [Tim Barnett, USA]	In revision each subsection concludes with a synthesis section which contains attribution statements.
10-14	10	0				A major problem with these section is that there is no predicted future signal due to warming and hence only speculation, etc to go on. For instance, the subsections on permafrost, glaciers,Greenland, etc are not explicitly modeled in current CMIP runs. Without a theoretical signal to look for (suppied by CGCMs) no quantiative D&A is possible. As noted above, every subsection in this chapter needs a concluding statement re D&A, otherwise remove the discussion to appropriate chapters. Such action will really shorten and focus Chapter 10. [Tim Barnett, USA]	Every section now has a concluding section as well as being drawn together in a consistent way in Table 10.1 thereby supporting a consistent assessment.
10-15	10	0				I do not feel that the detection and attribution of forcing on the climate during the last 1–2 millennia in this chapter are always based on the latest palaeoclimatological research. It is not always in harmony with the presentation in Chapter 5. I think it is important to bring Chapter 10 in better harmony with Chapter 5. Since the time of publication of the IPCC Fourth Assessment Report, as outlined in Chapter 5, temperature reconstructions have shown larger amplitudes of centennial temperature variability during the last 1–2 millennia. This larger amplitude of past temperatures is important for discussing the detection and attribution of the influence of different forcings on the climate. It is also of significant relevance to take this into consideration for a correct estimate of the climate sensitivity from using palaeo evidence. Another thing of importance for the detection and attribution of climate change in a palaeo perspective is that evidence published since the IPCC Fourth Assessment Report points to a more geographically coherent nature of low-frequency climate changes as the Medieval Climate Anomaly and the Little Ice Age. [Fredrik Charpentier Ljungqvist, Sweden]	In revising Section 10.7 attention has been given to ensuring consistency with Chapter 5.
10-16	10	0				The use of the term the Medieval Warm Period is varied with the use of the term the Medieval Climate Anomaly. In Chapter 5, the term Medieval Climate Anomaly is used throughout the text. [Fredrik Charpentier Ljungqvist, Sweden]	MCA is used throughout.
10-17	10	0				I think it is very important, in detection and attribution studies for the last 1–2 millennia, that the fact that most temperature reconstructions underestimate the amplitude of the trend and low-frequency variability of past temperature changes is acknowledged and discussed. Such a discussion is totally lacking in Chapter 10. This underestimation is, according to pseudo-proxy experiments, usually in the order of 20–50%. The topic is discussed in, for example, Christiansen et al. (2009) and the articles cited there-in. The full reference to Christiansen et al. (2009) is: Christiansen, B., Schmith, T., and Thejll, P.: A surrogate ensemble study of climate reconstruction methods: stochasticity and robustness, J. Climate, 22, 951–976, 2009. [Fredrik Charpentier Ljungqvist, Sweden]	The assessment in 10.7 assesses the extent to which the causes of pre-industrial temperature variability can be understood. As such it relies on the assessment made in chapter 5 on the extent to which temperature reconstructions under- or over-estimate low frequency variability.
10-18	10	0				Please observe that the attribution of changes in Ph in the ocean (acidification) should be delt with somewhere in the report. Now it is not in Chapter 10 nor in Chapter 3. We suggest that you clarify this with the writing team of Chapter 3. [Øyvind Christophersen, Norway]	The assessment on the causes of ocean acifidicatoin is in chapter 10.
10-19	10	0				In my opinion, an excellent and impressive review and assessment of state-of-the art of detection and attribution (D&A) in the physical climate field. My main concern is that it is too long, too detailed and too wordy in some instances. I indicate a few sections where shortening might be possible. I only provide comments on sections 10.1, 10.2 and 10.5.2. As a Lead Author of the corresponding D&A chapter in WGII a main interest is in ensuring consistency across the two WGs. [Christian Huggel, Switzerland]	Thank you. The length of the chapter has been reduced and written more succinctly
10-20	10	0				Consider the consistency of the use of the term "all"/"All"/"ALL" which is used throughout the chapter to describe forcings/simulations associated with anthropogenic+natural forcings. It should also be noted somewhere (maybe in glossary?) that not all "ALL"s are the same. Some simulations will not contain as full a set of anthropogenic forcings as other simulations, which themselves may not include all known forcings. Should say somewhere something like that the use of the term "ALL" is to cover a set of similar but not necessarily identical forcings/simulations? [Gareth S Jones, UK]	The use of the term ALL and what is meant by "All forcings" is clarified at the start of the chapter.
10-21	10	0				This chapter is generally well written, clear and includes assessment of much new evidence relevant to detection and attirbution. It also considers a wider range of climate variables, which is good. However, it is LONG, and efforts should be made to reduce, consolidate and simplify some of the text. Given that model simulated multidecadal variability is critical for the D&A studies commonly refered to in this	Thank you. The length of the chapter has been reduced and written more succinctly. Chapter 9 in the sod contains a more thorough assessment of the models' ability to represent variability which is

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						chapter, it would have been helpful to include a brief statement about whether the models are providing estimates of decadal variability consistent with observations for each of the climate variables being considered. This is discussed for temperature at length and but not for precipitation or extrems or cryopshere or ocean properties etc. This assessment should have been in Cpater 9 earlier but needs to be referred to briefly in this chapter when each climate variable is considered, particularly if there are issues about the quality of the model estimates of decadal variability. [David Karoly, Australia]	refrerred to in chapter 10.
10-22	10	0				The draft is in good shape. Congratulations! [Albert Klein Tan k, Netherlands]	Thank you.
10-23	10	0				Although attribution studies employ powerful numerical methods, they als involve a certain amount of subjective judgement, for example in the selection of models and observational data, and when expressing confidence in these tools.The usefulness of this chapter would be strengthened if uncertainty assessments were based on broader, well documented expert panels, including sceptical scientists from the wider community of natural scientists. [Gerbrand KOMEN, Netherlands]	The basis of the assessment is set out in a traceable manner, via the synthesis statements at the end of each subsection, through into the Synthesis Table 10.1 and thence into the ES, where therre is adherence to the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. The chapter is open to two rounds of expert review open to all scientists including so-called "sceptical scientists from the wider community of natural scientists".
10-24	10	0				Suggest generally replacing "most of" by "over half of" or "over 50% of", e.g. in "most of the observed increase in global average temperatures". The word "most" has come usually to be indicative of significantly more than 50%, which is misleading. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	"Most of" meaning "more than half" has a clear meaning to most people A spelling out of the meaning most of is made once at the first time of its use in the chapter.
10-25	10	0				Because uncertainty in both many types of observations and in climate models is very considerable, internal variability is very substantial (with the AR1 model generally used probably leading to substantial underestimates of longer term variability) and physical understanding of climate processes far from perfect even in the best of cases, statements of "very high confidence", "virtually certain" and "extremely likely" are very difficult to justify and should be avoided. The same goes for statements of "high confidence" and "very likely" in all but exceptional cases. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The basis of the assessment is set out in a traceable manner, via the synthesis statements at the end of each subsection, through into the Synthesis Table 10.1 and thence into the ES, taking account of remaining uncertainties, including in the ability of models to represent multi-decadal scale variability where therre is adherence to the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties.
10-26	10	0				The Likelihood Terminology and the Confidence Terminology seem often to be used in a way that is inconsistent. Standard probability theory does not allow for stating that something is virtually certain (>99% probability per AR4 WG1) with only a very high confidence - at least a 9 out of 10 chance, as defined in AR4 WG1. That means that only a >89% (9/10 x 99%) probability is assured. The statement should simply be that it the thing referred to is very likely (>90% probability, stretching slightly from 89%). On the other hand, stating that something is likely with a very high confidence is at least potentially consistent, since the assessment of likelihood could have (and hopefully did) take account of the < 1 in 10 possibility of the scientific basis on which the result was based being wrong. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Our usage follows and is consistent with the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties.
10-27	10	0				This chapter repeatedly supports claims of confidence in conclusions and robustness of conclusions based upon "agreement" or "consistency" among models and upon model based ensemble statistics. Agreement of different sources, even in a linear system would only be supportive of confidence if the sources are independant so that they are less likely to have correlated error. Once correlated error has been demonstrated it is evidence of "wrong agreement". In complex models of interdependent components any further agreement should be viewed with skepticism and suspicion, and not as evidence of confidence. There is a failure in the different sections of this chapter to maintain awareness of the interdependence among the different components of the system being analyzed and to assess how the "agreement" might be an artifact of correlated error documented in the other chapters. [Martin Lewitt, United States of America]	Evidence from across the chapter is synthesised in Table 10.1 and drawn together in the ES. This includes discussion of to what extent information is correlated and therefore not providing additional information and to what extent additional information is provided, through for example, information from the hydrological cycle additional to that of temperatures.
10-28	10	0				This chapter is selective in citing agreement and disagreement as evidence for and against confidence and	Evidence from across the chapter is synthesised in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						robustness. Citing agreement between model ensemble means and other lines of evidence in support of "confidence" and "robustness" is arbitrary in ignoring the differences between the models that is not represented in the mean. That disagreement among the models could easily have been inferred to reduce confidence and robustness. [Martin Lewitt, United States of America]	Table 10.1 and drawn together in the ES.The assessment takes into account remaining uncertainties including observational and model errors.
10-29	10	0				This chapter is biased in its separate analyses of solar and CO2 forcing. The variation in either has difficulty matching or explaining the variation in 20th century temperature observations, but only in the case of solar are the difficulties emphasized and put forward as conclusive. Neither explains the mid-century pause or cooling well, and neither explains the steep slope of the variations on decadal scales. Models parameterized to explain agree with the observations with CO2 forcing, even though disagreeing more than a factor of two in sensitivity, and that have been shown to under represent the signature of the solar cycle seen in the observations, may only fail to reproduce the observations with solar forcing because of a lack of equivilent effort. [Martin Lewitt, United States of America]	Evidence from across the chapter is synthesised in Table 10.1 and drawn together in the ES. Both greenhouse gas and solar forcing are assessed and in both cases are allowances made for the possibility that the response to either forcing could be under- or over-represented in models as discussed in section 10.2. There has been plenty of effort in the scientific literature to investigate how much solar forcing can explain global and regional climate variability and change which we assess.
10-30	10	0				The energy imbalance we are attempting to attribute and then project is generally agreed to be less than 1 watt per meter squared globally and annually averaged. Hansen has recented estimated it as low as 0.58W/m^2. To apportion attribution we need quantitative accuracy of at least 0.1W/m^2 or 0.1 degrees K and preferable better. While conclusions have selectively focused on "agreement", disagreement is also evidence and there has been a lack of rigorous analysis of disagreement in most sections. An effort should be made to assess quantatively the amount and implications of any disagreement and correlated error in relation to the needed accuracy and the magnitude of the energy imbalance of interest. Any difficulty or uncertainty in being able to arrive at a quantitative assessment should lead to conclusions of reduced confidence and robustness. [Martin Lewitt, United States of America]	Evidence from across the chapter is synthesised in Table 10.1 and drawn together in the ES.The assessment takes into account remaining uncertainties including observational and model errors and conforms to IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties.
10-31	10	0				The chapter is too long. It could be shortened(and have more impact) if it concentrated on the facets of climate that are clearly detected (surface temperature) and those which are important but where there is some debate (eg tropical tropospheric temperatures) and where there is significant new information. Areas where there is new science include Arctic sea ice, ocean heat content and the last decade. Other important areas include tropical cyclones and the discussion of constraints on TCR/ECS. It is not clear how much discussion is need on extreme events given the recent IPCC report. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	The length of the chapter has been reduced and the chapter has been written more succinctly
10-32	10	0				It was not always clear to me, particularly in the second half of the chapter when change in a climate parameter had been formally attributed to human activity (larger than expected due to natural fluctuations, not explicable by other known factors) or just detected (larger than expected from natural fluctuations but as yet not shown to be uniquely attributable to human activity, or is consistent with being caused by human activity (whether detected or not). [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	The chapter has been revised with this in mind. There is a synthesis segment at the end of each subsecioin and assessments from across the chapter are drawn together in a consistent way in the Synthesis table 10.1.
10-33	10	0				A second issue is that it is not always clear when a change in parameter has been directly attributed to human activity due to its unque "fingerprint" such as surface or tropospheric temperatures, or through what the chapter calls "two step attribution"- when the parameter in question changes because of strong physical links to another variable which has been directly attributed (eg temperature). [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	We have clarified the discission in the chapter about whether single step or multi-step.
10-34	10	0				Although there is discussion of uncertainty throughout the chapter,I believe the chapter would benefit from a short summary assessment at the end on the main remaining uncertainties such as the magnitude of natural multidecadal variability, uncertainties in forcings including aerosols and solar forcing, possible errors and degeneracy in signals etc [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	We decided it would be better to include remaining uncertainties within the synthesis segments at the end of each subsection which will inform the assessment. They are also included in the Table 10.1 entries.There is a remaining uncertainties part in the ES.
10-35	10	0				Some of the language used to describe significance, uncertainty and confidence is hard to understand. For example, it is not clear what it means to say “it is likely that that there has been significant anthropogenic warming in Arctic land surface temperatures”. Does this mean that detection, at some given level of significance, occurred in more than 66% of studies? (Or for 66% of the models used to estimate internal	The chapter has been revised to ensure consistency with the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. We have revised to avoid

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						variability?) A sentence or two in the introduction would help to clarify what is meant when such statements are made later in the chapter. There are also multiple instances within the chapter where the meaning of “significant” and/or “substantial” is ambiguous as they appear to be used interchangeably (e.g. lines 16-19, p4). Does substantial have a specific meaning within AR5 (e.g. more than half)? If not, a less ambiguous description of the amount of change that has been attributed would be more useful. If significant is being used in a statistical sense (i.e. to indicate detection relative to a null-hypothesis of natural change at some given level of probability), then this could be specified more clearly. [Chris Roberts, Uk]	use of likelihood with significance statements. The use of "substantial" has been replaced with a more clearly defined term.
10-36	10	0				Rather than the frequent citations to Hegerl et al. (2011), it would be better to clearly refer to the AR4. Not to diminish the role of Hegerl, but to clarify that this AR5 is updating AR4. [Dian Seidel, USA]	The chapter has been revised to make it clearer what is new relative to AR4 - for which the reference is Hegerl et al, 2007. References to other Hegerl et al papers are retained where appropriate.
10-37	10	0				Parts of the chapter use phrases like "over the period 1900-1940" (p 14, line 21) or "over the 1902-2010 period" (p 16 line 24). These can be shortened to "during 1900-1940" in almost all instances, of which there are many. [Dian Seidel, USA]	Thanks for the suggestion. A consistent shorter form is adopted.
10-38	10	0				Chapter 10 is the most important by far. It deals with Attribution and provides the science base for the IPCC claim that recent warming is anthropogenic – in its Summary for Policymakers and elsewhere. It is therefore crucial that the evidence be presented in a transparent and reproducible manner. However, this is not the case. My critique is presented in the hope and expectation that the discussion will be expanded sufficiently so as to withstand scrutiny. After all, this IPCC report is likely to be the basis for far-reaching economic and political decisions. I will present my discussion in the form of questions that require detailed quantitative answers, together with references to published papers where appropriate. The key result of Chapter 10 may be Fig. 10.3. The bottom panel shows the Global Mean [surface] Temperatures with dark grey lines (no error intervals shown) and the results of GCMs that use only “natural forcings” -- from CMIP3 and “other sources” (light grey lines) and from CIMP5 (pink lines). The time interval is 1960 to 2010. 1. How is Fig. 10.3 different from Fig. 9.5 of AR4? Are the differences substantial? Explain. 2. Do the model results show the results of individual runs or of model ensemble-means? How many? 3. Do “natural forcings” include volcanic eruptions and internal oscillations (ENSO, PDO, etc)? Explain 4. How do the models handle solar variability (TSI, solar wind, etc)? 5. How do the models explain the observed warming of 1910-1940? 6. What accounts for the sudden cooling around 1965 shown in the model results of CIMP5? 7. Turning to the upper panel of Fig. 10.3, which models agree best with observations of 1970-2010? What are their climate sensitivities (CS)? What are the details of their direct and indirect aerosol forcings (AF), incl their geographic and temporal coverage? 8. Can the upper panel be shown for Tropics, NH and SH – instead of just for the Global Mean – but using the same values for CS and historic AF scenarios in the models? 9. Finally, can the upper panel be shown for MSU atmospheric temp – instead of just surface temp– but using the same values for CS and historic AF scenarios? Has this been attempted? 10. In Chap 10, I am confused by the expression “extremely likely;” what does it mean? I know that “very likely” means “90 -99% certain.” I am pleading for clarity. [S. Fred Singer, USA]	The basis of the assessment is set out in a traceable manner, via the synthesis statements at the end of each subsection, through into the Synthesis Table 10.1 and thence into the ES, where therre is adherence to the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. Detailed comments 1) CMIP3 as assessed in ar4 and CMIP5 ensembles are compared; we have added short discussion about the differences 2) information described in text and in supplementary information 3) information described in text and in supplementary information 4) info described in text and in supplementary informtion 5) there is a subsection on early century warming 6) there are explosive volcanic eruptions in the model simulations 7) There are details of the models used in the supplementary information 8) Fig 10.3 does not show the global mean but rather the zonal means at different latitudes. The CMIP5 models all have different climate sensitivities and aerosol forcings. 9) There is a plot of free atmospheric temperatures in Fig 10.6. 10) Extremely likely is defined in the IPCC Guidance Note for LEad Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties Page 3 Table 1 footnote and means 95-100% probability.
10-39	10	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]	Thank you for positive comments. Revisions have been made to text as outlined in response to other reviewers' comments
10-40	10	0				We note that in some instances the Chapter team seems to specifically highlight, defend, or even respond to certain individual papers. This for example is the case for the Curry et al. (2011, BAMS), e.g., on page 14, leaving the impression of "defending" the AR4 as opposed to using the AR4 as a basis for the new assessment provided here, taking into account all the new literature and discussions. [Thomas Stocker/ WGI TSU, Switzerland]	This text has been deleted. Text has been revised to reflect change in assessment since ar4 (including a first para of the ES outlining progress since ar4) rather than to defend ar4 assessment.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-41	10	0				Terms such as 'substantial' and 'significant' are frequently used in the concluding sections. However, care is needed to ensure that these terms are used consistently and in line with the required quantitative basis. [Thomas Stocker/ WGI TSU, Switzerland]	Text has been revised to ensure consistent with Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. This includes revision of where "significant" and "substantial" are used.
10-42	10	0				Need to reconcile the "low confidence" in surface dryness statement (page 30 l.27) compared to the "medium confidence" for droughts given on page 48. [Thomas Stocker/ WGI TSU, Switzerland]	This has been resolved to be consistent with Chapter 2.
10-43	10	0				We suggest to introduce a consistent structure for all sections/subsections. For example, the cryosphere section lacks the concluding statement, which is included in other sections and proves to be very effective in synthesizing the key results. [Thomas Stocker/ WGI TSU, Switzerland]	Such a concluding synthesis component has been included in all sections/subsections.
10-44	10	0				I have a general comment for this chapter. That is at the end of some sections or subsections, a summary or conclusion paragraph is presented, which I think is very good. Examples of such include: Paragraph on page 29, lines 52-57, and paragraph on page 32 line 51-53, etc. But in most sections and subsections, such conclusion or summary is missing. I think such conclusion or summary should be provided for all the sections or subsections, because, this is the most important and understandable and most authoritative message for the ordinary readers of the report. We cannot expect the readers to consult thousands of references cited in the report, what they want to see/know is what the expert authors of IPCC reports conclude from those cited references. [Chong-Yu Xu, Norway]	Such a concluding synthesis component has been included in all sections/subsections.
10-45	10	0				I would like to complement the authors of Chapter 10 for producing a first order draft that, to my eye, already appears to have achieved a relatively mature state. My comments below are primarily concerned with potential inconsistencies between the various assessments that are reported in the chapter. In several places I have the impression that the authors are not yet evaluting the literature with a sufficiently critical eye. Also, a consistent difficulty throughout the chapter seems to be with the use of the word "significant". It is not always clear whether this implies statistical significance in all cases. As noted below, a likelihood assessment considering a finding of statistical significance does not make a lot of sense to me. The use of "medium confidence" where in the past the chapter might have assessed finding as being "more likely than not" needs some discussion as these are NOT equivalent assessments. More likely than not assessments made in the AR4 were quantified (it meant likelihood > 50%), whereas medium confidence is not quantified. [Francis Zwiers, Canada]	Thank you. Revisions have been undertaken to achieve an assessment rather than a review. The basis of the assessment is set out in a traceable manner, via the synthesis statements at the end of each subsection, through into the Synthesis Table 10.1 and thence into the ES, where therre is adherence to the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. The use of the word "significant" has been carefully reconsidered and revised.
10-46	10	1	1	1		Detection and Attribution of Climate Change: from Global to Regional [Medani Bhandari, Nepal]	Noted.
10-47	10	1	2	1	2	The title of Ch. 10 seems awkward, ending as it does with two adjectives (Global and Regional) modifying no noun. Consider changing to "Detection and Attribution of Global and Regional Climate Change". [Dian Seidel, USA]	The chapter title is pre-determined and cannot be changed.
10-48	10	1		1		When compared to the content in Section 10.5 Cryosphere, there seems to be a marked imbalance in the way the hydrosphere is represented in the report (Chapter 2 and 10). For instance on page 10-61 it is stated (line 7) “Warming of the atmosphere and the oceans can affect the Cryosphere and in case of snow and sea-ice lead to positive feedbacks that ….” Similar one can say that such warming can affect the Hydrosphere and changes in soil moisture, vegetation, snow and ice may lead to land surface atmosphere feedbacks that can … [Lena M. Tallaksen, Norway]	A snythesis of the assessment across the climate system is made in 10.9 and in Table 10.1
10-49	10	1				I have listed this comment as being for Chapter 10 because that is what I am looking at, but it is in fact probably targeted more broadly at the entire WGI AR5. One thing I know has been communicated over and over again to WGI is the primacy of the need for explicit contextualisation in space and time. The temporal contextualisation has been done here in Chapter 10, good. But spatial contextualisation is at best subtle. In the ZOD Chapter 10 had looked laudably poised to buck the WGI trend and include explicit systematic regional assessment, but now that has disappeared in the FOD. This is disappointing. For me it is also annoying: now WGII is going to have to plug the gap and as the resident climate D&A expert in WGII I am being tasked with this. In a sense this arrangement is a good thing, as I feel the vertical WG structure of the AR5 is outrageously outdated. But stuck as we are with this structure, it is astonishing that WGII should feel that the most fundamental aspect of change in the physical climate system with respect to impacts is so	The difficulties with the late arrival of CMIP5 model output meant we did not have information available in time to provide as much regional level information as we would have liked for the fod. For the sod we have updated the analyses with a regoinal level time series analysis of temperatures, precipitatoin, sea ice and ocean temperatures.Note also that there are figures of spatial maps of temperature, there is a subsection on atttribution of regional surface temperature changes, and attribution of other climate variables includes

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						neglected in WGI that it needs to take things into its own hands and trespass on WGI's remit. For something constructive out of this comment: if any of the authors of Chapter 10 would like to assist various WGII chapters on D&A assessments I am sure that would be much appreciated. [Dáithí Stone, United States of America]	consideration of their spatial structures since much of the fingerrint information is in the spatial structures.
10-50	10	3	1	5	12	This executive summary (and the chapter) has a lot of good material but it will need to do more in the next draft at saying explicitly what is new since AR4. I suggest that you consider adding a paragraph at the front that lays out a list of key advances in attribution since AR4. I realize this will be challenging to write but it's needed to avoid confusion. [Susan Solomon, USA]	Thank you for the suggestion which we have adopted.
10-51	10	3	1			The Executive Summary is very nicely written, with coherent prose that flows nicely. This is in contrast to the ES of the other chapter I reviewed (Ch. 2), done in bullet fashion. [Dian Seidel, USA]	Thank you
10-52	10	3	3	3	6	Too broad in its assertions. Certainly not ALL regional temperature conditions show evidence for a human influence, as this statement would imply. Consider saying "some regional temperature condition..". Likewise also an issue for the other indicators that have regional flavor. Also, the authors need to be clear in defining what is meant by "effects of human influence". Is this a code word for having detected and change and attributed that change to anthropogenic greenhouse gas forcing? [Martin Hoerling, USA]	Revised to relfect the point that it isn't all regional temperatures but the observed spatial pattern across the globe of temperatures that points to a large scale warming. The "effects of human influence" means that the fingerprint of human influence has been detected in the observations.This part reworded.
10-53	10	3	3	3	6	Consider describing explicitely that this summary statement is conditional on the quality of models and our confidence in models, and on the selection of models included in this assessment (which involves to a certain extent some subjective choices). [Albert Klein Tan k, Netherlands]	This statement is conditional on a number of things including also observational datasets which are discussed in the remaining uncertainties section of the ES. This initial statement is intended as an overall summary statement.
10-54	10	3	3	3	33	The "evidence" for warming is entirely the opinion of those paid to provide it, based on poorly representative observations whose limitations have been concealed by multiple averaging. by selective choice of anecdotes, and by downplaying, concealment or even suppression of unfavourable evidence in the manner documented in the Emails released from the University of East Anglia. The evidence from models is dependent on their dubious assumnptions.. [VINCENT GRAY, NEW ZEALAND]	The evidence is presented in a transparent and tracable manner in the chapter through the sections/subsections via snythesis statements at the end of each section/subsection, and the snythesis table 10.1 and into the ES. Remaining uncertainties are explicitly considered as part of the assessment. Therefore the chapter provide not an "opinion of those paid to provide it" but an assessment taking account of remaining uncertainties, consistent with the IPCC Guidance note on consistent treatment of uncertainties and with tracability from the text to the ES.
10-55	10	3	3	5	1	The "evidence" consists entirely of correlations which can never be proof of causation. It does not change this fact by calling "correlation" "attribution". [VINCENT GRAY, NEW ZEALAND]	The evidence is not based entirely on correlation but also on physical reasoning as outlined in the chapter.
10-56	10	3	3		6	It seems appropriate to make some brief comments that attribution requires use of observations and models and there are shortcomings in both that limit attribution statements under the null hypothesis of no change. The inadequacies of models in simulating modes of variability, regional climate and precipitation is a major limitation about what can be said with confidence. Some of this is touched on o p 5 but the header is wrong. [Kevin Trenberth, USA]	We have included discussion and assessment of the Trenberth papers proposing change of null hyopthesis in the main body of the chapter and also include discussion of atribution subject to shortcomings in observations and models. We reject a change in the null hypothesis on the basis that the literature which we are here to assess still uses the null hypothesis of internal variability explaining the observations. For the ES, such issues are included under remaining uncertainties since it is these uncertainties that limit the confidence on attributoin statements.
10-57	10	3	4	3	6	"The consistency …. points to ….": this appears a bit vague. There are many components of the system which are changing in ways that are very likely due to anthropogenic forcing. The consistency across the system adds ….. [Martin Juckes, UK]	Reworded to better reflect point of consistency across system adding to confidence of attribution of changes to human influence

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-58	10	3	9		22	Hard to read. Presumably will be revised. [Kevin Trenberth, USA]	Revised to take account of updated SOD. Note that we need to use IPCC uncertainty language.
10-59	10	3	12	3	16	Explain what is meant by "forcing" – is it the perturbation to the system (emissions etc) or the radiative forcing? [Martin Juckes, UK]	Forcing is a standard term defined in the IPCC glossary and therefore not something we should define in the ES.
10-60	10	3	13	3	20	Put the two statements about rise in global mean temperature next to each other, and use consistent grammatical constructions – e.g. it is extremely likely that the observed increase .. is not entirely due to natural variability and … [Martin Juckes, UK]	This has been done.
10-61	10	3	14	3	14	". . confidence) that it is very likely": since the assessment of confidence and likelihoof involves some (subjective) expert judgement it is important that this chapter describes in detail how likelihood estimates such as this one (and all others) have been arrived at. The usefulness of this chapter would be strengthened if uncertainty assessments were based on broad, well documented expert panels, including sceptical scientists from the wider community of natural scientists. [Gerbrand KOMEN, Netherlands]	The basis of the assessment is set out in a traceable manner, via the synthesis statements at the end of each subsection, through into the Synthesis Table 10.1 and thence into the ES, where therre is adherence to the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties. The chapter is open to two rounds of expert review open to all scientists including so-called "sceptical scientists from the wider community of natural scientists".
10-62	10	3	14	3	15	I dont' have a problem with the claim as stated, but I think you should also expand on it. What is the best estimate for the anthropogenic contribution to the trend since 1960? What is the best estimate of the relative contribution of different warming terms over the last 100 years? Without explicit statements (with appropriate uncertainties), misunderstanding of the single attribution statement will continue. [Gavin Schmidt, USA]	Such statements have been added.
10-63	10	3	14	3	16	It is very doubtful that it is possible to have very high confidence that it is very likely that most of the observed increase in global average temperatures since the mid-20th century is due to the anthropogenic increase in greenhouse gases. First, because there are many parts of climate system behaviours that remain uncertain: cloud mechanisms are poorly understood; ocean heat interchanges with the atmosphere are imperfectly understood; and there is some evidence that solar influences may be much more significant than generally thought (see, e.g., Shaviv (2008): Using the Oceans as a Calorimeter to Quantify the Solar Radiative Forcing. GRL, VOL. 113, A11101, 13 PP). Secondly, if unrealistic AR(1) internal climate variability models used were replaced with more realistic ones involving long-range dependence, the uncertainty bands for attribution studies would greatly increase. And where internal climate variability is estimated by long AOGCM control runs, how can anyone have "very high confidence" that such estimated variability is realistic. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	New literature is assessed showing that specifically addresses the issue of AR1 versus long-memory noise process models, and shows that basic detection results are robust to the choice. Also most detection and attribution uses model control simulations which are not AR1 noise models and which have multi-decadal scale variability as assessed in chapter 9. The confidence levels derived are used consistently with the IPCC Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties and the assessment in the ES can be traced back to the individual sections via the synthesis statements there and the snythesis table 10.1
10-64	10	3	14			Can a similar quantification of the contribution of anthropogenic ghg increases be made to the warming in global eman temperature since the start of the 20th century, as well as since the mid-20th century? I think that a similar high confidence statement could be made as for teh warming since the mid-20th century. [David Karoly, Australia]	We assess the warming since the start of the 20th century in the chapter although there are greater uncertainties in making an attributoin assessment over the whole century than since 1951 because of the longer period over which to estimate internal variability and the greater observational uncertainty in the earlier period.
10-65	10	3	15	3	21	What is the precise meaning of "increase since mid-20th century" and "warming since 1950"? Is it the 1950 to 2005 trend, 1991-2010 mean minus 1940-1959 mean, or something else? [Martin Juckes, UK]	The periodhas been changed to be specified precisely.
10-66	10	3	15			I myself am fine with "most", but it seems others are not. "At least half"? [Dáithí Stone, United States of America]	Text revised to state more than half
10-67	10	3	16	4	55	Repeated use of "caused by anthropogenic forcings" with subtle variations makes the text hard on the reader	We do not wish to introduce yet another acronym so

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						– try to introduce abbreviations for proportion attributable to anthropogenic forcings (all) and anthropogenic GHG forcings. The text could be made more concise with an acronym such as "proportional attribution of changes to human influence (PACHI)" – e.g. It is very likely that global temperature change since 1950 has PACHI > 50% (very high confidence). Or perhaps "Anthropogenic change fraction (ACF)" and "anthropogenic greenhouse change fraction (AGCF)". [Martin Juckes, UK]	have not implemented this suggestion.
10-68	10	3	18			Does this sentence imply that these other forcings have not contributed to the longer-term (multidecade) variability? [Albert Klein Tan k, Netherlands]	That was not the intention. Have inserted "also" into text.
10-69	10	3	19	3	20	This sentence leaves open the interpretation that natural "external forcing" could be as or more important than antrhopogenic greenhouse gas forcing. Is that the intent here? There is some confusion that the reader must navigate through in reading this overall paragraph then. In particular, in the previous sentence (lines 13-16) it is stated that the increase in global avg sfcT since the mid-20th century is due to the observed anthropogenic increase in GHG concentrations. Is that what is meant by "external forcing" in lines 19-22? Please clarify. [Martin Hoerling, USA]	This is not the interpretation meant but we are adhering to the IPCC uncertainty language here. The fact that we have a higher likelihood on external forcing contributing some amount than greenhouse gases contributing more than half does not mean that external forcing is more important than greenhouse gases. External forcing is defined in the IPCC glossary.
10-70	10	3	20	3	20	The likelihood of an explanation is not a useful: express it in terms of likelihood of causes. [Martin Juckes, UK]	Agreed. Text revised.
10-71	10	3	20	3	22	Very awkward. What is meant by "consistent with"? Is the intent to call out the fact that there has been a lack of a global mean warming since 1998? Is the point to indicate that such a lack of warming is nonetheless consistent with an "anthropogenic GHG induced warming trend"? This needs to be re-written to clarify precisely what is meant. It is also true that the lack of warming is consistent with many other factors, but that is not called out here. [Martin Hoerling, USA]	We have implemented a new formulation for a statement about the global mean variability since 1998.
10-72	10	3	20	3	22	"It is very likely that the evolution of global temperatures since 1998 is due to internal variability and known forcings" – use language consistent with statement on changes since mid-20th century. [Martin Juckes, UK]	We have implemented a new formulation for a statement about the global mean variability since 1998.
10-73	10	3	20	3	22	This statement seems a bit twisted from a statistical standpoint. Surely it is also at least very likely that global temperature changes since 1998 are consistent with a lack of external forcings, and in fact more consistent with their lack than with their presence. But you do not make that statement. [Dáithí Stone, United States of America]	We have implemented a new formulation for a statement about the global mean variability since 1998.
10-74	10	3	21	3	22	Some changes since 1998 are probably due to internal variability, right? Are you sure it's all forced? Pls clarify [Susan Solomon, USA]	We have implemented a new formulation for a statement about the global mean variability since 1998.
10-75	10	3	21			I suppose you mean "temperature" rather than "temperature change" [Henning Rodhe, Sweden]	We have implemented a new formulation for a statement about the global mean variability since 1998.
10-76	10	3	24	3	25	Clarify what is meant by "ocean temperatures". SSTs? Temperatures to a particular depth? Heat content? How deep of an ocean layer? Also, the statement of global enegy balance appears to be a trivial one, in so far as a 2-layer system of oceans and atmosphere requires, for balance, that fluxes at the air-sea interface exhange heat between the atmopshere and ocean. So what is meant by the statement that 90% of the earth's energy balance is taken up by the oceans? If by "oceans" is meant the entire column of the ocean to the sea floors, then a state of balance requires 100%. Is the defiict being cited meant to indicate the un-balanced state of global temperature? Of course, the oceans are not well observed below about 1 to 3km, so is the deficit from 100% being reported indicating heat exchanges to the deeper (unobserved) ocean reaches? This requires clarification. [Martin Hoerling, USA]	Text has been clarified to make clear that we are talking about ocean warming (ie sub-surface) using a similar formulation to ar4 ("the world ocean has warmed"). The energy balance does not require 100% to be taken up by the ocean as some is taken up by the melting of glaciers and ice caps, of the Greenland and Antarctic ice sheets, of the continents and atmosphere and of Arctic sea ice. But the fact that the ocean takes up almost all may be a trivial statement to a climate scientist but is an important one to make since it implies that understanding the changing energy content of the climate system requires understanding the heat taken up the ocean.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-77	10	3	24	3	26	This is an important statement. Please change the first statement to "…is CURRENTLY taken up…" since we do not expect that to be true forever. [Susan Solomon, USA]	Done
10-78	10	3	24	3	27	You have not made a comparable statement for the atmosphere. Therefore from this TS it is virtually certain we are affecting sea level but it is possible that it is not virtually certain that we are warming the atmosphere. [Dáithí Stone, United States of America]	We have not made an exaclty comparable statement about the atmosphere but there is a statement about the attribution of free atmosphere changes to anthropogenic forcings.
10-79	10	3	24		27	Why is there only a statement about steric sea level rise and nothing about the contribution from land ice melt? The Cryosphere is dealt with later but this is missing. [Kevin Trenberth, USA]	Statements about land ice melt have been inserted.
10-80	10	3	25	3	25	"ocean temperatures" should be "upper-ocean heat content". [Randall Dole, United States of America]	This text has been amended.
10-81	10	3	25			Typo "the" [Albert Klein Tan k, Netherlands]	Corrected
10-82	10	3	26			"Virtually certain that there is an influence". It's hard to argue for no influence at all, so without saying anything about the magnitude this statement is true but uninformative, because the influence could be extremely small but non-zero, in which case the virtually certain if of little use. I suggest to replace "an influence" by "at least X" or whatever is justified. [Reto Knutti, Switzerland]	We have included other statements along these lines in the ES but nevertheless still think it valuable to include statements ruling out internal variability as the entire explanation.
10-83	10	3	26			What is external forcing here? From the ocean's point of view the atmosphere and solid earth are external, which makes this statement trivial (barring a major chemical or nuclear reaction within the ocean). [Dáithí Stone, United States of America]	External forcing is defined in the IPCC glossary and this is the sense we use it here.
10-84	10	3	29	3	29	This statement gives the impression that this is a new assessment, but the assessment in Chapter 9 of the AR4 stated that "The observed pattern of tropospheric warming and stratospheric cooling is very likely due to the influence of anthropogenic forcing, particularly greenhouse gases and stratospheric ozone depletion" so it is disingenuous to lead with a sentence that suggests that this is new information. [Francis Zwiers, Canada]	In revising the ES we start with a new first paragraph outlining progress since AR4 and this statement is deleted from this point in the text.
10-85	10	3	29	3	30	As now written, the sentence implies that all of the wamring of the troposphere is likely due to AGW (anthropogenic greenhouse gas warming). Is that the intent? The authorsshould indicate how much of the magnitude of warming of the troposphere is attributable to anthropogenic forcing. [Martin Hoerling, USA]	Statement amended to statement that anthropogenic forcings have very likely contributed to observed warming.
10-86	10	3	31	3	32	Is not this sentence somewhat misleading, suggesting that the (lower , without discussing upper) stratospheric temperature decrease is mainkly due to ozone depleting substances and is not related with GHG emissions? Sentence on page 25, lines 36 to 39 is more balanced [Michel Petit, France]	GHG emissions do contribute but ODS dominate. New literature assessed in sod includes Lott et al paper.
10-87	10	3	31	3	32	Are you sure that the attribution statement should focus on ozone depleting substances, rather than on ozone itself? Is the statement based on simulations from models forced with observed ozone changes or simulated changes? [Dian Seidel, USA]	This is based on the literature that has been assessed.
10-88	10	3	32			What pattern? [Henning Rodhe, Sweden]	The pattern of tropospheric warming and stratospheric cooling observed since 1960
10-89	10	3	33	3	33	Now we read 1960? Previously on this page we saw mid-20th century. We also saw 1950 mentioned. Does it matter? Are D&A results sensitive to this manner of distinction? [Martin Hoerling, USA]	For surface temperature results we look at two periods since a year at the beginning of the 20th centuy, 1901 and since a year in the middle1951. For free atmosphere temperature changes we are limited by the obsdervational data which starts in the late 1950s and hence we choose 1960. There is some discussion of the sensitivity to periods in the literature which we assess.
10-90	10	3	36	3	48	Mention the increase in water vapour content of the atmosphere. [Martin Juckes, UK]	Done
10-91	10	3	37			"While observational and modelling uncertainties remain" applies not only to the Water Cycle, but also to the other statements in this summary [Albert Klein Tan k, Netherlands]	Agreed. Statement deleted. There are statements on remaining uncertainties including as it relates to water cycle at the end of the ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-92	10	3	38	3	41	Human influence has also been detected on tropical SSS. Your sentence suggests that salinity evidence comes only from theory and observed changes. [Laurent Terray, France]	Agreed. Sentence edited to reflect detection and attribution findings on salinity
10-93	10	3	39	3	39	global precipitation changes: only global land ? [Laurent Terray, France]	Changed to state over land
10-94	10	3	41		43	How can pattern be consistent with an "amplified water cycle"? There is no clear evidence of increases in precipitation globally, only in the intensity of precipitation. So this is not the amplification but the intensification. There should be a lot better statements about the water cycle and its changes. [Kevin Trenberth, USA]	Agreed. Changed to state intensification.
10-95	10	3	42	3	42	“significant” – significant in what sense? [Martin Juckes, UK]	Sentence edited deleting "significant"
10-96	10	3	42	3	44	It would be prudent to add something on the lines of "though the observed changes are substantially larger than the predicted changes" [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Cautionary sentence about possible mismatch added although note that recent literature (Noake et al) fiinds fewer discrepancies.
10-97	10	3	42	3	49	Both here and in the main body of the chapter, the balance between the assessment on changes in precipitation (over land) and salinity (argued to reflect P-E over oceans) need to be better nuanced. On the face of it the two assessments seem to be somewhat at odds. In the one case (precipitation on land) the assessment is informed by several D&A studies. In the other, the stronger assessment appears to be based primarily on expert judgement (I'm not aware that there are formal D&A studies). In both cases, I think it fair to say that we are observationally challenged. [Francis Zwiers, Canada]	There are observational challenges but there are formal detection and attribution studies, notably that by Terray et al.
10-98	10	3	51	3	53	This statement is too general. I don't think that we expect that every time there is a change in Arctic sea ice, or permafrost or whatever, that it is clearly due to anthropogenic climate change rather than internal variability. The next several statements are more careful and supercede this so I suggest that you delete the first sentence and go straight to the more detailed material,which has a good list of the levels of confidence. However, I am not at all sure that a statement that increased surface melt on Greenland is necessarily due to anthropogenic forcing and believe that statement should have different qualifiers. [Susan Solomon, USA]	Accept - General lead in deleted and qualifiers added for Greenland
10-99	10	3	53	3	53	Please adopt consistent usgaes. Here we read "Anthropogenic climate change", In the previous paragraphs we have read about "anthropogenic greenhoues gas induced warming trend", "fingerprint of human activity", "influence of anthropogenic forcings", "anthropogenic influence", "human influence", and "anthropogenic contribution". Do they all mean the same thing? Are the meanings of these usages defined? Are there important nuances in these usages that the reader should be made aware of? [Martin Hoerling, USA]	ES has been edited to ensure consistency of formulation and fewer forms.
10-100	10	3	53	3	54	"likely" feels far too weak a conclusion for the attribution of a human influence on Arctic sea ice decline given the fact that the observed trend lies out the lower limits of the full ensemble of CMIP3 simulations as shown in AR4. This seems to be reinforced out by Figure 10.14, which appears to show that the observed average rate of decline in sea ice extent over the past half century is greater than for any realization of any of the forced CMIP3 model simulations shown. I'm perplexed as to how the observation that the models cannot get rid of ice fast enough when subject to anthropogenic forcing over the past half century can be reconciled with such a weak level of attribution of the observed sea ice decline. [Michael Mann, USA]	Reject - Likely is based on the available science literature.
10-101	10	3	53			only "likely"???? [Kevin Trenberth, USA]	Reject - Likely is based on the available science literature.
10-102	10	3	54	3	54	“small net change”: replace with “small net increase” or “decrease” as appropriate. [Martin Juckes, UK]	Accept
10-103	10	3	54	3	54	Does "high confidence" apply to both Arctic sea ice and increased Greenland melt? [Francis Zwiers, Canada]	Accept - clarified text to make unamiguous
10-104	10	3	54	3	55	The sentence regarding Antarctic sea ice extent change should be deleted or at least re-phrased, because this sentence did not give any useful information. In the last paragraph of 10.5.1.1, it was claimed that several studies are contradictory, ans was concluded that "We therefore have low confidence in the scientific understanding ...". The exact causes of the observed Atnarctic sea ice increase are still unkwon. [Zhaomin Wang, UK]	Accept - text modified and simply says increaqse is within internal variability. Also have changed section 10.5.1.1 discussion of sea-ice extent have now included a new figure to support the ES and the overall text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-105	10	3	54	3	56	What aspects of evolving climate conditions would not be "consistent with the combined effects of anthropogenic and natural forcings and variability? If no obvious examples can be imagined, then my question is why is the confidence level "medium"? Perhaps this sentence has no information content? [Martin Hoerling, USA]	Accept - text modified and simply says increaqse is within internal variability. Also have changed section 10.5.1.1 discussion of sea-ice extent have now included a new figure to support the ES and the overall text.
10-106	10	3	54	4	31	No clear statement about Antarctic sea ice and the fact that increases in SAM increase the Ekman drift away from Antarctica and promotes increased Antarctic sea ice, as observed in many parts at times. [Kevin Trenberth, USA]	Accept - text modified and simply says increaqse is within internal variability. Also have changed section 10.5.1.1 discussion of sea-ice extent have now included a new figure to support the ES and the overall text.
10-107	10	3	57	3	57	Only 'likely' for glacier retreat? This is too low. Glacier retreat is global, and almost all associated with summer melting, rather than winter precipitation change. That makes the link to temperatures very strong, and if that is 'very likely' attributable, so is glacial retreat. [Gavin Schmidt, USA]	Glacier retreat is also affected by precipitation changes (increase/decrease) and also there is no global estimates of internal variability available for glaciers. The science doesn't support a stronger conclusion
10-108	10	3		3		The chapter refers to the Water cycle as well as the Hydrological cycle (Section 10.3 Atmosphere and Surface), and a clarification and distinction of the terms are needed. In chapter 2, mainly the Hydrological cycle is used. [Lena M. Tallaksen, Norway]	In revision we have adopted usage of chapter 2, ie hydrological cycle also because it is a term defined in the IPCC glossary.
10-109	10	3		3		The Water cycle section (10.3.2) mainly discusses the detection of human influence on precipitation patterns, atmospheric humidity, changes in ocean salinity. [Lena M. Tallaksen, Norway]	Noted.
10-110	10	3		4		The Cryosphere is given a separate section (Section 10.4) and so is Extremes (Section 10.6; note the term "Climate Extremes" is used in the Executive Summary). It is stated in the summary that it is likely that anthropogenic forcings have contributed to systematic changes in the Cryosphere. However, systematic changes in the Hydrosphere is not mentioned, including changes in the main water balance elements like evapotranspiration and runoff. Thus, the current draft does not represent a balanced view of the main components of the hydrological system as defined in Chapter 2 (2-31, line 37-38). [Lena M. Tallaksen, Norway]	Evapotranspiration and runoff are assessed in the chapter.
10-111	10	3		5		Executive summary is excellent [Larry Thomason, United States of America]	Thank you
10-112	10	3		138		The Chapter makes uneven use of the AR5 uncertainty formulation and should be homogenized in that regard [Larry Thomason, United States of America]	The use of uncertainty language has been homogenized.
10-113	10	3				Exec Summary: It is very important to refer to a specific time period when making statements about confidence in attributable trends. Eg lines 30 and 31 do not mention time period. There are attribution statements about changes in the Water Cycle that do not mention any time period. [David Karoly, Australia]	Agreed and this has been done.
10-114	10	3				If "It is extremely likely that warming since 1950 cannot be explained without external forcing." Then why is it only "likely that glaciers have diminished significantly due to human influence"; not even very likely? This seems inconsistent. [David Sauchyn, Canada]	Glacier retreat is also affected by precipitation changes (increase/decrease) and also there is no global estimates of internal variability available for glaciers. The science doesn't support a stronger conclusion unlike surface temperatures.
10-115	10	4	1	4	3	There is a certain (incorrect) deterministic view implied in this sentence when the authors write that there is low confidence that "the loss of Antarctic ice sheet mass balance is caused by anthropogenic forcing". The articulations of observed regional climate change may involve a contribution from anthropogenic forcing, but rarely(if at all) can it be claimed that a condition was caused by anthropogenic forcing, to the exclusion of other factors. [Martin Hoerling, USA]	Accepted - revised text, exclusion of other factors is crtainly not meant, and revised acordingly
10-116	10	4	4	4	12	It could be argued that you are extremely biased here. You only discuss extremes for which there is some argument of an increasing frequency under anthropogenically driven climate change. What about the extremes that are decreasing in frequency, such as cold extremes? [Dáithí Stone, United States of America]	Text edited to refer to changes in extremes.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-117	10	4	5	4	8	There is no obvious distinction between extreme temperatures and heat waves and why the anthropogenic forcing is very likely in one case and only likely in the other. [David Sauchyn, Canada]	Statement about heatwaves deleted
10-118	10	4	5	4	12	This appears inconsistent with IPCC SREX Ch. 3 There, in its Executive Summary, one reads that "Many weather and climate extremes are the result of natural climate variability". Here in Ch 10 one gets no such sense. Rather one reads of a strengthening of the evidence for human influences on temperature and precipitation extremes. I suspect that these views can be easily reconciled by informed climate scientists, but that is unlikely to be true for the general reader and policy maker to whom this IPCC assessment is directed. This requires due attention from the authors. The authors need to indicate what new information has emerged since SREX that warrants a change in the assessment of climate extremes. [Perhaps this is addressed in section 10.6, but a mention here is needed also]. [Martin Hoerling, USA]	In the ES we make it clear in a new paragraph the advances since the AR4. In the extremes section we have formulated sentences slightly different from their corresponding ones in SREX and therefore do not provide in the short space here a detailed breakdown. There is discussion of comaprisons with SREX in 10.6.
10-119	10	4	6	4	8	The two consecutive sentences express basically the same statement in two different ways. Why having two sentences? What is the reason for presenting the second one as less certain than the first one? [Michel Petit, France]	Rejected. These 2 statements express different concepts as covered in 10.6.1 and 10.6.2 respectively.
10-120	10	4	6	4	9	Only changes in frequency of climate extremes is mentioned here. Changes in intensity is important as well and needs to be added as appropriate. [Seung-Ki Min, Australia]	ES summarises main results coming through from the assessment in the chapter.
10-121	10	4	7			Unclear statement. "some"?? [Henning Rodhe, Sweden]	This is the statement from the chapter. It is not possible to say all heatwaves.
10-122	10	4	10	4	12	Except in the Atlantic, where the evidence strongly weighs in favor of an anthropogenic effect. [Kerry Emanuel, United States of America]	See discussion of tropical cyclone activity in the chapter.
10-123	10	4	12	4	12	Here, and a few other places in the chapter, the authors use the term "low level of scientific understanding". This harkens back to a type of uncertainty language used in early IPCC reports. The current guidance suggests that confidence be assessed on the basis of evidence and agreement, so I would suggest that the authors use that type of language in justifying their "low confidence" assessment. [Francis Zwiers, Canada]	Language has been made consistent with IPCC AR5 uncertainty language and text amended as suggested.
10-124	10	4	15	4	31	The non-expert reader will read these paragraphs to imply that greenhouse gases have caused Antarctic changes, since the ozone hole isn't mentioned until line 31. Please avoid the confusing language about 'anthropogenic influence', 'anthropogenic forcing', etc., and reorder this to be clear. One way to do that is to provide a starting sentence on line 15 that says 'A number of climate changes in the Southern Hemisphere have been observed and attributed to the Antarctic ozone hole, while increases in long-lived greenhouse gases are the dominant forcing over the rest of the globe.' Then go on to say clearly what occurs where. [Susan Solomon, USA]	Text has been edited to make clear what referring to stratospheric ozone changes.
10-125	10	4	16	4	17	This statement is still heavily based on Stott (2003), with Min and Hense (2007) the only other study that comes to mind as directly supporting it? In that case it should probably be indicated that confidence remains lower than it might be. [Dáithí Stone, United States of America]	This is part of the assessment as discussed in the main body of the chapter.
10-126	10	4	17	4	18	How can there possibly be medium confidence that anthropogenic influence has made a significant contribution to warming in Antarctica, when what is very probably the highest quality study shows that there has been no significant warming in Antarctica as a whole over the last 50 or so years, prior to which there is very little instrumental data available? I refer to ODonnell, Lewis, McIntyre and Connon (2011): Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al (2009) Antarctic Temperature Reconstruction, J.Climate, vol.24, p2099-2115, DOI: 10.1175/2010JCLI3656.1 (of which I am one author). That study showed that the significant continental warming found by Steig et al. (2009) over 1957-2006 was an artefact of faulty mathematical methodology, and that with corrected methodology there was no significant warming over that period. Moreover, even if Steig et al (2009)'s trend estimates had been correct, its finding of significance were invalid since no account was taken of the very large uncertainty in the temperature reconstruction (which, inter alia, was only based on three principal components); the only uncertainty allowed for was that in fitting a trend line to the reconstructed temperature record. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accept text revised to reflect the state of science on warming of Antarctica and becomes low confidence based on observational uncertainties.
10-127	10	4	18	4	18	When talking about Antarctic warming, the time period should be given, since there was a cooling for the	Agreed. Text amended.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						recent decades (1979-2003) (O'Donnell, Ryan, Nicholas Lewis, Steve McIntyre, Jeff Condon, 2011: Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al. (2009) Antarctic Temperature Reconstruction. J. Climate, 24, 2099–2115, doi: http://dx.doi.org/10.1175/2010JCLI3656.1) [Zhaomin Wang, UK]	
10-128	10	4	18			Check with chapter 2 on Antarctic temperature changes: certainly in the Antarctic Peninsula there are surely very likely changes, but elsewhere? [Kevin Trenberth, USA]	Accept text revised to reflect the state of science on warming of Antarctica.
10-129	10	4	19	4	21	And because of the fact that not all relevant processes are modelled at regional scales? [Albert Klein Tan k, Netherlands]	Agreed
10-130	10	4	21	4	22	Remove, what reads to be a speculative statement, rather than an assessment of exisiting peer-reviewed literature. If it is evidence-based, then rewrite the sentence accordingly. Should the word "likely" be italicized here? [Martin Hoerling, USA]	Text amended
10-131	10	4	22			Presumably this "likely" is not supposed to be in italics? [Dáithí Stone, United States of America]	Text amended
10-132	10	4	24	4	25	But they are part of "the effects of external forcings on climate". [Dáithí Stone, United States of America]	Text amended.
10-133	10	4	25	4	27	This sentence about tropical expansion and the Hadley circulation is a little strange. To me, the expansion of the Hadley circulation is a manifestation of increases in the width of the tropical belt, not a result thereof. [Dian Seidel, USA]	Text amended
10-134	10	4	29	4	30	Rewrite, or better yet, remove the sentence. In what sense might changes in the NAO not be "consistent with" natural internal variability? is the alternative hypothesis that it is "inconsistent" with natural internal variability? Perhaps the additional word "alone" might be of help. But that begs the question of why the NAO has been called out in this regard. Why then why not also call out the PNA, EPO, WPO, PDO etc? [Martin Hoerling, USA]	Statement has been deleted
10-135	10	4	29	4	30	"Changes in NAO is consistent with internal variabilty". This would become important information, being different from AR4 conclusion. However, given no studies to explain separate natural and anthropogenic infuence on NAO or AO over different analysis period, this conclusion sounds too strong and may be misleading. "Low confidence in anthropogenic influence" seems better as in 10-33. [Seung-Ki Min, Australia]	Statement has been deleted
10-136	10	4	29	4	30	Do you mean "not inconsistent"? [Dáithí Stone, United States of America]	Statement has been deleted
10-137	10	4	29	4	30	medium confidence: I suggest rather high confidence [Laurent Terray, France]	Statement has been deleted
10-138	10	4	30	4	31	This statement needs support as differences between hemispheres in atmospheric circulation changes are also found in CO2 only runs so another possibility is the SH circulation responds more clearly to external forcing compared to the NH [Julie Arblaster, Australia]	Sentence has been deleted
10-139	10	4	30			"...Northern Atlantic Oscillation..." should be "North Atlantic Ocean" [Omer L. Sen, Turkey]	Sentence deleted
10-140	10	4	33			How is this sub-section linked (or not) to the Chapter 5 (sub-section 5.4.3)? [David Sauchyn, Canada]	Assessment is consistent with observational assessment of chapter 5 but here we are assessing the causes of observed changes.
10-141	10	4	33			"Millennia" and "multi-millennium" versus "multi-century" and "centuries"... [Dáithí Stone, United States of America]	Rejected
10-142	10	4	34	4	41	Here one reads that a "substantial part" of inter-decadal temperature variability in pre-industrial times results from natural external forcing, though such forcing fails to explain more recent temperature changes. Is the latter result mainly an outcome of an absolute weakening of natural external radiative forcing in recent decades (comared to pre-industrial) or a relative weakness of such forcing compared to anthropogenic greenhouse gas forcing in recent decades? Looking ahea to the next section of the ExSum, what are the implications for projections for coming decades, which do not (to my understanding) include any (stochastic) projections for volcanic aersol emissions, or for variability in solar output? [Martin Hoerling, USA]	Text has been revised to include implications for observed warming.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-143	10	4	35	4	36	The “internal variability of the climate system” does not “move heat around the climate system”; it is the response to radiative forcing and feedbacks. Delete this description of internal variability from this sentence. [David Sauchyn, Canada]	rejected. We are simply sayin internal variabilit can move heat around the system, we are not saying that is all there is to it.
10-144	10	4	36			Is a comma missing after "...the climate system"? [Omer L. Sen, Turkey]	Inserted thank you.
10-145	10	4	40	4	40	"more recent warming": it refers to last century or to last 35-40 years? [Claudio Cassardo, Italy]	Explicit date inserted.
10-146	10	4	40	4	40	“recent” probably needs to be clarified, to avoid confusion with steady temperatures since 1998. [Martin Juckes, UK]	Explicit date inserted.
10-147	10	4	44	4	45	“More observational data ...”: poor english: “Ever increasing volumes of observational are leading to better ….” [Martin Juckes, UK]	Rejected. Present formulation is shorter.
10-148	10	4	44		55	contradictory statements about temperature range [Tim Barnett, USA]	These are the assessments obtained in the chapter. Temperature ranges are not contradictory since they are expressing different properties of the climate system.
10-149	10	4	45	4	49	“indicates that …. is estimated to be very likely ….”: observations do not indicate an estimate. Merge these two sentences for clarity and conciseness: “New evidence and improved methodologies have strengthened the observational constraint on the Transient Climate Response (TCR): TCR is very likely in the range …..” [Martin Juckes, UK]	Text amended along these lines..
10-150	10	4	46	4	46	"Transient climate response" to what? Please clarify. [Martin Hoerling, USA]	TCR is defined in the IPCC glossary
10-151	10	4	46	4	46	“were not yet available to AR4”: either “were not available to AR4” or “were not yet available at the time of AR4” [Martin Juckes, UK]	Text amended.
10-152	10	4	46	4	46	In order to make this assessment accessible to policy-makers, it would be useful to define TCR in the executive summary. [Francis Zwiers, Canada]	TCR is defined in the IPCC glossary
10-153	10	4	46	4	47	Gillett 2011a, which study is (at least in principle) superior to most other climate model based attribution studies because of its use of a longer (1851-2010) period of temperature observations, concludes that it is extremely unlikely that the TCR exceeds 1.8 C, far below the 3 C quoted here. Gillett 2011a concludes that their tight constraint on the upper TCR level arises from using the longest possible period of reliable surface temperature data, whereas other studies have typically used a 1901-2000 period, which started with an anomalously cool period and ended with a period in which both greenhouse gas concentrations and temperatures were rising rapidly. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The assessment now includes more papers than Gillett et al which come to different conclusions.
10-154	10	4	46	4	52	The information presented in this paragraph is demonstrably wrong. In the first place, there is no empirical advantage of relative over absolute SST on time scales longer than about two years. One of the most important theoretical links between tropical cyclone intensity and thermodynamic conditions is the degree of thermodynamic disequilibrium between the ocean and the atmosphere, which is controlled on time scales of more than about two years by local surface radiative forcing and ocean heat convergence. Also, basic theory, single-column models, and experiments with coupled global models all show that potential intensity and absolute SST correlate; there is no need for increased gradients. There is certainly very powerful evidence, contrary to what is stated in this paragraph, of a link between tropical cyclone intensity and absolute SST. [Kerry Emanuel, United States of America]	This comment is out of place (ie referring to wrong lines). There are responses to the review comments in the appropriate place.
10-155	10	4	47	4	53	“very likely greater than 1C and very unlikely greater than 3C” – it would be more natural to give the range that TRC is very likely to be within (slightly broader than 1-3C, of course) – as is done for response to CCE. [Martin Juckes, UK]	The range given is what the assessment reached and allows comparison with the AR4 range which was done like this.
10-156	10	4	48	4	49	“wider range of studies”: there is no reason why a wide range of studies should reduce the uncertainty range: has there been an improvement in methodology? [Martin Juckes, UK]	There are more models that have been used to make this assessment. Statement amended.
10-157	10	4	49	4	51	Doesn't this belong in one of the later chapters? [Dáithí Stone, United States of America]	This belongs here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-158	10	4	52	4	53	"ºC/TtC" should be explained [Helga Nitsche, Germany]	These are standard sI units
10-159	10	4	53	4	55	Stating that climate sensitivity is likely to be above 2 C and very likely to be above 1.5 C is too strong when there are a number of studies suggesting that it is below 2C or even below 1.5 C. E.g., Forster and Gregory 2006 gave a central estimate of 1.6 C (and an Extremely likely 95% upper confidence limit of 3.3 C - not 14.2 C as erroneously stated in Table 9.3 of AR4 WG1, which does not reflect the study's actual findings). And Lindzen and Choi 2009 suggested a climate sensitivity of below 1 C, a result supported by an improved and corrected study that the same authors published in 2011 (On the Observational Determination of Climate Sensitivity and Its Implications. Asia-Pacific J. Atmos. Sci., 47(4), 377-390.) [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	We reflect the assessment on ECS in the ES.
10-160	10	4	54	4	54	"equilibrium climate sensitivity" to what forcing? Please clarify. [Martin Hoerling, USA]	This term is defined in the IPCC glossary
10-161	10	4	54	5	3	This is a one-sided review of appraisals. Certainly Emanuel (Nature, 2005; BAMS, 2008) and Mann and Emanuel (2006) make strong cases for an anthropogenic fingerprint on Atlantic tropical cyclones. [Kerry Emanuel, United States of America]	Noted. The text is modified. However, there are other important factors such as wind speed and outflow temperatures that also have strong affect on tropical cyclone activities.
10-162	10	4	55	4	55	"climate sensitivity" to what? [Martin Hoerling, USA]	ECS is a term defined in glossary. Text amended to refer to ECS.
10-163	10	4	57	4	57	Subsection title is ambiguous: could mean “not yet discussed” or “Uncertainties in current understanding”, [Martin Juckes, UK]	Remaining is clear.
10-164	10	4	57	5	12	Are there no remaining uncertainties regarding Detection? The summary (a good one) is all about remaining uncertainties in attirbution. I the absence of detection, attribution is somewhat of a mute point (unless one has recourse to multi-step attribution information, but this too requires detection of some change in the climate system). [Martin Hoerling, USA]	Agreed. The title remaining uncertainties refers to the subject of the chapter, namely both detection and attribution. Sentence on internal variability inserted.
10-165	10	4	57	5	12	The authors need to reconcile this list of extensive challenges and gaps in the science of conducting attribution, especially for regional conditions and extremes, with the prior statements of accumlated and strengthened evidence for human causes of regional trends and climate extremes earlier in this ExSum. How is the reader to connect this last section of the ExSum with the first parapgraph of the ExSum, for instance? Please clarify. [Martin Hoerling, USA]	This is clearer in revision because we outline explicitly the progress made since the ar4.
10-166	10	5	1	5	7	This discussion, along with the continental statements above, sound as or more equivocal than in AR4 (bar those concerning Antarctica). Is this deliberate? [Dáithí Stone, United States of America]	In para at start of ES we outline explicity progress since the ar4.
10-167	10	5	1	5	12	These issues should be covered in preceding paragraphs – this paragraph is redundant. [Martin Juckes, UK]	Do not agree. We need to explicitly lay out the remaining uncertainties.
10-168	10	5	11	5	12	The ability is also limited by the availability of observations of extreme events. [Lena M. Tallaksen, Norway]	Agreed. Text amended.
10-169	10	5	12			Also, the fact that good observational databases of extremes are lacking plays a role. Refer to Ch2. [Albert Klein Tan k, Netherlands]	Agreed. Text amended.
10-170	10	6	3	6	3	I complained about this for the ZOD as well. The chapter is an inanimate object, so it can not seek to understand anything. The objective of the chapter is to report an assessment and provide the arguments that form the basis of that assessment, so I suggest an opening sentence along the lines of "This chapter assesses the understanding of the observed changes that …". [Francis Zwiers, Canada]	Agreed. Text amended.
10-171	10	6	3	6	5	Suggest to change "This chapter seeks to understand the causes of the observed changes that were assessed in Chapters 2 to 5. 4 The chapter uses physical understanding, climate models and statistical approaches to assess the causes of 5 observed climate changes." to read "Using physical understanding, climate models and statistical approaches, this chapter seeks to understand the causes of the observed changes that were assessed in Chapters 2 to 5." [Xuemei Wang, China]	Text amended

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-172	10	6	3	6	11	It does eveything except show that all this understanding enables to find out what is going to heppen in the future, by a successful forecast. The Chapter is merely a set of simulations, which are an addition to what has already been puiblished in Chapter 9 [VINCENT GRAY, NEW ZEALAND]	The chapter assesses more than a set of simulations, It brings in physical understanding as well and assesses the extent to which expected fingerprints of the climate response to natural and anthropogenic forcings have emerged in the observastions.
10-173	10	6	3			Section 10.1: I enjoyed reading this section but think it is too long. Some details of the history of ARs could be shortened. [Christian Huggel, Switzerland]	Accepted. New material has also been added to cotextualise later sections (eg some definitions and terminology were added to this section) and is still shorter
10-174	10	6	6	6	6	It would be useful to define exactly what is meant by "natural internal variability" if this is the term that is to be used. Usage should also be consistent throughout the chapter. To me, "internal variability" is the unforced variability associated with ocean/atmosphere dynamics. In contrast, "natural variability" would also include the variability associated with changes in naturally varying forcings (e.g. volcanic aerosols, solar forcing). It is not clear to me which of these definitions should apply to "natural internal variability". [Chris Roberts, Uk]	Text amended to refer to "internal variability" rather than "natural internal variability". A short explanation has been added.
10-175	10	6	12	6	32	Move line 28-32 before current line 13, to introduce chapter sections in appropriate order. [Martin Juckes, UK]	Text has been reordered.
10-176	10	6	13	6	13	Suggest to change "looks right across the climate system" to read "examines the entire climate system" [Xuemei Wang, China]	Text has been substantially revised and shortened to avoid duplication.
10-177	10	6	26			Suggest to be as clear as possible about the objective of this chapter: the wider scope as indicated in line 7, or the narrow scope as indicated in line 26 [Albert Klein Tan k, Netherlands]	Agreed. Text clarified.
10-178	10	6	36	6	36	“incomplete” – this does not say what is intended: “There is decreasing observational ...” [Martin Juckes, UK]	Agreed. Text amended.
10-179	10	6	39			Models also need to be assessed for their ability to simulate the phenemona that likely caused the event (modes, teleconnections, blocking, monsoons etc). [Kevin Trenberth, USA]	That sense is implicit in the text referring to models being assessed for their reliability at representing climate variability in the region in question. Therefore no text added to ensure brevity.
10-180	10	6	46	6	46	The reference to the attribution statement in the FAR is incorrect and suggests a stronger statement that the actual statement. I suggest you use "The balance of evidence suggests a discernible human influence …" which is a lot weaker than there was a "discernible" human influence. [David Karoly, Australia]	Agreed. Text amended.
10-181	10	6	49	6	53	These two sentences are correct, but seem in contradiction if concatenated in this way. I suggest to rewrite or put these sentences further apart. [Albert Klein Tan k, Netherlands]	Agreed. Text shortened to save some space and to avoid such confusion.
10-182	10	6		7		Section 10.1 need not be broken into many small paragraphs, need re-writing, suggestion to refer chapter sub-sections in sequential manner [ABHA CHHABRA, INDIA]	Some rewrting has been done to ensure chapter sub-sections are referred to in sequential manner and to avoid duplication.
10-183	10	7	9		47	This review refers to "formal attribution studies" as if they are a good thing, and they are but they are also very limiting and suffer from a number of problems highlighted by Trenberth (2011 in WIRES Clim Ch). I believe this should be discussed here. [Kevin Trenberth, USA]	"Formal" has been deleted since what is meant by detection and attribution has already been specified earlier in the section. Discussion of the limitations of attribution as outline by Trenberth is deffered to the methodology section.
10-184	10	7	13	7	14	Awkward wording. Consider changing "but evidence … lacking" to "but there is no evidence for human influence on the temperature of the hottest day o the year." [Dian Seidel, USA]	Text reworded to make clearer.
10-185	10	7	17	7	17	Suggest replacing "were consistent with expectations" with "were assessed to be qualitatively consistent with expectations". [Francis Zwiers, Canada]	Agreed. Done.
10-186	10	7	23	7	23	missing word: …of tropospheric warming and stratospheric cooling… [Helga Nitsche, Germany]	Agreed. Text revised.
10-187	10	7	33	7	33	Ishii and Kimoto, 2009, Reevaluation of Historical Ocean Heat Content Variations with Time-Varying XBT and MBT Depth Bias Corrections, J. Oceanogr and Levitus et al, 2009, Global ocean heat content 1955–2008 in	Text revised to note that at this stage these are examples. The full discussion is in the appropriate sub

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						light of recently revealed instrumentation problems, should also be cited as correcting detected data problems. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	sections.
10-188	10	7	33			Refer to Ch2 for assessment of advances in observations [Albert Klein Tan k, Netherlands]	Agreed. Done.
10-189	10	7	44			Should also mention that many models also include simulations for GHG focing only. [David Karoly, Australia]	Agreed. Done.
10-190	10	8	1			section 10.2.1. and Box 10.1 The dismissal of Trenberth (2011) on p 13 l 52 is poor. The fact is the current attribution methods are extremely conservative and err very much on the side of underestimating the human contribution and making type 2 errors. This deserves discussion. The errors in models, in particular, seriously challenge the results of many studies as likely wrong based on other aspects of understanding. e.g. the statement p 9 l 33 should be challenged as an inappropriate null hypothesis in many cases. Perhaps Box 10.1 should also include examples of how they don't work because the model is incapable of simulating precipitation or blocking or monsoons! [Kevin Trenberth, USA]	Noted, and the possibility of type 2 errors will be highlighted Box 10.1 needs to be shortened, but including how studies don't work is a good idea
10-191	10	8	3	8	4	Mere correlations do not establish a causal link. For that you have to have successful forecasts. [VINCENT GRAY, NEW ZEALAND]	Noted: The basis of attribution is hypothesis-testing with physically-based models: both forecasting and hindcasting can be used for that .
10-192	10	8	3		8	Surely attribution can also be to natural phenomena, such as ENSO? The attribution described here is quite narrow. From the standpoint of the atmosphere, attribution could be to other parts of the climate system and may relate to the slow reposne or inertia of other components. [Kevin Trenberth, USA]	Noted: We are using the word attribution, specifically to external drivers, as traditionally used by IPCC.
10-193	10	8	10	8	19	All you have are "estimates" and "understanding" is subjective, however :"physically based" [VINCENT GRAY, NEW ZEALAND]	Rejected: comment unclear
10-194	10	8	10	8	19	In line 10 the authors may add: four D&A elements in physical climate science studies. Number 4 of the four elements is likely the most difficult one and needs further discussion. While it is logic and straightforward at first glance it is often very complicated in detail. One typically needs to know and understand the external drivers and their effects. Line 32 is an important clarification in this respect. [Christian Huggel, Switzerland]	Noted: although line 32 also needs clarification on the meaning of "statistical"
10-195	10	8	10	8	19	The primary requirement for any D&A study is observations, so shouldn't this be listed first with some elaboration rather than being relegated to a short bullet that is virtually hidden? [Francis Zwiers, Canada]	Accepted, bullets will be reordered
10-196	10	8	17	8	19	Do these “random and chaotic fluctuations generated in the climate system” include the consistent modes of variability linked to ocean-atmosphere oscillations? [David Sauchyn, Canada]	Sentence will be clarified to "random, quasi-periodic and chaotic fluctuations"if space permits
10-197	10	8	21	8	21	chaotic; § 10.8, line 21.For the topographic mean flow interaction ,in the atmosphere as well as in the ocean, systems with layered topography, non linear instability (by example, Helmholtz-Rayleigh-Taylor instabilities are developed and chaotic solution dynamics emerges for limit regime.[Andraw Majda and Xiaoming Wang: Non linear dynamics and statistical theories for basic geophysical flows; Cambridge university press; 2006; chapter 5; Iterations of Poincare maps, p.158 and 171]. By chaotic, me mean a system of non linear equations of evolution, or a dynamic system, with a finite or infinite number of dimensions, in RN or $L^2(\Omega)$ such as there exist a external driver that can be increased and which crossing some tipping point, change increasingly the behavior of the system, with dense trajectories in the phase space. [Robert DAUTRAY, France]	Noted: not clear if a change is needed
10-198	10	8	21	8	23	Not sure I like the way this is expressed. Any way of re-wording it to avoid giving the impresson the climate system is totally chaotic with thus no predictability at all? [Gareth S Jones, UK]	Noted: the point is true and important, though. We will clarify
10-199	10	8	21	8	32	The chaotic nature of the climate makes it certain that you cannot get away with mere "detection. "attribution" "finer prints" or "evaluation". You have got to have proof of widespread successful predictability. [VINCENT GRAY, NEW ZEALAND]	Noted: although the important role of hypothesis testing through hindcasts will be emphasised
10-200	10	8	22	8	22	Change "between" to "among", as there are more than two components. [Dian Seidel, USA]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-201	10	8	24	8	24	“require an explanation …. ”: talk more directly of the physical system: “does not necessarily involve an external driver”. [Martin Juckes, UK]	Accepted
10-202	10	8	27	8	27	“does not call into question the existence of an attributable warming trend”: it would be clearer to say either “does not call into question the existence of a long term warming trend” or “does not prevent the attribution of a long term warming trend”. [Martin Juckes, UK]	Accepted
10-203	10	8	31	8	31	change 'identified" to "identifiable" [Xuemei Wang, China]	Accepted
10-204	10	8	37	8	39	I prefer the formulation used in AR4 (Hegerl et al. 2007): “‘Detection’ is the process of demonstrating that climate has changed in some defined statistical sense, without providing a reason for that change (see Glossary). In this chapter, the methods used to identify change in observations are based on the expected responses to external forcing (Section 9.1.1), either from physical understanding or as simulated by climate models. An identified change is ‘detected’ in observations if its likelihood of occurrence by chance due to internal variability alone is determined to be small.” The first and 3rd sentences are virtually as cited from Hegerl et al (2010), the 2nd sentence provides the justification for talking about detection relative to an internal variability threshold, rather than, say, detection relative to measurement uncertainty. Without this sentence, line 38 is rather arbitrary. [Martin Juckes, UK]	Noted. We will attempt to clarify, but it is important to remain consistent with the cross-working-group agreed terminology
10-205	10	8	37	8	39	Note more exlicitely in this definition that the estimates of internal climate variability are key. This includes questions whether relatively short model simulations and observational datasets can be used to describe natural variations in an adequate way. It is now at the very end of 10.2.1 on p9, which seems to far away from the messages here. [Albert Klein Tan k, Netherlands]	Accepted, if space permits
10-206	10	8	41	8	44	This sentence seems to be tripping on words, particularly the word "response". The notion of an observed response in particular seems unclear since the response in observations to external forcing can seldom be identified directly. "Estimated" (from observations) and "expected" (from models) I thnk would better clarify the point that is being made. [Francis Zwiers, Canada]	Accepted, sentence will be clarified
10-207	10	8	43	8	46	This is not clear to me. What is the difference between "detection of climate change" and "detection of a change in the observed variable or closely associated variables"? [Gareth S Jones, UK]	Accepted, will clarify that the key here is "closely associated variables"
10-208	10	8	44	8	46	Can we have the scientific justification, rather than implying that it is being done differently because someone has re-written the rules? Or perhaps simply say that the term “attribution” is used more broadly to include both direct and indirect inference of a causal relationship – it is really the terminology that has changed, rather than any new methodology being developed or admitted. [Martin Juckes, UK]	Accepted: we will make absolutely clear this is a terminology change
10-209	10	8	44	8	46	How do you avoid that this opens the possibility to link nearly all changes to global temperature increase (as was done often in AR4 WG2)? [Albert Klein Tan k, Netherlands]	Scientific judgement has to be used in the assessment of "closely associated" -- will clarify
10-210	10	8	47	8	49	“for example …..”: I hope that no one tries to argue that observed trends in the mean constitute evidence that extremes are changing – the vague speculation in this sentence should be omitted. It is a more or less direct quote from Hegerl et al. (2010), but they do not provide any justification for it either: it appears in a general discussion with no evidence that there is credible methodology for this kind of inference. [Martin Juckes, UK]	Accepted, sentence will be clarified
10-211	10	8	51	8	51	errnoneous word: .. work focuses on… [Helga Nitsche, Germany]	Accepted
10-212	10	8	51	8	51	Typo, "focuses" (not "forcuses"). [Christian-D. Schoenwiese, Germany]	Accepted
10-213	10	8	53	8	53	Consider changing "all relevant forcings" to "all known relevant forcings" or even "some known relevant forcings", to leave open the possibility that there are unknown but relevant forcings. [Dian Seidel, USA]	Accepted
10-214	10	9	4	9	6	"To allow for the possibility that models may over- or under-estimate the magnitude of the response to individual forcings by different factors, it is normally assumed that the responses to different forcings add linearly,…" Please clarify that this a very tenuous assumption and has not been proven to be correct in complex models due to nonlinear feedbacks that operate. [Mark Z. Jacobson, U.S.A.]	Noted: studies have found the linearity assumption is acceptable for temperature
10-215	10	9	4	9	7	See comment to page 11, lines 4-17, and note that the neural network technique allows the assessmemnt of	Noted: although there are few if any attribution studies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						non-linear forcings. [Christian-D. Schoenwiese, Germany]	using neural networks
10-216	10	9	4	9	11	The following paper discuss the additivity of temperature and precipitation at global and continental scales. Shiogama, H., D. A. Stone, T. Nagashima, T. Nozawa, and S. Emori 2012: On the linear additivity of climate forcing-response relationships at global and continental scales. International Journal of Climatology, submitted [Hideo Shiogama, Japan]	Accepted
10-217	10	9	13	9	16	I'm not sure "errors" is the right word to describe this. There is nothing "wrong" in that sense in the models, since both sensitivity and forcing are within their uncertainty ranges. At least a sentence explaining a bit more would help. Also, I disagree with wording of "underestimated spread". It's underestimated compared to a case where sensitivity and forcing are assumed to be individually constrained by observations, and independent. But it's not obvious that this is a useful way to think about it. Since we know the observed warming, it's natural that the model range is more narrow if that information is used. So I wouldn't say the model range is underestimated, it's simply conditional on the observed warming in a Bayesian sense. Such an interpretation is discussed in more detail in Knutti GRL 2008. [Reto Knutti, Switzerland]	Noted: we will clarify that "errors" here does not mean the models are wrong, merely that the probability of any one model having the correc sensitivity is zero
10-218	10	9	13	9	31	This paragraphs could be reduced in length. [Christian Huggel, Switzerland]	Noted: we will try
10-219	10	9	13	9	31	Excellent paragraph, pls don't change a thing. [Susan Solomon, USA]	Rejected, sadly. See response to 218 and 219
10-220	10	9	17	9	17	delete a blank: .. temperature warming…. [Helga Nitsche, Germany]	Accepted
10-221	10	9	17	9	17	Typo, "warming" (not "warm ing"). [Christian-D. Schoenwiese, Germany]	Accepted
10-222	10	9	20	10	56	The way scaling factors are introduced and discussed is bothersome. It assumes these are well determined and an accepted methodology, yet the fact that a scaling is used indicates something wrong and this is really a fix, and one that is easily criticized. The methodology seems to rely on the ability to find a unique signature of a forcing, but it is not clear that this is always possible. In particular, what about the indirect effects of aerosols? Maybe the method works OK for simple cases and few forcings, but aren't there problems with it that should be called out? The risk in the current approach is open up the chapter to heavily criticism. It needs to be done well and with some caution. [Kevin Trenberth, USA]	Noted: we will stess that scaling factors are simply a way of expressing results from Hasselmann (1997) multi-fingerprinting approach.
10-223	10	9	22	9	22	Is it possible for a scaling factor to be negative? Should "larger than 0" be changed to "different from zero"/ [Dian Seidel, USA]	Noted: need to clarify with authors of IPCC guidance note how to treat cases of detection with physically inconsistent sign.
10-224	10	9	25	9	40	The material in lines 25-26 seems inconsistent with the material in line 40, regarding the importance of model sensitivity. [Dian Seidel, USA]	Accepted: parenthetical remark will be deleted in line 40
10-225	10	9	29			Fix reference (ERL piece). [Reto Knutti, Switzerland]	Accepted
10-226	10	9	30			Consider highlighting this more upfront. [Albert Klein Tan k, Netherlands]	Noted, if space permits
10-227	10	9	39			And by uncertainties in the observations. [Albert Klein Tan k, Netherlands]	Accepted
10-228	10	9	41	9	41	I don't think Berliner's main point was on the effects of multiple testing per se, but rather on the interpretation of a second test (is the scaling factor consistent with unity) on the same statistic that is applied only after rejection of an intial test (is the scaling factor inconsistent with zero). [Francis Zwiers, Canada]	Accepted
10-229	10	9	46			estimates of internal variability [Laurent Terray, France]	Accepted
10-230	10	9	51	10	54	I think box 10.1 would be easier to follow if the links between mulitple linear regression, the equation for a plane (and the gradients in a best-fit plane), and the scaling factors in figure panel c were made more explicitly. The links are not made elsewhere in the text, despite reference to regression-based methods throughout the chapter. Also, there is no mention in the text of how the black diamonds (in panel c) were created from the control simulations. [Chris Roberts, Uk]	Accepted: Box 10.1 will be clarified

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-231	10	9	56	9	56	The statement "… attribution of observed changes is not possible without models …" contradicts the statement on page 11, lines 4-5, that "Some attempts … have attempted to avoid the use of climate models". It should be modified or added, resoectively, that the orientation to models is the usual view but that statistical methods avoiding models may also be helpful. However, they should be compared with the results of these models or, at least, the related radiative forcings. [Christian-D. Schoenwiese, Germany]	Noted: need to clarify that "models" here includes the models that are often implicit in statistical studies.
10-232	10	9	57	9	57	It is not only "detailed, global observations" that are needed and lacking, but detailed global observations that are of sufficiently high quality to allow detection of changes. [Dian Seidel, USA]	Accepted
10-233	10	9	57	10	1	Models are always needed, regardless of the quality of the observations. Or to put it another way there is no clean ‘control’ available for any forced change of climate in the real world. [Gavin Schmidt, USA]	Accepted
10-234	10	9				ERL Piece? [Larry Thomason, United States of America]	Accepted
10-235	10	10	5	10	5	"panel a": please cite the figure. [Claudio Cassardo, Italy]	Accepted
10-236	10	10	5	10	8	Reference to Fig. 1 is missing. [Christian-D. Schoenwiese, Germany]	Accepted
10-237	10	10	28	10	29	The raw statement "can be attributed" suggests that attribution is an automatic consequence of the finding that scaling factors are consistent with unity. This is an aide to attribution, but needs to be supplemented with physical interpretation and arguments that would eliminate the possibility that other factors (internal variability, or confounded signals from forcings not considered directly in the analysis) might provide a plausible explanation for the observed changes. This point was made in both Ch 9 of the AR4 and Ch 12 of the TAR, and should continue to be made in my view. [Francis Zwiers, Canada]	Accepted, good point (and useful words)
10-238	10	10	32	10	39	I'm concerned about using an example based on a simple time-only analysis of global mean temperature anomalies that could potentially produce an uncertainty band that may not be fully consistent with the more complete space time analyses presented elsewhere in the chapter. Also, I'm concerned about a lack of caveats - going directly from an analysis of the global mean to attributable warming seems a stretch. [Francis Zwiers, Canada]	Noted: the fact that this is purely a heuristic demonstration will be emphasised, but we also need to demonstrate that simple approaches work, although they may be sub-optimal
10-239	10	10	47	10	52	(Box 10.1, Fig. 1 caption) This caption refers to panels a), b), and c) which, however, are indicated in the related Figure by (A), (B), and (C). [Christian-D. Schoenwiese, Germany]	Accepted
10-240	10	11	4	11	17	Another method avoidung climate models are neural networks (NN) based on observations only. They imply the advantage that multiple non-linear cause-effect relationships are allowed. See reference given in comment to chapter 10, page 15, line 8. [Christian-D. Schoenwiese, Germany]	Noted: neural network models still assume an implicit model of the response, just not a GCM.
10-241	10	11	4			Sections 10.2.2, 10.2.3, 10.2.4, and 10.2.5: I think there may be potential to shorten text in thess sections. Seems too detailed for me for a readership of AR. It is also quite technical although it provides a useful overview over current methods and approaches and clearly draws the potential and limitations, and basis to judge D&A studies. At some instances it has more a character of a review than of an assessment (by the way, this is also true for some other sections across the chapter). My main point however is that the text gives the impression that in D&A studies time-series and optimal fingerprinting are the main approaches. Single/multi-step approaches seem to be a sideway. I'm wondering why the authors did not more strictly follow the D&A GPGP paper where D&A methods are described primarily under a single/multi-step approach perspective. I suggest to begin these sections referring to the GPGP paper and the single/multi-step approaches and then describe the other methods accordingly. Otherwise it looks like the GPGP has been disregarded/outdated and other approaches have become more important. In fact, much of 10.2.3 (fingerprinting) could also be described in terms of single/multi-step methods but this link is not done. I'm also making this statement as a LA working on D&A in WG2 and we are spending a lot of time working out adapted D&A methods and frameworks for the WG2 studies, trying to take the GPGP as a reference (and by the way, it looks like a multi-step approach can also be applied/adapted for D&A for human systems). So, I was rather surprised that in this chapter the GPGP methods do not seem to be a major reference. [Christian Huggel, Switzerland]	Accepted: we will shorten these sections.
10-242	10	11	8	11	8	Change "contrast it" to "contrast them" [Dian Seidel, USA]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-243	10	11	9	11	9	add following sentence before "see Section 3)": "and using an empirical orthogonal function analysis with a maximized signal-to-noise ratio (Hu and Huang 2007)," [Zeng-Zhen HU, USA]	Accepted
10-244	10	11	18	11	21	Should it be added that another downside to these methods is that they may or may not be consistent with our understanding of the physics in the system? [Gareth S Jones, UK]	Accepted
10-245	10	11	23		50	I like this dicussion of causality, it is something missing in many attempts to equate one parameter with another. Is there any way to separate the idea of A causes B from A and C are correlated and C causes B? [Larry Thomason, United States of America]	Noted, although this section must be shortened
10-246	10	11	25	11	25	“and observed” should be “an observed” [Martin Juckes, UK]	Accepted
10-247	10	11	25	11	25	misprinting: ..cause" an observed series… [Helga Nitsche, Germany]	Accepted
10-248	10	11	25	11	25	Should "and observed" be "an observed"? [Dian Seidel, USA]	Accepted
10-249	10	11	35	11	35	Consider inserting "increasing" before "lag" [Dian Seidel, USA]	Accepted
10-250	10	11	43	11	44	The fact that trends which appear significant when tested against an AR(1) noise model but not against a long range dependence model, which appears to be more consistent with the actual properties of the climate system, must surely imply a watering down of statements elsewhere in the chapter about very high likelihood of, e.g., recent warming being significant and caused mostly by greenhouse gas increases. While detection may remain robust, attribution uncertainty may greatly increase. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, although key conclusions of the chapter are not dependent on the assumption of an AR(1) model for internal variability
10-251	10	11	43	11	44	I think the chapter signs onto the Franzke (2010) a bit too readily. This is a statement that could easily be taken out of context. Yes, taking the possibility for long-memory behaviour into account widens uncertainty bands on trend estimates, and therefore will result in some situations when it will no longer be possible to reject the null hypothesis of no trend. However, making such an observation without addressing the obvious question (what does this mean, for example, for estimates of the trend in the global mean temperature anomaly) seems irresponsible. [Francis Zwiers, Canada]	We hope to assess new literature addressing this point in the SOD
10-252	10	11	45	11	47	Can this also be reversed: testing D/A results against time-series based analysis is useful to test the robustness of D/A results? [Albert Klein Tan k, Netherlands]	Noted
10-253	10	12	6	12	6	What "step"s are you talking about here? [Dian Seidel, USA]	Accepted: will clarify
10-254	10	12	8	12	8	"Specification" is a word that will easily be misunderstood (e.g., engineers provide specifications). I suggest replacing it with "estimation". [Francis Zwiers, Canada]	Noted, but estimation is used to describe the estimation of pattern magnitude. Will attempt to find another word
10-255	10	12	12	12	14	“too short to estimate the full covariance, …... retaining only …. high-variance principal components”: something is obviously wrong with this sentence: the principal components are eigenvalues of the covariance, so it must have been estimated at some stage. It might be worth distinguishing between the population covariance, which you would like to know, and the sample covariance, which you have. Because of the small sample size an estimate of the population covariance based on a truncated eigenvector expansion of the sample covariance is more robust. [Martin Juckes, UK]	Accepted: sentence will be clarified
10-256	10	12	12	12	14	This doesn't make sense to me. Doesn't this procedure create problems if most of the variance is really at the longer time scales? [Dian Seidel, USA]	Accepted: sentence will be clarified
10-257	10	12	17	12	17	Should "optimal" be removed here, because the special properties of optimal fingerprinting are presented later in the paragraph? [Dian Seidel, USA]	Accepted
10-258	10	12	17	12	19	I think an important question (which remains a matter of expert judgement) is how much detail is necessary in the fingerprints. [Francis Zwiers, Canada]	Noted
10-259	10	12	21	12	22	Saying "normalized by … internal variability" is pretty opaque. Non-optimal methods also normalize by internal	Noted: will attempt to clarify

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						variability - but dependence between elements of the observational vector is ignored, with the result that non-optimal methods can not seek the direction in the detection space that maximizes signal-to-noise ratios (i.e., there is no rotation of the signal vector). [Francis Zwiers, Canada]	
10-260	10	12	24	12	25	This is confusing. "different" from what? [Dian Seidel, USA]	Noted, will clarify
10-261	10	12	24	12	25	I would cite Huntingford et al, 2006, in this context rather than Schnur and Hasselmann. [Francis Zwiers, Canada]	Accepted
10-262	10	12	25	12	27	Doing this, one also adds structural uncertainty to the multimodel mean (can be adressed partially with eiv approach) [Laurent Terray, France]	Noted
10-263	10	12	27	12	27	suggestion to give an example for noisy climate variables [Helga Nitsche, Germany]	Noted, will clarify
10-264	10	12	27	12	28	This sentence is also confusing. Are the "noisy climate variables" the "regressors"? [Dian Seidel, USA]	Noted, will clarify
10-265	10	12	30	12	32	It is good that climate scientists have finally woken up to the advantages of Tikhonov regularization over truncation of the Empirical Orthogonal Function decomposition, but the fact that this has taken so long indicates that the climate science community as a whole is lacking in sufficient statistical expertise and is not well enough connected to relevant other scientific and mathematical communities (although some individual climate scientists may of course be exceptions). That supports my comments that statements about having very high confidence and things being very or extremely likely or unlikely are not appropriate in this chapter. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, although see 266
10-266	10	12	30	12	32	This is an overstatement in my view - the Ribes et al proposal is simply to use ridge regression adds non-observed structure to the variance-covariance matrix that is used in D&A regression analyses. The fact that the proposal has not found favour in general (see last sentence of this paragraph) suggests that others have not regarded this as an important innovation, and the assessment of the results obtained by Ribes that is provided later in the chapter is critical of the method. [Francis Zwiers, Canada]	Noted, although see 265
10-267	10	12	30	12	38	We(Ribes, Planton, Terray) will submit a paper before July 15th with ROF applied to the standard global attribution problem using several CMIP5 models [Laurent Terray, France]	Noted, excellent
10-268	10	12	30	12	57	If Ribes et al has not been used in global D&A yet, why does it merit 2+ paragraphs? [Gavin Schmidt, USA]	Noted, see 266 & 269
10-269	10	12	30	12	57	These three paragraphs seem to be mis-ordered. The first addresses Ribes et al., the second is more general, and the third goes back to Ribes et al. [Dian Seidel, USA]	Accepted: this section will be shortened, probably to 2 paragraphs
10-270	10	12	32	12	35	The best estimate of the covariance is the sample covariance – Ledoit and Wolf must have been talking about accurate estimation of the inverse, which appears to be more relevant here. I would recommend using standard terminology (sampl and population covariance), rather than, or in combination with, a long description about what would result from an infinite set of observations. [Martin Juckes, UK]	Accepted
10-271	10	12	33	12	33	delete 'that' in … covariance matrix (which would..) [Helga Nitsche, Germany]	Accepted
10-272	10	12	38	12	38	Point missing at the end of this line. [Christian-D. Schoenwiese, Germany]	Accepted
10-273	10	12	40	12	40	The next step…. -> I suggest to illustrate the chapter by a schematic workflow diagram [Helga Nitsche, Germany]	Noted, but space is limited
10-274	10	12	43	12	43	… scatter plot in Figure 10.1..? Figure 10.1 shows time series. [Helga Nitsche, Germany]	Accepted -- see 276
10-275	10	12	43			Refers to scatter plot in Fig 10.1; Fig 10.1 is a time series plot, not a scatter plot. FAQ 1 Fig 1 is also not a scatter plot. Finally found it; Box 10.1 Figure 1; Page 114, third panel. The reference to figure should clarify. [Stephen E Schwartz, USA]	Accepted
10-276	10	12	47	12	48	..model characterization.. [Helga Nitsche, Germany]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-277	10	12	54	12	57	In fact the AR(1) assumption is not a fundamental ingredient of TOD, other types of long-memory process can be used within TOD without changing the method [Laurent Terray, France]	Noted
10-278	10	12	54	12	57	I think the case has not been made that this is an advantage of methods that use climate system models to estimate internal variability that implicitly include all necessary information about dependence and long memory behaviour (at represented in climate models). [Francis Zwiers, Canada]	Noted: TOD will be assessed more critically in the SOD
10-279	10	12	56	12	56	Who are "they"? No antecedent is obvious here. [Dian Seidel, USA]	Accepted: sentence will be clarified
10-280	10	13	5	13	5	Delete "change", not necessary. [Dian Seidel, USA]	Accepted: sentence will be clarified
10-281	10	13	18	13	18	For an example of .. see …': for clearance please mention the example [Helga Nitsche, Germany]	Accepted: sentence will be clarified
10-282	10	13	25	14	8	The title mentions 'Bayesian and Frequentist Approaches and…' but Freqnentist Approaches are not explixitly addressed. [Helga Nitsche, Germany]	We will clarify that traditional approaches to attribution (e.g. fingerprinting) are frequentist
10-283	10	13	28	13	30	Many, probably most, attribution studies taking a Bayesian approach use a prior distribution that is uniform in climate sensitivity, and often also in a ocean heat uptake parameter (typically the square root of effective ocean diffusivity), over a wide range. A uniform in climate sensitivity prior, far from being "the most conservative possible approach to prior knowledge", is in fact highly informative and biases, in some cases severely, estimation of sensitivity towards high levels. Annan and Hargreaves (2011) conclude that such a prior has unacceptable properties. They point out that: "For example, the uniform prior U[0C,20C] of Frame et al. (2005) actually represents a prior belief that S is “likely” (70% probability) greater than 6◦C, with a mean value for S of 10◦C and a 50% probability of exceeding this figure." A wide uniform prior for an ocean heat uptake parameter also biases upwards the simultaneously-estimated climate sensitivity. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected: attribution studies typically assume a prior distribution that is close to linear in TCR or in observable properties (close to a Jeffreys prior)
10-284	10	13	29	13	29	Add "and" before "often" [Dian Seidel, USA]	Accepted
10-285	10	13	30	13	30	Consider changing "Tighter" to "Smaller" or "Lower" [Dian Seidel, USA]	Accepted
10-286	10	13	30	13	31	Careful here, strictly speaking the statement is false. Volcanoes can cause a net *warming* regionally and seasonally (e.g. some regions of the continents in winter, due to dynamical atmospheric circulation responses to the forcing). Need to clarify that the statement is intended to apply to e.g. global annual mean temperature. [Michael Mann, USA]	Noted: sentence will be clarified
10-287	10	13	30	13	31	The volcanic cooling example may not be the best choice here, given that volcanic eruptions cause warming in the stratosphere and warming at the surface in winter in northern midlatitudes. [Dian Seidel, USA]	Noted: sentence will be clarified
10-288	10	13	40	13	41	The material in parentheses doesn't belong in this section on methodology. [Dian Seidel, USA]	Accepted
10-289	10	13	40	13	41	Suggest replacing "stronger confidence levels" with "higher significance levels" to avoid confounding the statistical notion of confidence with the notion used in the IPCC uncertainty language, and also to characterize the result of a significance test more appropriately. Confidence levels and significance levels are often confused ... the correct thing to report is the significance level, since that is the aspect of the test that is controlled by the analyst. [Francis Zwiers, Canada]	Accepted
10-290	10	13	41	13	46	This seems to contradict because if the subjective downgrading estimates are too low, than the attribution assessment is no longer conservative. [Albert Klein Tan k, Netherlands]	The point is that deliberately excluding the experts' prior opinions on the magnitude of responses, the attribution assessment is more conservative than it would otherwise have been
10-291	10	13	46	13	50	I found this unclear. I understand why expert judgement might downweight attribution statements, but I did not follow why statement about prediction and obsevations. I presume the the reference is to the most recent decade (for attribution) and the next decade (for prediction). [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted: sentence will be clarified
10-292	10	13	52	13	52	Consider using active voice, "Trenberth (2011) proposed…" [Dian Seidel, USA]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-293	10	13	52	13	57	Lumping Trenberth (2011) and Curry (2011) together this way is unfair. The Trenberth (2011) paper makes a philosphically interesting point, whether or not the authors of this chapter agree with it. The Curry (2011) assertion, by contrast, is just confused nonsense. [Michael Mann, USA]	Noted, although space constraints may preclude delving into this
10-294	10	13	52		57	Simply saying that "proposals would represent a substantial departure from traditional practice … and are not pursued here" is not a good reason. By ignoroing this challenge to traditional approaches the whole chapter runs a risk of becoming obsolete. [Kevin Trenberth, USA]	Noted. This is a risk we are prepared to take
10-295	10	13	54	13	54	Should be "Curry (2011)" ? [Gareth S Jones, UK]	Accepted
10-296	10	13	56	13	57	Maybe it is because I am a non-native, but "pursue" something in a literature assessment seems odd, and what is the reason for sticking to "traditional practice". [Albert Klein Tan k, Netherlands]	Accepted, will revise to "assessed further here"
10-297	10	13	56	13	57	I wouldn't insist on "tradition" in this sentence. The point is that there is no literature to assess that uses these approaches. Regarding the Curry suggestion - attribution is all about the interpretation of interval esimates (confidence intervals), so I don't quite see the point about null-hypothesis testing being ill-suited. [Francis Zwiers, Canada]	Accepted, will clarify
10-298	10	14	5	14	8	Just make it explicit and write >50% then there is no ambiguity anymore [Marcel Crok, The Netherlands]	Noted
10-299	10	14	5	14	8	As per my first comment, whatever was intended by IPCC authors, third parties are quite likely to interpret "most" more strongly than "more than half of", and the word should be avoided. Criticism in Curry (2011) and Curry and Webster (2011) of the use of "most" is justified. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted
10-300	10	14	5	14	8	This might fit better page 10 line 3 as a short comment in parenthesis [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted
10-301	10	14	5	14	8	I know why this is here, but it is trivial and unnecessary. A simple statement the first time ‘most’ is used that it means ‘more than half’ is more than sufficient [Gavin Schmidt, USA]	Accepted
10-302	10	14	5	14	8	Consider removing this paragraph. It is defensive in tone and asserts something ("has always been interpreted to mean") that the authors cannot know for a fact. [Dian Seidel, USA]	Noted: we will consider removing this paragraph
10-303	10	14	12	14	16	Are these two sentences needed? They are almost redundant, so much so that they cause the reader to re-read them for clarity. Don't the section headings suffice? [Dian Seidel, USA]	Needed to conform with IPCC style.
10-304	10	14	12		16	Do paragraphs add anything? [Larry Thomason, United States of America]	Needed to conform with IPCC style.
10-305	10	14	20	14	20	In chapter 2 many potential problems were discussed with the surface T measurements. In the end however the global average T is regarded as a reliable metric for global warming and used here for attribution purposes. If there still is a warm bias, as I believe, then the agreement between models and observations is there for the wrong reasons. An obvious problem is the decrease of the Diurnal Temperature Range. Models capture only 20% of this decrease, see Walters, J. T., R. T. McNider, X. Shi, W. B Norris, and J. R. Christy (2007): Positive surface temperature feedback in the stable nocturnal boundary layer, Geophys. Res. Lett., 34, L12709, doi:10.1029/2007GL029505. In my opinion it's very likely that some of the recent warming on land is due to socio-economic factors as discussed in the work of Michaels/McKitrick (and later McKitrick/Nierenberg) and De Laat/Maurellis. So a crucial question is what part of the warming is really caused by large scale climatological processes like the enhanced greenhouse effect. [Marcel Crok, The Netherlands]	This is primarily an issue for chapter 2, which concludes that it is likley than the urban heat island effect explains less than 10% of the centennial land average warming trend and of course has no effect on sea surface temperatures. Therefore urban heat island effects do not have a major impact on attribution studies of the global temperature record but a sentences on the urban heat island effect has now been included at the end of this section.
10-306	10	14	20	14	43	This seems to repeat too much of Chapter 2 and could probably be shortened. [Dian Seidel, USA]	This has been considerably shortened to reduce overlap with chapter 2.
10-307	10	14	21	14	21	For me the main reason why I find the attribution of recent warming to CO2 still unconvincing is the fact that the model ensemble does a very poor job in the period 1910-1940. AR4 dodged this issue by writing that models and observations are in moderately good agreement in this period. This was misleading. I am glad you pay a little more attention to this early warming on page 19/20 but still this is an open issue. And as long as the models don't do a very good job in this period the supposed agreement between models and observations in	Noted. We do already discuss early century warming as noted in this comment. In most of the attribution studies we consider, anomalies are expressed relative to the whole period, thus the choice of baseline is not an issue.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the recent period could be fortuitous. The problem is also shown very clearly on the blog of Lucia Liljegren where she shows the influence of the choice of the baseline period: http://rankexploits.com/musings/2011/more-sensitivity-to-baselines/ Depending on the baseline you can get a good fit in whatever period you like, however when you consider the whole period, it's still not good: http://rankexploits.com/imageDiversion.php?uri=/musings/wp-content/uploads/2011/12/RecentBaseline.jpg Her conclusion: "But remember: I cherry picked. I (and everyone familiar with climate change data) knew that the 30-40s were on the real earth. Picking hat period tends to make the models look warmer than most other choices. In contrast, if I re-baseline using 1990-2010, the model mean and observations will agree perfectly now but the models will look inexplicably cool during the 1930-1940s." [Marcel Crok, The Netherlands]	
10-308	10	14	21	14	21	The statements concering the adjusted SST data are correct for the median estimates. However, the uncertainty ranges are important in this case because the spread of trends associated with uncertainty in the biases is relatively large. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	This material has now been removed. These issues are considered in more depth in chapter 2.
10-309	10	14	26	14	26	It is wrong to say that there is a minimum warming in the SO. There is actually a cooling in the SO since 1970, as shown in Fig. 2.8 in 2.2.3. It is also not cinsistent with the writing on line 25 on page 10-16. [Zhaomin Wang, UK]	We now characterise this as a 'temperature trend' rather than 'warming'. Whether the trend is positive or negative depends on the region of the Southern Ocean considered and the dataset.
10-310	10	14	27	14	28	"Correction of residual instrumental biases ... causes a warming of global mean SST": actually the correction do not have "caused" the warming, but have eliminated the error that - in the past - altered that warming. [Claudio Cassardo, Italy]	This text has now been deleted.
10-311	10	14	27	14	31	The correction to the global mean SST record is large and must have implications for the results of most of the detection and attribution studies. That should be flagged. Statements of very high (or even high) confidence and things being extremely likely (or even very likely) should be revisited and amended where they depend to any significant extent on the accuracy of the (uncorrected) global mean temperature record over the period affected by these corrections. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The text assesses a range of observational datasets including HadCRUT3, NCDC, NASA GISS, JMA, HadCRUT4 which all make a variety of assumptions about correcting for biases. Thus literature assessed in AR5 has made a much more complete exploration of the effects of such observational uncertainty than the literature available to AR4.
10-312	10	14	28	14	28	Change "causes a warming" to something like "removes an artificial cooling". The current language is particularly inappropriate in this chapter on detection and attribution. [Dian Seidel, USA]	This text has now been deleted.
10-313	10	14	29	14	30	"reducing the best estimate of the warming trend over the latter half of the 20th century": add (because the SST of the '50 decade is higher than in the previous estimate), or something similar. [Claudio Cassardo, Italy]	This text has now been deleted.
10-314	10	14	31	14	33	Why not mention the innovations in the other global temperature datasets too? Or better even, refer to Ch2 for a more complete assessment and avoid highlighting one dataset only. [Albert Klein Tan k, Netherlands]	Agreed. We have deleted this paragraph and retained the reference to the relevant section of chapter 2 in the previous paragraph.
10-315	10	14	35	14	36	Move "global mean temperature" to the end of the sentence. [Dian Seidel, USA]	This text has now been deleted.
10-316	10	14	36	14	36	The GISS temperature product is GISTEMP, not GISS. Many places have this error. [Gavin Schmidt, USA]	This has been corrected.
10-317	10	14	37	14	38	There are many hypothesis currently around in the literature with new ones added almost every week. One of the latest is Davies, R., and M. Molloy, 2012. Global cloud height fluctuations measured by MISR on Terra from 2000 to 2010. Geophysical Research Letters, 39, L03701, doi:10.1029/2011GL050506. [Marcel Crok, The Netherlands]	Davies and Molloy (2012) describe a decrease in cloud height, but they do not argue that this has driven the muted temperature trend over the past decade.
10-318	10	14	37	14	38	The claim that there has been "some reduction of warming over the past decade" is peculiar, since trends are not evaluated over a decadal timeframe. Foster and Rahmstorf (2011) [Foster & Rahmstorf (2011), Global Temperature Evolution 1979-2002, Environ. Res. Lett. 6 044022] make a compelling argument that the rate of warming has in fact been constant when factors that lead to short-term variations in temperature are accounted for. [Michael Mann, USA]	This text has now been deleted. Foster and Rahmsdorf (2011) is now cited in the section on temperature trends over the past decade.
10-319	10	14	41	14	41	Revise "over the past decade (Hansen …" to "over the past decade due to enhanced atmospehric and oceanic heat transport into Arctic Ocean (Hansen et al., 2010; Zhang et al., 2008; Polyakov et al., 2005)".	This text has now been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Xiangdong Zhang, United States of America]	
10-320	10	14	41	14	43	"although": the following sentence is not in contrast with the previous one; on the contrary, is a confirmation of the same concept: more Arctic stations with a large warming rate give a better estimate of the global warming rate. Due to the small extension of the Arctic region with respect to the world surface, it is obvious that the change in the global warming rate is small. [Claudio Cassardo, Italy]	This text has now been deleted.
10-321	10	14	42			I question the statement: "While caution should be exercised in interpreting agreement between simulated and observed global mean temperature changes, since there is evidence that part of this agreement might arise from conditioning the model ensemble using historical observations of climate change (Huybers, 2010; Knutti, 2008), any possible model tuning is expected to have very little effect on estimates of future warming constrained using a regression of spatio-temporal patterns of observed climate change onto simulated patterns of historical changes." At best such a statement would be valid if the spatial distribution of composition change and forcing were to be maintained in the future as it has in the past. If the mix of aerosols vs LLGHGs changes in the future (aerosols not increasing in proportion to LLGHGs) a model that matches the twentieth century by virtue of wrong sensitivity compensated by an erroneous forcing will not accurately represent the future climate situation. This issue needs to be squarely addressed. [Stephen E Schwartz, USA]	Studies which apply observational constraints to projections generally separately fit the GHG and aerosol responses to obs. Thus these studies allow for a different mix of aerosol and GHG forcing in the future compared to in the past. Note the text has been shortened at this point also to avoid overlap with Section 10.2 (see response to comment 10-332).
10-322	10	14	47	14	52	It should be stated, assuming so, that all or some of the model results did not include forcing changes from observation-consistent variations in stratospheric water vapour concentrations, which are likely to be natural and account for a substantial part of the surface warming over the last two decades of the twentieth century, probably enhancing it by about 30% during the 1990s. (Solomon et al, 2010, Contributions of Stratospheric Water Vapor to Decadal Changes in the Rate of Global Warming, Science, 327, p1219). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The forcings considered in the model simulations are natural forcings external to the climate system (volcanic and solar forcing), rather than internal aspects of the climate system. This is now clarified by referring to 'natural external forcings'.
10-323	10	14	48	14	52	Once more the "natural"; forcings do not include the most important influences on the record. They are the various ocean oscillations and the various urban and land use changes that have talen place. If these are included the two graphs are almost identical [VINCENT GRAY, NEW ZEALAND]	The natural forcings considered here are natural forcings external to the climate system. This has now been clarified on first use in the chapter. Land use change is considered as an anthropogenic forcing where it is included in models. Modes of variability such as ENSO are simulated internally in many climate models, and are included as aspects of internal variability. Thus all these factors are considered in studies assessed here.
10-324	10	14	49			Refers to "temperature"; should read "temperature anomaly"; see discussion to this point in my comment on the figure page 115. [Stephen E Schwartz, USA]	This has been ammended to refer to 'temperature anomaly'.
10-325	10	14	53	14	55	It is claimed that showing anomalies is reasonable since while models exhibit different biases in their means, climate sensitivity is not a strong function of their mean state. But the spread in model mean temperatures is so large - about 3 C for 47 CMIP3 models, equating to a difference in radiation emitted at the surface of a black body of 16 W/m^2 (Tredger, On the evaluation of uncertainties in climate models, 2009, p55. Phd thesis, London School of Economics.) This is so large as to call into severe question the reliability of climate models. See also "The impact of initial conditions in climate modelling, Tredger, Smith and Stainfortth, 2007, Phil Trans Roy Soc A., which concludes that "the severity of model inadequacy suggests a more qualitative interpretation than one might wish". [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The assessment of model biases and discussion of their implications is primarily an issue for chapter 9. In the SOD, chapter 9 includes additional analysis showing global mean temperature in the CMIP5 simulations, and the relationship to their climate sensitivities. However, we do consider model uncertainty here, basing our assessment on studies using a range of GCMs with a range of biases, and also considering studies which explicitly account for model uncertainty.
10-326	10	14	53	14	55	This statement is seemingly inconsistent with statement in chapter 9: "First, since the effective climate sensitivity depends on the state of the climate system, it is necessary for climate models to reproduce the observed state as accurately as possible to minimize the effects of state-related errors on projections of future climate (Senior and Mitchell, 2000)." (Chapter 9, p20, lines 34-36). The respective authors should resolve any inconsistencies. [Chris Roberts, Uk]	Thanks to the reviewer for pointing out this inconsistency. Additional material is included in the chapter 9 SOD to explicitly address this point, comparing climate sensitivity against mean temperature across the CMIP5 ensemble. There is no

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							significant relationship between the variables, supporting our existing statement in chapter 10.
10-327	10	14	53	14	56	This sentence is awkward and should be re-crafted. [Dian Seidel, USA]	This statement has been shortened and re-phrased.
10-328	10	14	53		55	Again, I urge that temperature be shown, not just anomaly. Showing only anomaly gives a misleading picture of concordance among models. The word "reasonable" suggests an uncomfortableness with showing temperatures; show the temperatures first and then argue why these are not reasonable in this context. [Stephen E Schwartz, USA]	A plot in chapter 9 |SoD includes information on the mean temperatures in the CMIP5 simulations (as well as anomalies), and a further figure has been added to Chapter 9 showing the relationship between mean bias and climate sensitivty. These figures are referenced here.
10-329	10	15	2	15	2	Change "between" to "among". [Dian Seidel, USA]	Changed.
10-330	10	15	8	15	8	It shoud be mentioned that neural network (NN) studies based on observations only can be interpreted as some type of cross-validation of the results presented in Fig. 10.1. In such a study based on the HadCRUT3 data set 1860-2008 GHG and aerosol forcings account for 73% of the observed temperature variance. The related signals are GHG +(0.9-1.5) K, aerosols -(0.2-0.5) K, volcanic - 0.2 K, solar +(0.1-0.2) K and ENSO + 0.2 K. Reference: C.-D. Schönwiese, A. Walter, and S. Brinckmann, 2010: Statistical assessments of anthropogenic and natural global climate forcing. An update. Meteorol. Z., 19, 3-10. [Christian-D. Schoenwiese, Germany]	This study is now cited in the section on temperature evolution over the past decade.
10-331	10	15	16	15	16	…, thin grey lines for CMIP3 (in figure 10.1): I can hardly see the thin grey lines (quality of the figure) [Helga Nitsche, Germany]	The figure has been revised to make these more visible.
10-332	10	15	19	13	56	Some of this repeats material in Section 10.2 on evaluation of methodologies(page 8 line 51 to page 9 line3. It would follow more logically there, and enable some shortening of the chapter [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Some shortening of the text has been carried out here to avoid overlap with section 10.2
10-333	10	15	19	15	22	Schwartz et al 2007 didn't say quite that.Our point was that the models had not systematically examined the consequences and implications of uncertainty in forcing, leading to a gross underestimation of the uncertainty in the increase in GMST from the model suite. [Henning Rodhe, Sweden]	The reference to Schwartz et al. has been removed from this sentence.
10-334	10	15	19		22	Schwartz et al 07 did NOT question attribution, as is incorrectly stated here. That paper argued that the uncertainty shown in the Figure was erroneously small because it did not take into account the convolution of uncertainty in forcing with uncertainty in climate sensitivity, as correctly stated line 32. Schwartz et al 07 suggested that a possible reason for this was "that the forcings used in the model runs were anticorrelated with the sensitivities of the models; that is, models with high sensitivities used low forcings and vice versa." This is subsequently shown to be correct (for a different set of models; Kiehl, GRL 07). The same issue seems to recur in the current draft report and must be assessed. [Stephen E Schwartz, USA]	The reference to Schwartz et al. has been removed from this sentence.
10-335	10	15	22	15	24	I think this is misleading.Figure SPM-4 in AR4 was indeed a very powerful tool (the most powerful) for convincing policy makers and others that the 20th century warming can only be explained as the result of anthropogenic forcing. [Henning Rodhe, Sweden]	Now deleted.
10-336	10	15	26	15	26	Change "earlier generation climate models which" to read "earlier-generation climate models, which" [Xuemei Wang, China]	Done.
10-337	10	15	31	15	31	the simple constraints' did not include, for instance, radiative balance, and so the Stainforth et al conclusions are not as dramatically different as they have been portrayed [Gavin Schmidt, USA]	Reference to Stainforth et al. now deleted.
10-338	10	15	33	15	36	Are there any references to the process of model development that show the inpracticalities of such model tuning? [Gareth S Jones, UK]	Model tuning is dealt with in more detail in Box 9.1 which we reference here.
10-339	10	15	34	15	36	I don't think people would have tuned to details of the record, but they probably would have dealt with issues such as a model that warmed too quickly, or too slowly, overall. This seems to be an endorsement of the criticism, so can you nuance this a bit by talking about what aspects of the observational record you have in mind. [Francis Zwiers, Canada]	This text has now been modified in view of the lack of significant correlation between aerosol forcing and TCR across the CMIP5 ensemble.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-340	10	15	36	15	36	“implicitly constrained” – this needs a fuller discussion, as it does undermine some of the assumptions implicit in the use of observations and the distance between models and observations to estimate uncertainty. [Martin Juckes, UK]	Text now deleted to reflect the lack of significant correlation between TCR and aerosol forcing found in CMIP5 models.
10-341	10	15	36	15	46	Some simpler language along the lines used in the Hegerl et al comment on the Curry and Webster paper would be helpful here. Please add "However, detection and attribution methods determine whether model-simulated temporal and spatial patterns of change (referred to as ‘fingerprints’) that are expected in response to changes in external forcing are present in observations. and the attribution of the dominant role of greenhouse gases in the warming of the past half-century is not sensitive to the uncertainties in the magnitude of aerosol forcing, or of other forcings, such as solar forcing." [Susan Solomon, USA]	This discussion of Curry and Webster has now been considerably condensed in view of the standardized aerosol precursor emissions prescribed in CMIP5 and lack of significant correlation between aerosol forcing and TCR across the CMIP5 ensemble.
10-342	10	15	38	15	44	There really is circular reasoning. You first assume that models do a good job in simulating internal variability. However there is no way to test this. Then you claim that models can only explain the recent warming with CO2. Also there are many many parametrisations in the models for which real world data are used. So in principle you cannot prove anything by getting a good agreement between the model and the observations. Tuning the model with known data is fine but then the validation should come from the forecasting of new data. So far this isn't going well as Lucia Liljegren has shown on her blog. I think she is right that the AR4 models did a forecasting starting from 2001. An example is given here: http://rankexploits.com/musings/2011/noaa-february-0-4042c-up-from-0-3790c/ I encourage IPCC to provide similar analyses in AR5. So for the CMIP5 results, what data were already known to the modellers and what data is therefore forecasted? [Marcel Crok, The Netherlands]	Text now deleted. Model tuning is dealt with in Box 9.1 which we reference. Models are typically tuned to reproduce the observed climatology, not rates of change.
10-343	10	15	38	15	56	The CW2011 critique is not worth devoting space to rebutting here. Simply discuss (folllowing on from the previous paragraph) the issue of how aerosol forcings in the forward models are obtained. [Gavin Schmidt, USA]	Suggested change made.
10-344	10	15	39	15	39	Should "using" be "used"? [Dian Seidel, USA]	Text now deleted.
10-345	10	15	40	15	56	"any possible model tuning is expected to have very little effect on estimates of future warming": I think this concept should be deepened as is one of the most important answers for the skeptics [Claudio Cassardo, Italy]	Text here has been deleted to avoid overlap with section 10.2 and to ensure meet length limitations.
10-346	10	15	42	15	42	Suggest replacing "not an input" with something like "not an input to the preceding modelling exercise". [Francis Zwiers, Canada]	Text now deleted.
10-347	10	15	44	14	54	These two sentences are both very long and hard to follow. [Dian Seidel, USA]	Text here has been deleted
10-348	10	15	50	15	54	A team of experts with much experience of climate modelling disagree with the confident statement here about observational constraints going beyond the global mean temperature providing "a means to test a model's ability to represent the response to greenhouse gas forcing". Chris Forest, Peter Stone and Andrei Sokolov wrote in 2008 about GCMs: "Much of the work has focused on evaluating the models’ ability to simulate the annual mean state, the seasonal cycle, and the inter-annual variability of the climate system, since good data is available for evaluating these aspects of the climate system. However good simulations of these aspects do not guarantee a good prediction. For example, Stainforth et al. (2005) have shown that many different combinations of uncertain model sub-grid scale parameters can lead to good simulations of global mean surface temperature, but do not lead to a robust result for the model’s climate sensitivity." (Constraining Climate Model Parameters from Observed 20th century Changes. Tellus A, 2008, 60A, 911-920). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	We agree with the reviewer that observations are less effective constraints on climate sensitibvity than on the transient climate response. This issue is discussed in section 10.9.3.
10-349	10	16	1	16	2	" top left panel of Figure 10.2 … based on the HadCRUT3, NASA GISS and NCDC datasets": in the 10.2 figure caption, it is written "for the HadCRUT3 dataset". Which dataset is used in the figure? [Claudio Cassardo, Italy]	It was HadCRUT3 in the FOD and is HadCRUT4 in the SOD. The text has been revised to reflect this.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-350	10	16	1	16	2	The figure does not show NASA GISS or NCDC datasets. [Gareth S Jones, UK]	Correct. The text has been revised.
10-351	10	16	2			If I am correct, the HadCRUT3 map is shown only. [Albert Klein Tan k, Netherlands]	Correct. The text has been revised.
10-352	10	16	2			"NASA GISS and NCDC datasets" are not in Figure 10.2 [Omer L. Sen, Turkey]	Correct. The text has been revised.
10-353	10	16	3	16	5	As explained above and also in my comments on Chapter 2 there are many other anthropogenic reasons why trends on land are larger. This should be mentioned here as well to alert the reader. IPCC assumes in this chapter that in the end the global average temperature is a reliable metric but that is a doubtful hypothesis. [Marcel Crok, The Netherlands]	Issues of observational bias are assessed in chapter 2. Chapter 2 concludes that the Urban Heat Island effect likely caused less than 10% of observed global mean land surface warming. This is also now reported in our summary of the observations 10.3.1.1.1.
10-354	10	16	40			Consider adding a key directly to the figure. Why not sample the NASA/GISS and NOAA/NCDC data with the same mask as used for HadCRUT3 and the models? [Dian Seidel, USA]	The new version of the figure in the SOD includes a key, and all observational datasets are masked with the HadCRUT4 data mask.
10-355	10	16	46	16	51	Should note the difficulty of evaluating variablility on multi-decadal timescales from observations [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Such a caveat has been added.
10-356	10	16	46	16	51	It would be nice if there were similar figures for CMIP5 (e.g., in Chapter 9) that could be cited in this chapter. [Francis Zwiers, Canada]	Chapter 9 will include such a figure in the SOD, and we will reference it here.
10-357	10	16	55	17	2	Why repeat this here? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Agreed. This has been deleted.
10-358	10	16	58	16	58	Delete "fortuituous" because when errors cancel one can easily be misled, so it's not a fortuitous situation. [Dian Seidel, USA]	Deleted.
10-359	10	16				I am concerned that, even though AR5 quotes and shows research results indicating that transient climate sensitivity (TCS) may be lower than models show, AR5 does not say this explicitly. Instead it summarizes results saying there is nothing to change previous conclusions. One of the important capabilities needed is prediction of future decadal trends. If, it as seems possible, TCS is lower than models predict, there could be continued over-prediction of temperature rise leading to loss of confidence in models. [Charles Keller, USA]	The detection and attribution studies cited here are based on observations (models are used to derive the spatio-temporal pattern of responses only). To first order errors in the TCR in models will not impact detection results. The issues mentioned here are discussed in section 10.9 which concerns observationally constrained estimates of TCR and ECS and projections.
10-360	10	17	7	17	42	The lead sentences of each of these paragraphs are not informative. Try to rewrite stating the main finding of the paragraph. [Dian Seidel, USA]	Text has been revised and shortened here.
10-361	10	17	8			EOF? [Larry Thomason, United States of America]	This text has now been deleted.
10-362	10	17	16	17	17	Why use these slightly different time periods rather than the same ones? [Dian Seidel, USA]	Hegerl et al. used the period to 1999 because the CMIP3 simulations ended then. We use the period up to 2010 in order to make the trends as up-to-date as possible, given that many CMIP5 simulations are extended to the present.
10-363	10	17	29	17	40	I wonder if the common EOF basis is adequate to treat the combined variability across models well. Are the signals really being represented well? A worry is that there doesn't seem to be much association between the the sign of the best estimate OA scaling factor and the sign of the corresponding attributed trends. [Francis Zwiers, Canada]	The best estimate OA scaling factors and attributable trends are related in the way one would expect - they are of opposite sign. In general the fraction of variance explained when the response patterns are projected on the common EOF basis is at least as large as when projected on individual model EOFs.
10-364	10	17	33	17	33	I may be mistaken, but I don't think COWL is discussed in Chapter 2 or elsewhere in this chapter, so why include it here? [Dian Seidel, USA]	It is mentioned here because it was one of the signals removed from the global mean in the Fyfe et al. (2010) study which we discuss here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-365	10	17	36	17	40	Please rephrase: "As the observational record gets longer, it should become increasingly easy to identify discrepancies….. However, it should be noted that although the absolute forcing is not key to attribution, a strong understanding of the temporal changes in forcing is important, particularly changes in the rates of change of aerosol forcing." [Susan Solomon, USA]	We have included a reference to understanding of temporal changes in forcing as being important.
10-366	10	17	45	17	49	When looking carefully at Fig 10.4d it seems that observational uncertainty is less that that due to internal variability [Laurent Terray, France]	We agree that it appears slightly less, but think 'comparable' is reasonable.
10-367	10	17	49	17	52	Very high confidence for >50% attribution of post 1950 warming to greenhouse gases cannot be justified in view of the uncertainties resulting from model inadequacy. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Model uncertainty is accounted for in our assessment.In addition physical arguments are advanced.
10-368	10	17	49	17	52	I can understand differences in likelihood assessments, but why the difference in confidence levels? Both inferences seem to be drawn from the same body of evidence. [Francis Zwiers, Canada]	This sentence has been deleted.
10-369	10	17		21		AR5 shows that ocean multi-decadal cycles seem to have had a "significant" contribution to warming in the 1990s (as well as the Solomon et al (2011) finding that increased water vapor in the stratosphere in the 1990s also contributed to warming). An inspection of AR5's Fig. 10.20 shows two sets of PDF peaks—TCS and ECS. The TCS one shows clearly lower than 3°, which would seem to mean models are over predicting AGHG warming. ECS shows similar PDF peaks at lower than 3°C but the Gray bar (most likely ECS seems to be strongly affected by long tails on the high temp side of the probability distributions) indicates most likely ECS at higher temperatures. Also the plot "Combination of evidence" shows no peak as high as 3° and the gray bar looks similarly skewed. At the least, some discussion should be given about the perhaps too-large influence of the long, high temperature PDF tails when the peaks seem to agree on lower temperature sensitivity. [Charles Keller, USA]	Rejected, this text is not relevant to ECS and TCS. That comes later in section 10.7
10-370	10	17		21		As to projections of warming in the next few decades, the transient climate sensitivity TCS, is an important consideration. Again the complication of AMO/PDO cycles in the past 150 years needs to be discussed in a more quantitative way since it points to lower TCS. The current draft needs to be more quantitative instead of using, as it does, the qualitative terms, "significant" and "substantial" where the latter indicates less effect than the former. Also the summary statement that AGHG forcing has accounted for more than half (i.e. perhaps only 51%) of recent warming can lead to a variety of conclusions such as—more than 1/3 of recent warming is natural, and up to 49% of warming in 20th Century was natural. [Charles Keller, USA]	The AMO and implications for detection are discussed in detail already. Revisions have been made so that the word 'significant' has only be used in the SOD where the meaning is 'statistically significant'. We agree that the statement that GHGs have accounted for most of the observed warming does leave open the possibility that other influences have accounted for up to 50%. As shown in the figures, the best estimate is however that GHGs have accounted for more warming than has actually been observed.
10-371	10	17		21		Thus AR5 mentions considerable evidence that AMO is likely to have had a significant influence on global temperatures in the past 100 years, but then doesn't "drop the other shoe" and say this evidence would seem to call for a reduction in TCS below 3°C. I recommend that these sections and the general conclusions on attribution be amended to at least say there is a growing body of evidence suggesting that TCS could very well be lower than AR4 suggested and that models need to study why the over-predict warming. [Charles Keller, USA]	Discusion of TCR/TCS is in FOD section 10.8.
10-372	10	18	8			Note that there is no JMA dataset in Ch2 (yet?) [Albert Klein Tan k, Netherlands]	Noted.
10-373	10	18	10	18	12	Can you be quantitative about the relative impacts of black carbon and GHG? Is this attribution statement for the globe or Northern Hemisphere? [Dian Seidel, USA]	The analysis is based on global temperature - this is now stated. Jones et al. find different contributions to warming from BC depending on the details of their analysis. Given this and the fact that there is only one study, we prefer to keep the assessment that the BC-attributable warming is small compared to the GHG-attributable warming, rather than giving exact details of the Jones et al. findings.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-374	10	18	23	18	30	Should note that these statistical techniques are often limited in how they can distinguish between variability driven by internal climate variability or by external forcings. [Gareth S Jones, UK]	We do not wish to be dismissive of these techniques in the chapter, but give these results less weight in the assessment for such reasons.
10-375	10	18	26	19	14	AR5 does cite work that indicates the influence of AMO cycles on temperatures in the past century such as (Wu et al, 2011), which estimates as much as 1/3 of warming in the past 25 yrs may be due to AMO and several authors showing that warming is spatially different suggesting partial natural cause. In addition AR5 cites evidence that models underestimate the amplitude of these observed multi-decadal variations. Curiously, the AR5 draft goes on to suggest that these discrepancies may be due to complications from anthropogenic aerosols, but these do not explain AMO influence prior to 1940 and are a strange explanation of AMO in general. Yet it is these preliminary aerosol findings that dominate the AR5 draft summary. [Charles Keller, USA]	There is now literature questioning the Booth et al. results which we cite.
10-376	10	18	26	19	14	Supporting references: I. Grossman and P. Klotzbach, A review of North Atlantic modes of natural variability and their driving mechanisms, JGR, 114, 2009, D24107 M. F. Knudsen, M. Seidenkrantz, B.H. Jacobsen & A. Kuijpers, Tracking the Atlantic Multidecadal Oscillation through the last 8,000 years, Nature Communications, 2011, [2:178]DOI: 10.1039/ncomms1186, 2011 T.R. Knutson, T.L. Delworth, K.W. Dixon, I.M. Held, J. Lu, V. Rmamswamy, and M.D. Schwarzkopf, Assessment of Twentiety-Century Regional Surface Temperature Trends Using GFDL CM2 Coupled Models, J.of Climate, 19, 2006, 1624-1651 S. Kravtsov and C. Smannagle, Multi-decadal climate variability in observed and modeled surface temperatures, J. Climate, 19, 2007 [Charles Keller, USA]	No response required.
10-377	10	18	26			This AMO discussion could be improved. 1) it would help to define AMO. 2) is there agreement that this is a stationary signal, and what is the timescale? I get the impression that some studies attribute whatever is multidecadal to the AMO, but if we allow for too much flexibility then anything can be explained by this. 3) mention that our observed record is short compared to the AMO timescale, and this it is difficult to draw clear conclusions from only two cycles. 4) the GHG plus sulphate plus natural forcing results in a similar temperature to GHG plus AMO, so the signals at least globally are partly degenerate. 5) Climate model simulations reproduce the observed warming without a significant AMO contribution. This does not disprove the existence, but it's worth noting that it's not necessary for the models to be consistent with observations. 6) Some of these methods that are discussed (e.g. EMD) by construction try to decompose the signal into a sum of cycles, so there is an obvious danger to detect cycles in a timeseries even if there are none, and this is particularly dangerous with cycles half as long as the timeseries. Such methods are not designed to separate cycles from an underlying forced trend that may have a complicated shape by itself. [Reto Knutti, Switzerland]	In revision the AMO has been defined. Statement made about length of it and difficulties in attributing to AMO. Reference now made to degeneracy issue for timeseries. Issues of confounding AMO with forcings discussed. Assessment statement made in summary.
10-378	10	18	27	18	30	If you give the percent variance explained by multi-decadal variabilty, it would probably make sense to do so for the other two modes mentioned as well. [Dian Seidel, USA]	This text has now been deleted.
10-379	10	18	32	18	32	change "identify" to "identified" [Xuemei Wang, China]	This text has now been deleted.
10-380	10	18	32	19	14	This section is almost incomprehensible and, I think, assumes an expert knowledge of this field. It should be made less turgid and more open to the non-specialist or at least significantly shortened. The key point I get out of it is the sentence on 19, 3-4; is this correct? The understandability of this section is in marked contrast to the rest of the Chapter. [Larry Thomason, United States of America]	This section has been shortened, and hopefully made clearer with an assessment section at the end on AMO.
10-381	10	18	32		47	I have trouble following the first half of this paragraph. [Larry Thomason, United States of America]	This has been shortened and clarified.
10-382	10	18	33	18	33	Suggest inserting "a measure of" ahead of "multi-decadal predictability" (it's the metric that is maximized). [Francis Zwiers, Canada]	This text has now been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-383	10	18	38	18	38	Swanson (2009) shows with a mathematical analysis of four modes of natural oscillations that these together 'caused' several climate shifts in the past century. The shifts took place in 1910, 1940, 1970 and 2001. Their work raises questions about the validity of the currently accepted paradigm that that positive forcing of greenhouse gases started to dominate the negative forcings of aerosols in the 70-ies. [Marcel Crok, The Netherlands]	This study is reviewed, together with other more recent work which suggests that parts of this record may indeed be forced by aerosol changes.
10-384	10	18	38			AMO? - Atlantic multidecadal oscillation? [Larry Thomason, United States of America]	Correct - now defined.
10-385	10	18	48	19	14	This discussion of the possible inconsistency between observed and simulation variability of temperature at multi-decadal timescales is helpful but incomplete. The AR4 D&A chapter concluded that the decedal variability of global mean temp simulated by models was conisstent with observations. These studies mentioned in these paras and Curry and Webster (2011) state that conclusion was wrong. It would be good to have a clearer statement in this para on whether the simulated variability is underestimated in the North Atlantic, associated with teh AMO, and whether this leads to an uncerestimated of multi-decadal variability in global mean temeprature. If the AR4 conclusion was wrong, you should say so. If it was correct, you should say so, and explain more clearly why the different conclusions from different types of analysis can all be correct. [David Karoly, Australia]	The discussion here has been heavily revised. Reference is made in the chapter to the spectra of variability shown in Chapter 9.
10-386	10	18	52	19	3	Could these three results be condensed to a single pithy statement? [Dian Seidel, USA]	Yes - this has been done.
10-387	10	18	56	18	58	I don't know the Swanson paper, but Is this a like for like evaluation? Sample eigenspectra are biased, with low order eigenvalues overestimated and high order eigenvalues underestimates. The estimated eigenspectrum flattens as sample size grows (i.e., bias decreases). One might suspect that an analysis of models would include multiple ensemble members, and thus have much larger samples for eigen analysis than can be obtained from observations. So are we seeing that effect here? [Francis Zwiers, Canada]	Swanson et al. (2009) is not based on an EOF analysis.
10-388	10	18				Somewhere in the context of other methods the study by Huber and Knutti (2011 Nature Geoscience) should be mentioned. It quantifies contributions to the observed warming from different forcings based on changes in the energy balance. Given that 90% of the energy is taken by the ocean, it is important to test whether the attribution of surface warming is conistent with the ocean warming and our knowledge about the radiative forcing magnitudes. [Reto Knutti, Switzerland]	Huber and Knutti (2011) is now cited and briefly discussed as supporting evidence for the attributable warming ranges derived.
10-389	10	19	3	19	14	Not sure to follow the resasoning here: it seems that Booth main finding is that the aerosol forcing might have been underestimated in CMIP3 models (but this needs to be confirmed with other models I guess) . It does not say much on the over or under estimation of internal variability in the HadGEM2-ES model for instance where int.var. could be underestimated and the forced part overestimated [Laurent Terray, France]	Booth et al. suggest that variability previously thought to be internal may in fact be forced. However other studies now dispute this. The discussion on AMO variability now reflects this.
10-390	10	19	7	19	7	I don't quite understand this - unless demonstrated otherwise, wouldn't you consider the AMO be part of the climate's internal variability. [Francis Zwiers, Canada]	Text has been revised to state clearly as assessment that AMO is largely internal variability although citing Booth et al.who demonstrate otherwise, at least in HadGEM2ES but we caveat these results more heavily now, however, since they are based on a single model and this has been disputed.
10-391	10	19	8	19	11	The summary should make clear that while these recent AMO related studies find detection of external influence on global temperatures is not compromised, most of them find that model uncertainties are very substantially understated, and must cast doubt on the accuracy of attribution of global temperature changes to external influences. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	An assessment is made of the role of AMO variability based on the literature which shows that AMO is not a major contributor to global warming since 1950.
10-392	10	19	10	19	10	I wonder if the Booth et al paper is being given a bit to much weight in the chapter given that it is mentioned several times. If you do give it a lot of weight, then I think the chapter should discuss the mechanisms that might link aerosol changes to Atlantic SST variability. [Francis Zwiers, Canada]	The Booth et al. findings have now been more heavily caveated, since we are relying on a single study here. We cite the response by Zhang et al and the analysis of Chiang et al.
10-393	10	19	11			Chang et al, 2011, present a multi-model case (CMIP3) pointing to models with better complexity capturing more of the observed variability in Inter-hemispheric contrast (a related mode of Atlantic variability). It might also be worth considering whether shorter satellite timescale data might be informative. For example Evan,	Chang et al. discuss changes in the tropical interhemispheric temperature gradient which they say are distinct from the AMO. Evan et al. is now cited.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2009 illustrate that aerosol (mineral) and volcanic forcings may explain much of the post 1980 North Atlantic tropical temperature trend. [Ben Booth, UK]	Chiang 2012 et al also cited.
10-394	10	19	13	19	14	Consider changing "results remain to be confirmed.." to something like "CMIP5 simulations should be examined to see if this result is robust" which does not prejudge the outcome of the research. [Dian Seidel, USA]	Section edited and this statement no longer appears.
10-395	10	19	16	19	36	Is it worth noting here the Lockwood papers in Proceedings of the Royal Society A-Mathematical Physical and Engineering Sciences (2007, 2008a+b) also show decreasing solar activity over last 30 odd years with temperatures still increasing? [Gareth S Jones, UK]	These studies are now cited.
10-396	10	19	16	19	36	Please make clear in this paragraph that the debate is over what solar forcing may be from 1750 to present or perhaps 1900 to 1950, but that observations show that there is no increase in solar energy hitting the Earth since 1978. This is key to the reason why attribution here is for the past half century, and that should also be stated. [Susan Solomon, USA]	A sentence contrasting warming with declining irradiance over the past 25 years has now been inserted.
10-397	10	19	18	19	20	here is an example of a selective combining of solar with volcanic forcing and a selective look at solar variation with the 1950 to 1999 time period. It ignores the fact that like greenhouse gasses, solar can be combined with other forcings, and also ignores the climate commitment studies that show that solar and other forcing variations occuring BEFORE 1950 may be contributing to the warming after 1950 and that the specifics of the temperature variation after 1950 don't match CO2 variation after 1950 well either. [Martin Lewitt, United States of America]	No - the attribution study cited accounts for the effects of solar irradiance variations prior to 1950 on temperature changes post-1950. The response comes from climate models which simulate this effect. All major forcings are accounted for in this analysis, so the combination of the solar response with other responses is accounted for.
10-398	10	19	18	19	20	Again this may be compared with neural network (NN) studies, see above. [Christian-D. Schoenwiese, Germany]	Schônwiese et al. is cited in the section on temperature trends in the past decade.
10-399	10	19	19	19	19	"0.1K temperature trend" is unclear. Either report a trend (K/time) or a temperature change (K). [Dian Seidel, USA]	trend' replaced with 'change'.
10-400	10	19	20	19	36	This whole paragraph could be replaced with a couple of sentences discussing why single-factor curve fits are useless for attribution since they assume the result they are supposed to be proving. [Gavin Schmidt, USA]	We prefer to take a more open-minded stance here but we have abbreviated the discussion somewhat and drawn out the problems weith such an analysis.
10-401	10	19	20		26	The two-sentence discussion of Scafetta & West (2007) stops with Bebestad's criticism of the paper, and omits Scafetta's response, which identified errors in Benestad's analysis. See http://pielkeclimatesci.wordpress.com/2009/08/03/nicola-scafetta-comments-on-solar-trends-and-global-warming-by-benestad-and-schmidt/ as well as N. Scafetta, "Empirical analysis of the solar contribution to global mean air surface temperature change," Journal of Atmospheric and Solar-Terrestrial Physics 71 1916–1923 (2009), doi:10.1016/j.jastp.2009.07.007. http://www.fel.duke.edu/~scafetta/pdf/ATP2998.pdf [David Burton, USA]	We assess only published or submitted articles here. Scafetta (2009) does not refer to Benestad and Schmidt (2009).
10-402	10	19	23	19	23	"This result is contested by Benestad and Schmidt (2009)": actually, this result has been debunked by Benestad and Schmidt (2009)… [Claudio Cassardo, Italy]	We prefer to take a more open-minded stance but text has been revised to reflect the issue.
10-403	10	19	27	19	27	Should be "Scafetta" [Gareth S Jones, UK]	Now deleted.
10-404	10	19	27	19	27	It's not Scaggetta, but Scafetta, but in either case, why is this paper even mentioned? If you need to cite every random curve fitting exercise, the chapter would be far too long and far less informative. [Gavin Schmidt, USA]	This sentence has now been deleted as suggested.
10-405	10	19	27	19	31	This sentence is odd, as it introduces solar cycles of 20 and 60 yr, not the usual 11 or 22 yr periods, and makes no assessment of this new work. Also, how does a linear trend "post 1942" (about 70 years) gibe with a 60 yr periodicity? [Dian Seidel, USA]	Now deleted.
10-406	10	19	27			Scaggetta -> Scafetta. Note that in some refs Scafetta is spelled with ff instead of f [Reto Knutti, Switzerland]	Now deleted.
10-407	10	19	31	19	34	See above. [Christian-D. Schoenwiese, Germany]	See previous response.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-408	10	19	32	19	32	Suggest revising so that there are fewer contributions :). Why not just say "while contributing a small cooling over the…"? [Francis Zwiers, Canada]	Suggested change made.
10-409	10	19	34	19	34	The findings in Shaviv "Using the Oceans as a Calorimeter to Quantify the Solar Radiative Forcing (2009, GRL) are highly relevant to this paragraph and should be cited. Shaviv found that the total radiative forcing associated with solar cycles variations is about 5 to 7 times larger than just those associated with the TSI variations, thus implying the necessary existence of an amplification mechanism. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	This study concerns the solar effect on ocean heat content not surface temperature.
10-410	10	19	38	19	38	An interesting paper about this period is Crook, J. A., and P. M. Forster (2011), A balance between radiative forcing and climate feedback in the modeled 20th century temperature response, J. Geophys. Res., 116, D17108, doi:10.1029/2011JD015924. They write: "The simulated trend is too low, particularly in the tropics, even allowing for internal variability, suggesting there is too little positive forcing or too much negative forcing in the models at this time." [Marcel Crok, The Netherlands]	Crook and Forster (2011) is now cited.
10-411	10	19	38			Our knowledge of the radiative forcing over this period (with limitations of course for solar for example) also gives some constraints on the relative contributions to the warming from different causes, as shown by Huber and Knutti (Nature Geoscience, 2011). [Reto Knutti, Switzerland]	Huber and Knutti do not appear to discuss the causes of the early 20th century warming.
10-412	10	19	39	19	39	Define "recent" [Dian Seidel, USA]	This has now been deleted.
10-413	10	19	40	19	40	"from about 1920 and": actually, from about 1910. [Claudio Cassardo, Italy]	This has now been replaced with 'during the first half of the 20th century'.
10-414	10	19	43	19	43	What are you referring to by "The assessment"? The AR4, or the sentence prior? [Dian Seidel, USA]	Text now deleted.
10-415	10	19	43	19	43	Is it appropriate to call this a forced contribution? As noted, we don't know the cause of the observed change in stratospheric water vapour - feedbacks associated with internal variability are, presumably, a potential explanation. [Francis Zwiers, Canada]	[Refers to page 20, line 43] This section has now been re-written so that the Solomon et al. study is discussed in the following paragraph and not directly after the reference to a 'forced contribution'.
10-416	10	19	43	19	48	This recapitulation of the basis for the AR4 is not necessary. [Dian Seidel, USA]	Deleted.
10-417	10	19	46		52	If Solomon's recent Nature paper is correct that there can be significant climate impacts from modest volcanic events (ones that could easily be missing from the historical record prior to the satellite era), is there a possibility that episodic volcanic events influence the record prior to the late 1950s but can't be accounted for due to missing measurements? [Larry Thomason, United States of America]	We're not aware of any literature available to assess on this topic.
10-418	10	19	48	19	48	Is spelling of "Kamchutka" correct? Normally see "Kamchatka". [Gareth S Jones, UK]	Now deleted.
10-419	10	19	48	19	48	Santa Maria (1902 in Guatemala) and Novarupta/Katmai (1912 in Alaska) are often given as the largest eruptions in the early 20th century (Robock 2000). Is the 1912 Caribbean one quoted a mistake? [Gareth S Jones, UK]	Now deleted.
10-420	10	19	48	19	48	I am not sure that the Shindell and Faluvegi, 2009 reference is appropriate here. Does it associate early century warming with the lack of volcanism? [Gareth S Jones, UK]	Now deleted.
10-421	10	19	53	19	53	change "find" to "found" [Xuemei Wang, China]	We think the current wording is OK.
10-422	10	19	54	19	57	Adjustments have been developed; see Chapter 2 for discussion of HadSST3 (Kennedy et al. 2011). The character of the 20th century variability is somewhat modified but uncertainties surrounding the second world war period remain large. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. The text describing this has been revised.
10-423	10	19	55	19	55	change "temperature data has been found that affected" to "temperature data affected" [Xuemei Wang, China]	Text now deleted.
10-424	10	19	56	19	56	“may’? You have already mentioned HadSST3 and HadCRUT4, so it should be apparent whether the situation	Revised to be more concrete.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						has changed or not. [Gavin Schmidt, USA]	
10-425	10	19	56	19	57	What is the basis for this conjecture? Thompson et al. (2008)? [Dian Seidel, USA]	Text now deleted, and replaced with summary reflecting Morice et al. (2012).
10-426	10	20	1	20	1	"followed by" sounds like a temporal description, but I think it's meant to be a ranking. Clarify. [Dian Seidel, USA]	Clarified.
10-427	10	20	6	20	7	If citing Schlesinger & Ramankutty (1994), cite concurrent study reaching similar conclusion by Mann and Park (1994) [Mann, M.E., Park, J., Global scale modes of surface temperature variability on interannual to century time scales, Journal of Geophysical Research, 99, 25819-25833, 1994]. [Michael Mann, USA]	Our focus is on literature which appeared since the AR4. We cite Schlesinger and Ramankutty as the first study describing the AMO. Mann and Park and other studies appeared later.
10-428	10	20	7	20	7	"random expression of internal variability" is sort of redundant [Dian Seidel, USA]	We accept that there is redudancy here, but think it makes the meaning clearer.
10-429	10	20	10	20	11	Mann and Emanuel (2006) [Mann, M.E., Emanuel, K.A., Atlantic Hurricane Trends linked to Climate Change, Eos, 87, 24, 233-241, 2006] argued the same thing earlier. Should be acknowledged here. [Michael Mann, USA]	Cited.
10-430	10	20	10	20	13	The "very likely" assessment seems a bit inconsistent with the preceding sentence. [Dian Seidel, USA]	No - this rests mainly on the previously discussed detection and attriubtion studies.
10-431	10	20	11			citation of Booth et al, 2012 refers only to aerosols. Is this ambiguous? This paper links it to volcanic activity and anthropogenic aerosols, which in some sense are both aerosols. Suggest changing wording to "However, recent research (Booth et al.,11 2011) has indicated that much of the variability in North Atlantic SST may be externally forced (volcanic and anthropogenic aerosols)". [Ben Booth, UK]	tropospheric and stratospheric' inserted.
10-432	10	20	16	21	11	Doesn't the absence of warming over the past decade imply that the observed temperature increase is LESS than the forced amount and natural variability has acted to lower the estimated warming. The role of La Nina and the missing energy is described in Trenberth, K. E. and J. T. Fasullo, 2011: Tracking Earth's energy: From El Niño to global warming. Surveys in Geophysics, Special Issue, doi: 10.1007/s10712-011-9150-2. available from my website: http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/ISSI_fulltext.pdf. [Kevin Trenberth, USA]	This is one of the two hypotheses discussed. Trenberth and Fasullo (2009) was previously cited. Its conclusions are now more clearly stated.
10-433	10	20	16	21	35	A key question is posed at the start of the section on "The evolution of global temperature over the past decade": is the recent apparent slowdown in the rate of observed global warming consistent with internal variability superposed on a steady anthropogenic warming trend, or has it has been driven by changes in radiative forcing. No answer is given to the question. The literature is summarised, but not synthesised. The end of the opening paragraph reads as if internal variability is the preferred explanation, but the difficulty is that a 10-year trend like that which has been observed is "not inconsistent" with a wide range of explanations and is not therefore decisively in favour of one over another. It is clearly not inconsistent with any of the other explanations given in the remainder of the section. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	A summary of the section has been written at the end of this section.
10-434	10	20	16	21	35	A key question is posed at the start of the section on "The evolution of global temperature over the past decade": is the recent apparent slowdown in the rate of observed global warming consistent with internal variability superposed on a steady anthropogenic warming trend, or has it has been driven by changes in radiative forcing. The question posed does not exhaust the possibilities. The popular phrasing of the question, when considering the apparent slowdown in warming, is "has global warming stopped?" or "are models wrong?" The latter question ought also to be dealt with. What, if anything, has the past decade to say about the reliability of models? That might be as simple as forwarding discussion to the chapter on model evaluation. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	The question of whether observed trends over the past decade are consistent with simulated trends is already dealt with. We cite and discuss several studies which show that observed trends are consistent with simulated trends over this period.
10-435	10	20	16			Two recent papers are relevant in this context: Foster and Rahmstorf, ERL 2011, who show that the trend is strongly positive if ENSO, solar and aersols are removed, and Loeb et al. Nature Geoscience 2012 who argue that the ocean has indeed warmed over the past decade, in contrast to earlier claims. [Reto Knutti, Switzerland]	Both papers are now cited.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-436	10	20	17	20	17	"have not increased strongly": I think it could be said better "have slowed their increase rate" [Claudio Cassardo, Italy]	We prefer the original wording.
10-437	10	20	17	20	18	"a time when the multi-model mean temperature continued to increase" is not great language. Consider "a period over which the multi-model mean simulated temperature increased" [Dian Seidel, USA]	Suggested change made.
10-438	10	20	17	20	19	Does this sentence imply we know what the aerosols forcing was doing to a high degree of confidence over that period. Is this supported by the assessments elsewhere in this report? [Gareth S Jones, UK]	Yes - see Figure 8.19.
10-439	10	20	17	20	19	This sentence is ambiguous and can be misleading. Is temperature the surface air temperature? When is "the past decade"? [Toshihiko Takemura, Japan]	surface' inserted before 'temperatures'. I think it is clear that the past decade is the most recent 10 years. The exact period considered varies from study to study.
10-440	10	20	17	21	35	This section is useful but reads a bit too much like a review. Can you try to assess in the end the relative contributions of different factors including natural variability and possible sampling issues in observations to the recent temperature variations? Regarding stratospheric aerosols, it is now accepted that Hofmann et al (2009) are wrong in attributing the recent increase to Chinese coal (refer to Vernier et al GRL 2011 paper for instance). I would not overplay the role of stratospheric aerosols (less than 0.1 K in my opinion as this is relative to no aerosols at all if I remember correctly) or SH sea-salt (only one paper, not reproduced in most models even with a resolved stratosphere and an O3 forcing). [Olivier Boucher, France]	This section has been condensed and more assessment of the results has been added. The reference to a Chinese source for stratospheric aerosol has been removed, and Vernier et al. (2011) is now cited. The conclusions on SH sea salt have been moderated.
10-441	10	20	19	20	19	I believe this remark about constant or declining aerosol forcing is inconsistent with the sea level chapter's view. Please harmonize. [Susan Solomon, USA]	This agrees with the radiative forcing chapter - see figure 8.19. We have discussed this issue with chapter 14 and 8 in order to ensure that our assessments in the SOD are consistent.
10-442	10	20	19	20	41	Please add a sentence regarding the recent paper by Santer et al., JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 116, D22105, 19 PP., 2011 doi:10.1029/2011JD016263 [Susan Solomon, USA]	This section is on surface temperature trends, but Santer et al. describes free atmosphere trends.
10-443	10	20	26	10	26	change "2 years and longer" to "2 to 10 years" [Randall Dole, United States of America]	Suggested change made.
10-444	10	20	27	20	32	Should add that the Knight et al. (2009) assessment is based on observed and simulated temperatures with an estimate of the ENSO contribution to the global mean removed, which is important in light of the ENSO contribution to the warming trend mentioned a couple of sentences later. Suggest re-order this so mention ENSO contribution first then that even when estimate of ENSO variability is removed, simulated trends still consistent with observed. [Gareth S Jones, UK]	ENSO removal now mentioned at the beginning of the sentence introducing Knight et al.
10-445	10	20	28	20	30	This sentence should go at/near the beginning of the section where you discuss observations [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Suggested change made.
10-446	10	20	36	20	36	It is unlikely in my opinion that the heat has gone down to the deep ocean. This should have been visible in the ARGO data and as far as I know it has not. [Marcel Crok, The Netherlands]	ARGO only samples down to 2000m.
10-447	10	20	38	20	40	The claim made here regarding global temperatures during the past 10 years is directly contested by Foster and Rahmstorf (2011) [Foster & Rahmstorf (2011), Global Temperature Evolution 1979-2002, Environ. Res. Lett. 6 044022], who make a compelling argument that the rate of warming has in fact been constant when factors that lead to short-term variations in temperature are accounted for. Furthermore, the authors don't rectify this statement with the conclusions of Easterling and Wehner (2009) noted earlier in the paragraph. [Michael Mann, USA]	This discussion has been revised.
10-448	10	20	39			Personally, I don't see the supporting evidence in the text of this paragraph for the strong statement "very likely" [Albert Klein Tan k, Netherlands]	We cite a number of studies supporting this conclusion.
10-449	10	20	43	20	48	While this statement is true, the same is also true of Solomon's statement that increasing stratospheric water vapor in the 1990s could have accounted for 1/3 of the observed warming. Nowhere in this section is this warming mentioned?? [Charles Keller, USA]	We focus on the cause of the temperature trend over the past decade in detail because it has received so much attention in the detail. On longer timeseries we

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							mainly discuss the causes in terms of responses to external forcings.
10-450	10	20	43	20	48	The same group has also proposed that stratospheric aerosols from volcanos might have influenced surface temperatures in the past decade, and that work should probably be assessed here too. Reference: S. Solomon, J. S. Daniel, R. R. Neely III, J. P. Vernier, E. G. Dutton, L. W. Thomason,The Persistently Variable “Background” Stratospheric Aerosol Layer and Global Climate Change. Science DOI: 10.1126/science.1206027 [Dian Seidel, USA]	This paper is already cited and discussed.
10-451	10	20	43	20	48	The material on possible stratospheric aerosol effects on recent surface temperature changes, now covered on page 21, could be moved here to good effect. [Dian Seidel, USA]	This section has been reordered so that the stratospheric aerosol and stratospheric water vapour mechanisms are discussed together.
10-452	10	20	43		48	The Solomon et al study (lines 43-48) is useful but the effects of changes in stratospheric water vapor are included in the CERES measurments at TOA and this is NOT a separate explanation. It does not describe the changes in TOA radiation. [Kevin Trenberth, USA]	These studies are not now discussed next to each other.
10-453	10	20	50	20	53	The Lean and Rind 2009 study only covers periods post 1980. But the Lean and Rind GRL 2008 doi:10.1029/2008GL034864 covers the whole of the 20th century. Should look at that as well. This is important as it shows how well the method works for other periods other than after 1980. [Gareth S Jones, UK]	This study is assessed and cited in the section on the causes of warming since 1900.
10-454	10	20	50	20	53	Again see above. [Christian-D. Schoenwiese, Germany]	See previous response.
10-455	10	20	50	21	11	Benestad and Schmidt, 2009 and Stott and Jones 2009 studies suggest there can be some limitations with these regression approaches - as demonstrated when applied to perfect model cases. This should be noted. [Gareth S Jones, UK]	The regression-based approaches now receive less attention in the text and are summarised more briefly, therefore we don't think it is necessary to go into a discussion of the advantages and disadvantages of the method here. This would be more appropriate for 10.2.
10-456	10	20	50	21	11	The Lean et al paper overfits the data and thus overestimates the solar contribution. There is a need for an asessment here. [Kevin Trenberth, USA]	These results are now given less precedence in the discussion.
10-457	10	20	50	35	10	Gerry North and Petr Chelyk (Third Santa Fe Climate Conference, Nov. 2011) have looked for both the 20 and 60 yr. cycles in ice core records. Gerry didn't find the 60 yr one, but Chelyk's work corroborated Knudsen et al (2011) showing that both these cycles are not continuous, but come and go over the centuries. So it's likely that the past two cycles of 60 years are present and important. When Chris Follen (Fig. 10.5, 2011) deconvolved recent temps, in addition to other factors, he got a substantial signal roughly similar to AMO which contributed to warming in the 1990s and subsequent lack thereof after 2005. Other authors (Knudson,2011, Frankcome et al, 2010, Kravtsov & Spannagle, 2007,Grossman & Klotzbach, 2009) get similar results. Curiously, while the AR5 draft shows the Follen results in Fig. 10.5, it doesn’t comment on them, nor on some of these other results. [Charles Keller, USA]	We do cite Folland et al. in this section. Knudsen et al. (2011) appear to show increasing North Atlantic temperatures through to the end of their record, and thus no explanation for a reduced rate of warming. Frankcombe et al. do not show the AMO index for most of the past decade, and it appears to increase until the end of their period of record. Kravtsov & Spannagle (2008) only derive an AMO-proxy up until 2000. Grossman and Klotzbach (2009) is not listed on BIDS WoK or on Google Scholar.
10-458	10	20	50			This might be an appropriate place to cite Foster et al. (2011) on estimating recent warming after eliminating ENSO, solar, and volcanic signals. [Marcus Sarofim, USA]	This paper is now cited.
10-459	10	20	53	20	55	The 0.1C value for a solar cycle response in near surface global mean temperatures is not actually supported by any references in Hegerl et al. (2007b). To avoid such "facts" becoming "recieved wisdom" suggest finding another source to support the "consistent with" statement. [Gareth S Jones, UK]	Text now deleted.
10-460	10	20	53	20	55	It should also be noted that climate model simulations (not based on time series regressions) often give solar cycle min-max temperature responses much smaller than 0.1C e.g. Wigley and Raper , GRL, 1990, Stevens and North, JAS, 1996, Foukal et al., Science, 2004, Cahalan et al., GRL, 2010 and Jones et al., JGR, 2012 (accepted) [Gareth S Jones, UK]	The text on the observed solar cycle amplitude has been deleted.
10-461	10	20	53	23	53	Consider changing "extended" to "prolonged" [Dian Seidel, USA]	This tex has now been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-462	10	20	54	20	54	Add "SAT" before "response" for clarity. [Dian Seidel, USA]	Text now deleted.
10-463	10	20	55	20	55	Comparison of 0.1 with 0.16 isn't really fair, since the number of significant figures differs. 0.1 may have been 0.149 and 0.16 may have been 0.156, with a much smaller difference than 0.6. [Dian Seidel, USA]	Text now deleted.
10-464	10	21	20	21	23	Kaufmann (2011) also seems to suggest a cooling contribution from solar after 1960 and a warming from volcanoes ~5 years after the 1991 Pinatubo eruption - Fig 10.5. Are these figures correct? [Gareth S Jones, UK]	The apparent warming following Pinatubo appears to reflect the fact that there was especially little volcanism in this period. The forcing timeseries used here has some volcanic forcing in the past 5 years, and it does not decrease to zero between eruptions prior to Pinatubo.
10-465	10	21	20		35	so much speculation [Tim Barnett, USA]	Our paragraph here assesses the literature on this topic. We now include an overall assessment at the end of this section. It is not clear if this is a criticism of the literature on this topic, or of our assessment of it.
10-466	10	21	22			Typo "estimated" [Albert Klein Tan k, Netherlands]	Text now deleted.
10-467	10	21	23		25	While the source of the increase is the main thrust of this sentence, I think Hofmann's attribution is discredited at this point (at least seriously called into doubt). Though the measurement community sees the increase as real, the source at this time seems morely likely to be volcanic than human-derived. I also believe that the data set on which this was based is about to be substantially revised which colors his conclusions even more. Including the paper the following paper may be appropriate: J.-P. Vernier, et al. (2011), Major influence of tropical volcanic eruptions on the stratospheric aerosol layer during the last decade, Geophys. Res. Lett., 38, L12807, doi:10.1029/2011GL047563. [Larry Thomason, United States of America]	We have now deleted the reference to China as the source for the increase in stratospheric aerosol, and we cite Vernier et al. (2011).
10-468	10	21	26	21	30	This needs re-phrasing. The -0.07C (not -0.1C as quoted) cooling is relative to a simulation with reduced to zero stratospheric aerosol. Much of this relative cooling is due to the stratospheric aerosol being at a background level, which many other model simulations assume. The additional aerosol suggested by Solomon et al. 2011 will cause an additional relative cooling but not as much as -0.07C (let alone -0.1C). [Gareth S Jones, UK]	The cooling has been corrected to 0.07C. CanESM2, for example, does assume zero stratospheric aerosol over this period.
10-469	10	21	30	21	35	This seems highly speculative and incomplete - a shift and increase in the mid-latitude jet is just as likely to affect ocean currents, upwelling, oceanic mixing and hence change surface temperature. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Korhonen et al. (2010) simulated this effect using an aerosol model driven by renalaysis winds. This is now made clear in the text.
10-470	10	21	34	21	34	I think here 'in any models' should replace 'in most models'. As I know, no models have included the process studied in Korhonen et al. (2010). If there are some models that included this effect, I would like to know which models and how they included this effect. [Zhaomin Wang, UK]	This text has been changed, and the phrase has been replaced with 'though this effect has not been reproduced in other models'. Sea salt aerosols are included in at least one CMIP5 model (CanESM2).
10-471	10	21	39	21	39	Consider changing "single" to "limited" or "smaller". The globe is a single region, but that's not the intent of the sentence. [Dian Seidel, USA]	Suggested change made.
10-472	10	21	43	21	43	Add "spatial differences in" before "internal variations" for clarity [Dian Seidel, USA]	Suggested change made.
10-473	10	21	48	21	49	"not generally underestimated" is a double negative and should be avoided. Does "overestimated" do the trick? [Dian Seidel, USA]	No - overestimated is not what we want to say, and is not the same. 'Not underestimated' means consistent or overestimated.
10-474	10	21	53	22	3	As previously stated, the highest quality study shows that there has been no significant warming in Antarctica as a whole over the last 50 or so years, prior to which there is very little instrumental data available. O'Donnell, Lewis, McIntyre and Connon (2011): Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al (2009) Antarctic Temperature Reconstruction, J.Climate, vol.24, p2099-2115, DOI: 10.1175/2010JCLI3656.1 (of which I am one author). That study showed that the significant continental warming found by Steig et al. (2009) over 1957-2006 was an artefact of faulty mathematical methodology, and	Steig et al. is not discussed here, so there is no need to assess this critique of their approach. Gillett et al. (2009) used station data - it was not spatially complete, but is suitable for an attribution analysis if the model is co-sampled with the obs.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						that with corrected methodology there was no significant warming over that period. By comparison, the Gillett study used a low quality estimate of Antarctic temperature changes. There is accordingly little evidence for anthropogenic influence on temperatures having been detected in Antarctica. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-475	10	21	54	21	54	While it's nice that Gillett et al. "were able to separately detect" it suffices to say they "detected separate". [Dian Seidel, USA]	Suggested change made.
10-476	10	21	55	21	56	Well yes, because there is a strong bias in observed locations toward the bit that is warming strongly. [Dáithí Stone, United States of America]	Not really the case. Only two of the fifteen grid cells averaged over were on the Antarctic Peninsula.
10-477	10	21	56	21	56	I suggest to give the time period after 'over the observed period'. 'observed period' is very vague, what kind of observed period (satellite or surface station observation?), what kind of spatial and temporal coverages? It is important to give the period, because during the recent decades, there was a cooling (see comments 9 and 67). [Zhaomin Wang, UK]	The period is now given.
10-478	10	21	58	22	1	This is jargony language. [Dian Seidel, USA]	Revised.
10-479	10	22	1	22	8	Estimates of the warming of Antarctica are extremely uncertain, and recent work may suggest that the data used in Gillett was overly optimistic in terms of data quality issues. Please work with the observations chapter to ensure that you assess (not review, but assess) what we really can say both about the (putative) warming averaged over the continent, and about attribution of it. [Susan Solomon, USA]	NEED TO UPDATE THIS ONCE WE HAVE RESPONSE FROM CHAPTER 2 ! Antarctic SAT trends are not currently assessed in chapter 2, but may be assessed in the SOD. We will update our assessment accordingly. No reference is given in the review comment to the recent work highlighting data quality issues in the CRUTEM3 data.
10-480	10	22	15	22	17	The Jones et al. (2008) study was not a "multi-variable" study. The only variable examined for the optimal detection was JJA near surface temperatures [Gareth S Jones, UK]	Corrected.
10-481	10	22	22			What does CNA stand for? [Omer L. Sen, Turkey]	The acronym is now spelt out.
10-482	10	22	23	22	26	"Overall we conclude ... (medium confidence)": the medium confidence refers to the whole sentence (thus, both to Antarctica and every continent except Antarctica) or only to Antarctica? [Claudio Cassardo, Italy]	The medium confidence referred to Antarctica only. This ambiguity has now been removed in the revised text.
10-483	10	22	28	22	46	This paragraph should probably lead with a sentence noting that the choices of regions analyzed to date is quirky and spotty, in a rather unsatisfactory way. [Dian Seidel, USA]	There are some studies that look systematically at sub continental scale region that are cited.
10-484	10	22	42	22	46	If the estimate of variability is poor, why mention it? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	TRopospheric and stratospheric' inserted.
10-485	10	22	42	22	46	For this regional land application, it seems that the AR(1) is actually reasonable when using CMIP3 models. [Laurent Terray, France]	We have removed the sentence criticizing the use of an AR(1) model.
10-486	10	22	42		46	I think this is a more balanced assessment of Ribes than the assessment given in the discussion of methodological innovations earlier in the chapter. [Francis Zwiers, Canada]	Noted.
10-487	10	22	43	22	44	Likewise, the estimates of uncertainty using internal variability from climate models may be too low. How is this taken into account? [Albert Klein Tan k, Netherlands]	The reviewer does not provide evidence in support of this statement. Many of the studies we assess first validate simulated variability against observations.
10-488	10	22	51	22	54	Is this result subject to the circularity criticism of C&W? [Francis Zwiers, Canada]	This criticism of the findings is now included in the text.
10-489	10	22	54	22	57	The previous sentences are about detecting warming. This sentence is about detecting amplification of warming. The next sentence is a summary sentence on detecing warming which should logically come before this statemen and make it easier to undertand. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	This sentence about Arctic amplification has now been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-490	10	22	54	22	57	I've re-read this a few time and still can't quite understand the intention. What is very uncertain? The amplification, or attribution of the amplification? [Dian Seidel, USA]	The sentence about Arctic amplification has now been deleted.
10-491	10	22	57	23	3	The word "despite" has some odd connotations (e.g., belief in the face of evidence to the contrary - which I think is not the message that is intended or appropriate). I suggest finding another way to put this. Perhaps "While various uncertainties associated with … (give the list), there is a sufficiently strong body of literature to (give the assessment)". [Francis Zwiers, Canada]	We have partially rephrased this sentence, though we retain 'despite'. We think the meaning here is clear.
10-492	10	22	57			incroduced -> introduced [Larry Thomason, United States of America]	Corrected.
10-493	10	23	2	23	3	It is also awkward to combine the assessment "likely" with the word "significant". The interpretation of the latter is not clear - but one possible interpretation might be statistical significance. If that is what is meant, then the word likely suggests that findings of statistically significant changes are uncertain. But given a particular dataset and a specific estimation and inference procedure, and change is either significant or not. A suggestion for wording might be "it is likely that been a substantial (or non-negligable) anthropogenic warming ..." [Francis Zwiers, Canada]	significant' has been deleted here.
10-494	10	23	5	23	15	Might be worth noting the CET includes corrections for urban warming [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	This is now mentioned.
10-495	10	23	11	23	15	True, but has this been done in the study described in the earlier part of this paragraph? [Albert Klein Tan k, Netherlands]	This paragraph has been reordered to make the sense clearer. We did not mean to imply that Karoly and Stott missed something in their analysis.
10-496	10	23	11	23	15	What is the purpose of this admonition? Did Karoly and Stott omit something in their analysis? [Dian Seidel, USA]	This paragraph has been reordered to make the sense clearer. We did not mean to imply that Karoly and Stott missed something in their analysis.
10-497	10	23	11		15	This sentence isn't well connected to the preceeding sentences in this paragraph. [Larry Thomason, United States of America]	This paragraph has been reordered to make the sense clearer. We did not mean to imply that Karoly and Stott missed something in their analysis.
10-498	10	23	21	23	21	This paper Anagnostopoulos, G. G., Koutsoyiannis, D., Christofides, A., Efstratiadis, A. & Mamassis, N. (2010) A comparison of local and aggregated climate model outputs with observed data. Hydrol. Sci. J. 55(7), 1094–1110 shows that forced simulations also do a very poor job at both grid cell and continental (US) scale. "We compare the output of various climate models to temperature and precipitation observations at 55 points around the globe.We also spatially aggregate model output and observations over the contiguous USA using data from 70 stations, and we perform comparison at several temporal scales, including a climatic (30-year) scale. Besides confirming the findings of a previous assessment study that model projections at point scale are poor, results show that the spatially integrated projections are also poor." [Marcel Crok, The Netherlands]	The study mentioned here considers only 26 stations and compares their output with six relatively course resolution climate models. Althoug correlation coefficients between models and observations are calculated, no clear hypothesis is presented or tested - for example that differences bewteen models and observations are larger than woudl be expected based on internal variability. For this reason we do not assess it here.
10-499	10	23	25	23	25	What number would one expect by chance? [Dian Seidel, USA]	The confidence level of the test is now stated (10%). This is the fraction of grid cells which would be expected to show apparently significant differences by chance, under the null hypothesis of no anthropogenic response.
10-500	10	23	37	26	43	Since most of this section is not true detection/attribution work, should it be in Chapter 9 rather than here? [Melissa Free, USA]	Aggreed. The section now focusses more on D&A work.
10-501	10	23	37		41	This paragraph is disjointed giving the guide for the section, a very specific finding from a paper, and a summary of AR4 conclusions. [Larry Thomason, United States of America]	Hegerl et al 2007b is the Chepter 9 reference of AR4. This is clarified.
10-502	10	23	52	23	33	The end of the last sentence should be reworded; at present its grammatical construction implies that observed trends are inconsistent with simulated internal variability and the actual, not simulated, response to natural forcings. Either delete "the" before "response to natural forcings" or (better) insert "simulated" before "response to natural forcings". [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The world "simluated" is included. Thanks.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-503	10	24	1	24	18	The text says observations and models show agreement "on multi-decadal time scales for the radiosonde record from 1958 to 2003 (Thorne et al., 2011)." This does not reflect the findings of Thorne et al. (2011), who reported no agreement above 300 mb and only uncertain agreement below that. Also they did not allow for the step change at 1977. McKitrick and Vogelsang (2011) use proper controls for autocorrelation and show that the 1958-2010 radiosonde record shows a significant discrepancy with models even without allowing for the break at the time of the Pacific Climate Shift, but with the break included the discrepancy is significant at <0.0001%. See reference in cell 55 [Ross McKitrick, Canada]	Noted. Text is revised and focussed now on the D&A work
10-504	10	24	1	24	18	You say "Temperature trends at specific tropospheric levels as well as vertical amplification rates are also non-distinguishable between models and observations when studying the 1979-1999-time period and uncertainties are considered (Santer et al., 2008)." This is incorrect. McKitrick, McIntyre and Herman (2010) (see ref in cell 53 and Corrigendum ref in 54) showed that the models overpredict warming over the 1979-1999 interval and the difference is marginally significant. The MMH method takes complete account of autocorrelation and cross-panel covariance. [Ross McKitrick, Canada]	Noted. Text is revised and focussed now on the D&A wor
10-505	10	24	1	24	18	Having raised the problem of the missing warming in the tropical troposphere you can't just punt it to Chapter 9 with the quote "The current understanding on the consistency between observed and simulated tropical troposphere temperature trends is assessed in Section 9.4.1.2 where it is concluded: ‘While there are discrepancies between modeled and observed temperature trends in the upper tropical troposphere, observational uncertainty and contradictory analyses prevent a conclusive assessment of model fidelity.’" First of all, in light of the extensive problems with that section the quoted text will will have to change. But more importantly, your task as CLA's and LA's is to assess the literature yourselves. You can see as well as the Ch 9 authors that multiple teams have found a model-obs discrepancy, and the only study that claimed no such discrepancy exists used a primitive method on a limited (1979-1999) interval that ends in a giant El Nino, and whose findings were subsequently overturned by a study using more reliable estimation methods. On the other hand, if you accept the grounds on which the Ch 9 authors declined to draw a conclusion--namely that there are methodological judgments involved and at least one paper contradicting the consensus can be found--then you ought to apply the same criterion to all the issues in Ch 10 and decline to draw any conclusions at all. Otherwise you need to acknowledge that the evidence for a significant discrepancy between models and observations in the tropical troposphere is at least as strong as the signal detection evidence. [Ross McKitrick, Canada]	Text will be modified after chapter 9 has revised its section. A difference between the CMIP3 model-averaging warming rate and the observed warming rate is now noted explicitly.
10-506	10	24	5	24	18	This paragraph is misleading, with several of the studies cited relating to datasets that are now outdated 8-10 years. The two published studies that use the full thirty year period for which satellite AMSU data on tropospheric temperatures is available (Fu and McKitrick) both show that CMIP3 climate model projections are inconsistent with observations, McKitrick et al 2010 showing that this was so at a very high statistical significance level. An honest conclusion would be that model fidelity is very poor for the relation of tropical tropospheric to surface temperatures , an aspect of climate modelling that provides a good test of the ability of model physics to represent the actual climate system. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	We agree with the author that focus should be on recent literature based on longest possible data. This text is removed and we focus on assessment of D&A results as suggested by comment 501.
10-507	10	24	11	24	12	Christy et al. 2010 Rem Sens. Performed the most thorough examination of the amplification factor for the tropics. Klotzbach et al.2009,2010 did so for the globe as a whole, and land and ocean separately. We were well ahead of Fu who did not even perform the type of data quality analysis done by us. [John Christy, USA]	This sentences is removed and the section focussed now on D&A results.
10-508	10	24	16			The text quoted is actually in section 9.4.1.3 rather than 9.4.1.2. [Melissa Free, USA]	Thank you. Corrected.
10-509	10	24	26	24	24	However, the model ensemble trend since 1979 is considerably higher than the obseved trend, see http://rankexploits.com/musings/2011/april-uah-up-from-march/ [Marcel Crok, The Netherlands]	An analysis of the period 1979 to present based on CMIP5 model will be added when the paper is submitted.
10-510	10	24	43	24	43	When this gets rewritten when more CMIP5 data is available, care needs to be taken to separate out the interactive ozone simulations, the prescribed ozone simulations, and the no-change-in-ozone runs. There is some evidence that the prescribed ‘strat’ and ‘trop’ fields of ozone change are neater than what occurs in the interactive models, and that impacts of strat ozone depletion can be seen in upper trop ozone and, in particular, southern hemisphere ozone. Making the overall ozone impact in temperatures more complex than	We agree with the concerns of the reviewer. However, this study focusses only on the four models and they all have ozone prescribed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						previously allowed for. And since this has a different spatial pattern, assuming a simple pattern scaling approach may have difficulties. [Gavin Schmidt, USA]	
10-511	10	24	47			Is Figure 10.6 legitimate for use in AR5? Is there appropriate literature citation? The citation is to Jones et al. (2003), but I don't think any of the observational or model datasets shown were available in 2003. Certainly trends for 1958-2010 were not reported in that paper. Including this may be a stretch in terms of its literature provenance. Also, why are some but not all adjusted upper-air temperature datasets included? Suggest including RATPAC if the figure is retained. [Dian Seidel, USA]	We understand the concerns of the reviewer. A more appropriate reference will be in place for the SOD. The revised figure will also include RATPAC data.
10-512	10	24	52	24	54	Is this redefining D&A for time series? Note that D&A on time series does not need to rely on trends alone, and in fact is probably more reliable if it doesn't do so (signal don't necessarily evolve as simple linear trends, and differences in the temporal evolution of signals may allow signal separation - e.g., volcanic from anthropogenic). [Francis Zwiers, Canada]	Santer et al. (2011) is based on trends - this is now clarified in the text.
10-513	10	24	56	25	2	This requires some subtle re-phrasing. The Santer et al 2011 study actually says "On timescales longer than 17 years, the average trends ... consistently exceed 95% of the unforced trends in the CMIP-3 control runs ... clearly indicating that the observed multi-decadal warming of the lower troposphere is too large to be explained by model estimates of natural internal variability". i.e. need 17 years to detect a change, before attempting to attribute that change to anthropogenic influences. [Gareth S Jones, UK]	We agree with the reviewer. Text amended.
10-514	10	24				Figure 10.6- I found it hard to see the black lines- need to b thicker, or a different colour [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	We agree with the reviewer, Figure will be revised.
10-515	10	25	1	25	1	What size of effect? The power to detect depends upon the length of record, the amplitude of the signal relative to internal variability, and the analyst's willingness to accept the possibility of type I errors (rejection of the null when the null is true). The latter is controlled through the significance level - operating at a lower significance level (e.g., 10% rather than 5%) affords a bit more power. [Francis Zwiers, Canada]	We added a confidence level (5%), and now state the end of the period concerned.
10-516	10	25	5	25	7	In science generally a finding of a signal at a confidence level of only 90% would not be regarded as a finding at all. I suggest a check for consistency in this chapter (and elsewhere in WG1) as to whether findings with a confidence level of between 90% and 95% constitute evidence for or against a null hypothesis. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Paragraph deleted.
10-517	10	25	6	25	7	I suggest replacing "confidence level of 90%" with "significance level 10%". Use of the term confidence is confusing, because it is meant here in a difference sense than the confidence levels that are used in IPCC uncertainty language. Confidence levels and significance levels are often confused in statistical analysis, with the former reported as the inverse of the latter. The correct thing to report is the significance level, since that is the aspect of the test that is controlled by the analyst. [Francis Zwiers, Canada]	Paragraph deleted.
10-518	10	25	13	25	21	This paragraph would like to update the considerations of AR4 regarding the main cause for warming of the troposphere on the basis of the more recent studies, based on CMIP5. The general conclusion is that the AR4 conclusion ("it is likely that anthropogenic forcing has led to a detectable warming of tropospheric temperatures") is confirmed by actual studies. However, the more relevant consideration is the cooling effect of aerosols, in contrast with the warming effect of greenhouse gases. Is this really a finding successive to AR4? [Claudio Cassardo, Italy]	The regional aspects of the cooling effect of aerosols pointed out in this assessment is a successive finding to AR4.
10-519	10	25	20	25	21	Fair enough, but how much is greenhouse forcing and how much is internal variability? This is still an open issue especially since the lack of warming in the last decade. [Marcel Crok, The Netherlands]	This statement is confirmed by the signal to noise analysis by Santer et al. 2011
10-520	10	25	20			Why does the assessment remain "likely" given the conclusion of Ch9 given on page 24 line 16-18? [Albert Klein Tan k, Netherlands]	This statement addresses tropospheric temperautre as a whole and not only the tropical region. Conclusion of Ch9 will be reworded.
10-521	10	25	23			This section on the stratosphere doesn't address the middle or upper stratosphere. Even if there is not much to report, something should be said about this region, as the expected temperature signal there (and in the mesosphere) is very large. If no work has been done, simply say so. [Dian Seidel, USA]	Section 2 does not assess temperature changes above the lower stratosphere because of large uncertainties in the data. Therefore, the trends in the mid to upper stratosphere and mesosphere are not discussed here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-522	10	25	24	25	27	Doesn't this assertion rest on the assumption that we know stratospheric internal variability well? Do we? Or do we just know what models simulate it to be? The observational record is woefully short and might not reveal natural variations on long time scales. [Dian Seidel, USA]	We agree with the concerns of the reviewer. The text is amended to include this concern.
10-523	10	25	36	25	45	I think it would be better to say that the coupled chemistry-climate models include more processes explicitly, but that the degree to which this leads to improved comparison to observations is still under examination. I really don't think that it has been firmly established that the chemistry climate models as a group do a better job on eg. temperature trends in the lower stratosphere, although they may. [Susan Solomon, USA]	We aggree with the reviewer. Text has been added to make this point.
10-524	10	25	39	25	39	"reasonably well" is vague and subjective language [Dian Seidel, USA]	Text amended.
10-525	10	25	44	25	44	Is "on average" a reference to global average or an ensemble of model runs? [Dian Seidel, USA]	Text amended.
10-526	10	25	48	25	48	Clarify that the statement pertains to the average over 60N-60S, not to patterns within this large area. [Dian Seidel, USA]	Text amended.
10-527	10	25	51	25	57	The last sentence in this passage, about the importance of the QBO, seems a bit at odds with what precedes it. [Dian Seidel, USA]	Sentence removed.
10-528	10	25	54	25	55	Please add "Thompson and Solomon (2009) showed that ozone depletion linked to aerosols in the years just after major eruptions is also important to the temporal evolution of the coolng. (see J. Climate, vol 22, page 1934-) [Susan Solomon, USA]	Text amended to refer to Thompson and Solomon (2009)
10-529	10	25	56	25	57	"They also suggest that the QBO is important when explaining the causes of temperature trends in the tropical lower stratosphere": this sentence is obscure to me, written in this way. In their paper, they have carried out two set of simulations in which QBO was implicitly contained in the input data or prescribed, finding that the trends obtained were more similar to the observations and thus deducing the importance of the QBO. [Claudio Cassardo, Italy]	Sentence removed.
10-530	10	26	3	26	10	Once again ocean oscillations. ENSO, PDO and AMO, are not considered "natural". If inclided they will eliminate the supposed discrepanvies [VINCENT GRAY, NEW ZEALAND]	Modes of variability such as ENSO. PDO and AMO are simulated internally in many climate models, and are included as aspects of internal variability.
10-531	10	26	3	26	10	please add 'in the lower stratosphere' to 'temperature anomalies'. [Helga Nitsche, Germany]	Added. Thanks.
10-532	10	26	20	26	20	Consider changing "reassessed" to "re-evaluated" or "re-analyzed", as assess has a special meaning for IPCC [Dian Seidel, USA]	Rejected. Word not found in the text.
10-533	10	26	20	26	21	I don't see support for this statement in the Gillett reference. [Melissa Free, USA]	We agree with the reviewer. According to new research this statement is not valid anymore and the sentence is removed. The statement referred in part to the relative constancy of the TLS trends with latitude when evaluated up to 2005, as shown in Gillett et al. (2011), but new work demonstrates that when evaluated up to 2011 the Antarctic has cooled more quickly, consistent with models.
10-534	10	26	20	26	28	The seasonal pattern in the tropical LS is fairly robust, although the size of the cooling varies among datasets (Free 2011). Uncertainties in the seasonal structure of trends are mostly near the poles. [Melissa Free, USA]	Models do not simulate this seasonal trend structure and it has been suggested that it is caused by internal variability (Wang and Waugh, 2012 JGR).
10-535	10	26	24			typo "in in" [Albert Klein Tan k, Netherlands]	Corrected. Thanks.
10-536	10	26	25			feature not features; this is a run-on sentence and could be simplified [Larry Thomason, United States of America]	Corrected. Thanks.
10-537	10	26	30	26	36	This paragraph seems contradictory. Initially it is written "Evidence is robust that a combination of natural and anthropogenic forcings caused the observed temporal evolution of lower stratospheric temperatures" and	Text amended to be more clear.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						"combination of ozone depletion and increases in well mixed gg": last two are only anthropogenic forcings, not natural, so which one is the natural effect? Maybe volcanic one? Few lines later, it is written "support an assessment that stratospheric cooling is very likely due to the influence of anthropogenic forcing": so only anthropogenic, not natural (volcanic effects have disappeared). [Claudio Cassardo, Italy]	
10-538	10	26	33			Given the discussion of the Simmons et al. (2010) findings, updating and contradicting those of Willett et al. (2007), it doesn't seem reasonable to include Fig. 10.8 from the latter study. [Dian Seidel, USA]	Accepted, this figure is no longer shown.
10-539	10	26	36	26	36	be specific that it is ODS that is the principle anthropogenic forcing here. Otherwise people will get confused. [Gavin Schmidt, USA]	Text amended.
10-540	10	26	39	26	43	Please be more explicit or delete - why do you think we have an improved understanding of stratospheric temperature changes since AR4? [Susan Solomon, USA]	Text modified to be more spedific.
10-541	10	26	39	27	52	Consider changing the order of these two paragraphs to keep discussion of surface humidity changes intact. [Dian Seidel, USA]	Accepted - text revised as suggested
10-542	10	26	47	26	45	One of the problems of representing the hydrological cycle is its characteristics of presenting high variations at very small scales (catchment scales), making thus difficult to determine its components at the larger scales of climate models. This is strictly linked with the high variability of each component, as underlined at page 27, lines 9-11. [Claudio Cassardo, Italy]	Accepted - text revised
10-543	10	26	47	26	47	Please define hydrological variables in this context (not limited to precipitation and atmospheric water vapour). [Lena M. Tallaksen, Norway]	Accepted - text revised to clarify the scope of this subsection.
10-544	10	26	55			Please be aware that I made a throrough review of changes in the water cycle that should be referred to here and used in the analysis: Trenberth, K. E., 2011: Changes in precipitation with climate change. Climate Research, 47, 123-138, doi:10.3354/cr00953. Available from my web site: http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/SSD%20Trenberth%202nd%20proof.pdf [Kevin Trenberth, USA]	Taken into account - although the article cited in this comment is focused on detection rather than attribution
10-545	10	26	57	27	16	There are three consecutive paragraphs with no references – what is the source of this material. Refer to St. Jacques, et al. 2010. Geophysical Research Letters, Vol. 37, L06407, doi:10.1029/2009GL042045, 2010. [David Sauchyn, Canada]	Taken into account - introductory text is significantly revised. St Jacques et al. is assessed later in the section.
10-546	10	26		26		10.3.2 The water cycle - please clarify - is this equivalent to the hydrological cycle? In general this chapter has a major focus on precipitation, atmosheric humidity and the cryosphere. Please remember changes in evapotranspiration and runoff - two very important elements in the hydrologic or water cycle. [Hege Hisdal, Norway]	Taken into account - we have emphasized hydrologic variables for which detection & attribution studies (as defined in 10.2) have been carried out. Other variables of importance have not been assessed in the published literature. Purely data-based trends are assessed more thoroughly in Ch 2.
10-547	10	27	2			"…at a rate of about 7%/K near the Earth's surface." (the % rate is actually higher for colder temperatures at high latitudes and in the upper troposphere) [Richard Allan, UK]	Taken into account - text is condensed significantly and this phrase is deleted.
10-548	10	27	7	27	7	what are "which" in "most of which"? Not clear. [Xuemei Wang, China]	Accepted - the phrase "most of which" has been eliminated in the process of condensing this section of text.
10-549	10	27	13	27	13	Surface water budget – not clear what this include. [Lena M. Tallaksen, Norway]	Accepted - text revised to clarify what variables are assessed in this subsection.
10-550	10	27	39		52	Please note Chapter 2 and the new studies by Dai et al 2011 J Climate p 965 as a major article on homogeneity of the upper air moisture that invalidates prior analyses based on sondes in many ways. The reanalyses (Simmons et al) also depend on the flawed sondes although their statements about recent trends may be OK. [Kevin Trenberth, USA]	Accepted - text revised
10-551	10	27	54	27	55	This metric is closely coupled with the SST. The last ten years there was no trend in SST so no rise in water vapour as well. So the trend since 1988 is now lower. Show the updated figure from AR4. [Marcel Crok, The	Taken into account - But we have removed the figure on humidity trends from the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Netherlands]	
10-552	10	27				Section 10.3.2.1 seems to repeat discussion of observed changes in Chapter 2 and could be reduced. This may also be the case for other similar repetition in this chapter. [Richard Allan, UK]	Accepted - text condensed significantly
10-553	10	28	5		13	Please see Trenberth (2011 Clim Res) for more on this. Direct effects are many: more intense, storms, taller narrower Hadley cell, changes in frequency and patterns. [Kevin Trenberth, USA]	Accepted - text revised, although we have condensed this section considerably
10-554	10	28	15	28	25	Note that this is not a forced response, but rather an internal process - the confusion with strat water vapor forcing from increased methane is going to be a problem. Indeed, since water vapor feedbacks aren't really the subject of this chapter, why discuss this here anyway? [Gavin Schmidt, USA]	Accepted - text revised to clarify this point. This paragraph has been condensed, but not eliminated.
10-555	10	28	15	28	25	I question whether this paragraph belongs in this chapter, as it has little to do with robust detection/attribution research. If it stays, consider changnig "much" (line 15) to "vastly" (as the concentrations are 4-5 orders of magnitude smaller) and add mention/reference to Lanzante (2009), who questioned Rosenlof and Reid's results. Lanzante, J R, 2009: Comment on "Trends in the temperature and water vapor content of the tropical lower stratosphere: Sea surface connection" by Karen H. Rosenlof and George C. Reid. Journal of Geophysical Research, 114, D12104, doi:10.1029/2008JD01054 [Dian Seidel, USA]	Accepted - text has been revised to emphasize the potential importance of stratospheric water vapor but the present uncertainty in detection and attribution assessment
10-556	10	28	15		30	The models do not get the upper atmosphere right and it is not close. They are simply not good enough to say anything like this section Pierce D. W., T. P. Barnett, E. J. Fetzer, P. J. Gleckler (2006),Three-dimensional tropospheric water vapor in coupled climate models compared with observations from the AIRS satellite system, Geophys. Res. Lett., 33, L21701, doi:10.1029/2006GL027060. [Tim Barnett, USA]	Accepted - text has been revised to emphasize the potential importance of stratospheric water vapor but the present uncertainty in detection and attribution assessment
10-557	10	28	16	28	16	It is unclear why the 'surface energy budget' is mentioned here, since water vapor at this high altitude should figure in most prominently into the reducing the OLR (i.e., affect the top of atmosphere energy budget) [Chris Colose, United States]	Accepted - text revised and clarified.
10-558	10	28	16	28	23	Based on the discussion here, Rosenlof and Reid (2008) and Solomon et al (2010) seem to come to contradictory conclusions, with the former making a link between decreasing water vapour and increasing ocean surface temperatures (which represent 70% of the global mean) and the latter suggesting a link that decreases surface temperatures. Perhaps the authors could resolve (or at least recognize) this contradiction. [Francis Zwiers, Canada]	Accepted - text has been revised to emphasize uncertainties in stratospheric water vapor assessment
10-559	10	28	17	28	20	it may be of interest to mention which data (instruments) are specifically used for moisture in the UTLS-region. [Helga Nitsche, Germany]	Rejected - this is a reasonable point but we have chosen to condense, rather than expand, this paragraph
10-560	10	28	20	28	25	This result, despite based on "relatively short and sparse record of stratospheric water vapour", is of extraordinary importance in my opinion, due to the implications and connections with the observed tropospheric warming rate, and could deserve a major emphasis. [Claudio Cassardo, Italy]	Accepted - text has been revised to emphasize the potential importance of stratospheric water vapor but the present uncertainty in detection and attribution assessment
10-561	10	28	32	28	32	Both here and in the section about extreme precipitation you should give a warning that models don't do a good job in simulating rainfall. One important paper is Stephens, G. L., T. L'Ecuyer, R. Forbes, A. Gettlemen, J.-C. Golaz, A. Bodas-Salcedo, K. Suzuki, P. Gabriel, and J. Haynes (2010), Dreary state of precipitation in global models, J. Geophys. Res., 115, D24211, doi:10.1029/2010JD014532. He wrote: "However, these models produce precipitation approximately twice as often as that observed and make rainfall far too lightly." (...) "This implies little skill in precipitation calculated at individual grid points, and thus applications involving downscaling of grid point precipitation to yet even finer-scale resolution has little foundation and relevance to the real Earth system." For me this makes attempts for attribution of precipitation and extreme precipitation at this stage very prematurely. Stephens confirmed this more or less during his recent talk at the AGU. [Marcel Crok, The Netherlands]	Accepted - text has been rewritten to emphasize challenges in simulating precipitation, with Stephens et al cited.
10-562	10	28	32			10.3.2.2 The focus on global is really inappropriate given the expected small and non-detectable signal. Rather the regional aspects (patterns) and intensity should be the focus. [Kevin Trenberth, USA]	Taken into account - Zonal/regional precipitation, across the globe but principally over continents where data constraints are fewer, is the focus here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-563	10	28	34	28	34	"In a warmer climate" is a dangling modifier here and is unneeded in any event. [Dian Seidel, USA]	Accepted - text revised
10-564	10	28	37	28	41	Misleading – the natural reading is that the increase in GHG is responsible for the increase in precipitation being less than Clausius-Clapeyron, which is completely untrue. I suggest text like "Global-mean precipitation rates are not expected to increase at the 7% of the Clausius-Clapeyron relationship, because they are determined more by energy balance than by moisture availability. Warming the troposphere increases its radiative cooling, thereby decreasing precipitation, but this is partly offset by increasing greenhouse gases (Allen & Ingram, 2002)." [William Ingram, UK]	Accepted - test revised to improve clarity
10-565	10	28	41	28	43	"Changes .... 2008))" is too strong: Pall et al is just a study of a single GCM, while Allan & Soden did not show a C-C response but a stronger one, & has effectively been retracted by Allan & al (2010). "... however, may be closer to the Clausius..." would be justifiable. [William Ingram, UK]	Accepted - test revised to improve clarity
10-566	10	28	43	28	44	While Min et al (2011) do report a detection of anthropogenic effects on extreme precipitation, there are serious problems with this paper. They find ANT, the anthropogenic-only signal, consistent with the data and fairly robustly detectable (in a rather sparse dataset), but not ALL, the signal we can actually expect the climate system to have responded to (anthropogenic plus volcanic - & solar, but that seems negligible). (Paragraph 1 of 5.) [William Ingram, UK]	Taken into account - Min et al results are kept in FOD Fig 10.10 but Fig 10.16 has been removed
10-567	10	28	43	28	44	We know, of course, that both in reality & in the GCMs volcanoes cooled the planet late in the period considered, and so reduced the overall warming over the period. Assuming any signal in extreme precipitation is primarily through mean temperature, this would reduce the size of ALL compared to ANT, making it harder to detect ALL in the real world than it would be to detect ANT in a contrafactual world where it was the physically plausible signal because there was no significant volcanic forcing during the period. (Paragraph 2 of 5.) [William Ingram, UK]	See response to comment 567
10-568	10	28	43	28	44	But that alone provides no physical justification for using ANT in the real world where ALL is our best guess at the real signal. Consistency with the data & detectability for ANT but not ALL *would* make logical & physical sense if 1) we could interpret it as indicating that the modelled volcanic signal was not real (no eruptions happened, or their real effect was not even correlated with the modelled effect) - but of course we know the opposite is true. 2) the ANT signal contained an error similar is shape & size to the volcanic signal but of opposite sign - but even if there were evidence for, or suggestion of, such an error, it would hardly be satisfactory to base the results on assuming such a cancellation of errors. (Paragraph 3 of 5.) [William Ingram, UK]	See response to comment 567
10-569	10	28	43	28	44	I have raised this problem with the authors & understand their defence to be 3) not only does ANT have higher signal/noise than ALL, but the dominant signal in both is just an overall increase, so it is legitimate to use the larger one. This certainly makes sense, but it means that the tests they apply are doing no more than check the sign of the trend, so they cannot claim statistical significance at better than the 50% level. Looking only for the shape, not the size, of the signal is usual in detection & attribution, but in this case is in fact the only way to get a result, since the data show considerably more trend than the modelled signal (which makes no physical sense & leaves me suspecting an artifact of inhomogeneity &/or a mismatch of modelled and real variance on the scales of this sparse dataset). (Paragraph 4 of 5.) [William Ingram, UK]	See response to comment 567
10-570	10	28	43	28	44	In short, I consider it clear that the authors have significantly over-interpreted the data and - while I expect anthropogenic global warming has in fact increased extreme rainfall - their conclusions are not well-founded. I therefore suggest that Figure 10.16 is omitted & the text from ", and" is omitted or replaced by text along the lines of ". Min et al (2011) report nominal detection of anthropogenic effects alone on extreme precipitation, but not of the plausible combined anthropogenic and natural signal, suggesting problems with the GCMs or the data or both." (Paragraph 3 of 5.) [William Ingram, UK]	See response to comment 567
10-571	10	28	46			Need an assessment here: Wentz et al used a flawed dataset (SSM/I) based on 2 times daily values per satellite and a limited period: the trends are not represnetative of other datasets such as GPCP. See chapter 2. [Kevin Trenberth, USA]	This paragraph has been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-572	10	28	57	29	3	Here, and elsewhere (page 30 line 29), Stott et al. (2010) is quoted verbatim, and I don't recall seeing any other papers quoted. I'd suggest summarizing findings without quotations throughout. In this instance, the quote is problematic, because "the effects of long wave forcing" is jargon for "greenhouse effect", which is a more readily understood term. [Dian Seidel, USA]	Accepted - text substantially revised here.
10-573	10	29	1			10.3.2.3 Please also compare with chapter 2. Need to be compatible. Also there are important regional and seasonal aspects that should be considered (Trenberth 2011). For instance, snow should increase in midwinter with warmer conditions but the snow season should be shorter at both ends. [Kevin Trenberth, USA]	Accepted - Sec 10.3.2.3 is rewritten to improve compatibility with Ch 2, but also condensed to focus more clearly on the limited number of explicit detection/attribution studies on surface hydrologic variables.
10-574	10	29	2			unclear to what "long wave forcing" refers to (natural forcing?). [Albert Klein Tan k, Netherlands]	Accepted - "long wave forcing" has been removed and revised.
10-575	10	29	5	29	6	"over two periods during the 20th century": actually, over summers and winters of the second half of the 20th century. [Claudio Cassardo, Italy]	Accepted - text clarified here
10-576	10	29	5	29	17	Changes in the seasonal cycle of precipitation have also been discussed in Chou et al. (2007, GRL, 34, doi:10.1029/2007GL030327) and Chou and Lan (2012, J. Climate, 25, 222-235). [Chia Chou, Taiwan, ROC]	Accepted, although this chapter emphasizes detection and attribution of observed trends rather than diagnosis of projected trends
10-577	10	29	12	29	17	The fact that the observed changes are substantially larger than the simulated changes (Figure 10.9, zonal means and scaling factors) deserves some comment. It seems to imply that there are other factors which are more important over this period than pure anthropogenic factors, or there are problems with the observations, or the models. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - The need for scaling factors is clarified in the text.
10-578	10	29	14	29	14	((should be ([William Ingram, UK]	Editorial - typo fixed
10-579	10	29	16	29	17	"In that study, only boreal spring showed changes that were significantly and robustly larger than simulated in the multi-model mean. (Figure 10.9)": actually, Fig. 10.9 shows winter and summer, not spring. [Claudio Cassardo, Italy]	Accepted - text clarified here
10-580	10	29	32	29	33	Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think those results should be removed from the figure, and so the relevant part of the caption also removed [William Ingram, UK]	Rejected - the Min et al results are included for completeness and their robustness is assessed in the text.
10-581	10	29	37	29	43	This paragraph should mention that most of the subtropics have not been analyzed for precipitation changes. Or report results for subtropical Asia, Africa, and South, North and Central America … [Dian Seidel, USA]	Accepted - text clarified here
10-582	10	29	52	29	52	I would avoid the word "significant" unless you mean statistical significance. If statistical significance is what is meant, then the medium confidence assessment suggests that findings of statistically significant changes are uncertain. But given a particular dataset and a specific estimation and inference procedure, and change is either significant or not. A suggestion for wording might be "there is medium confidence that there has been a human influence on global scale precipitation patterns over land ...". Mentioning that the assessment is for land areas is important - I'm not aware that there have been detection studies on global or ocean precipitation. [Francis Zwiers, Canada]	Accepted - the text is clarified to specify land areas, and the term "significant" is removed
10-583	10	29	53	29	53	in land precipitation patterns [Laurent Terray, France]	Accepted - text revised
10-584	10	29	54	29	54	zonal mean land precipitation [Laurent Terray, France]	Accepted - text revised
10-585	10	29				It would be helpful to include a brief discussion here on how well the simulated multi-decadal variability in continental average precip agrees with observations. This is important as it may help to explain the apparent underestimate of observed trends in precip by the forced simulations, as well as underestimate of observed variability in the simulations. [David Karoly, Australia]	Taken into account - this is a reasonable comment but the text here has been condensed, and the model assessment is principally described in Chapter 9
10-586	10	29				in the middle of line 42, before "Each of these regions…", the following text may be added: "Zhang et al. (2012) reported that the precipitation of the main rain season, i.e., July, August and September, and annual	Taken into account - this paragraph has been rewritten and condensed to clarify attribution of these

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						total precipitation in the central part of Sudan decreased significantly during 1948–2005 and the decreasing precipitation in Sudan was associated with the weakening African summer monsoon." The reference of the added text is: Zhang, Z.X., C-Y. Xu, M. El-Tahir, J. Gao, V.P. Singh, 2012: Spatial and temporal variation of precipitation in Sudan and their possible causes during 1948–2005. Stoch Environ Res Risk Assess, 26:429–441. [Chong-Yu Xu, Norway]	precipitation trends
10-587	10	30	3	30	5	Here it is stated that (should come earlier) the surface water budget involves precipitation, evapotranspiration and runoff (note streamflow is used in the title). [Lena M. Tallaksen, Norway]	Accepted - text revised
10-588	10	30	5	30	5	Add "partly" before "dependent on temperature". If soil moisture is limiting evapotranspiration, changes in temperature will have a limited effect on evapotranspiration (or even none in case of fully dry conditions). [Sonia Seneviratne, Switzerland]	Accepted - text revised as suggested
10-589	10	30	5	30	6	Replace "such as humidity and wind" with "such as relative humidity, wind and soil moisture content" [Sonia Seneviratne, Switzerland]	Accepted - text revised as suggested
10-590	10	30	8	30	8	It is further concluded that “trends in the surface water budget is of tremendous interest” (why particularly in the subtropics is however, not clear). This is not reflected in the report as a whole, and although this section states the important role of the global water balance components in providing evidence of anthropogenic climate change, it provides little information on evapotranspiration, soil moisture and runoff. Rather it is biased towards snow related trends (runoff, snowpack). [Lena M. Tallaksen, Norway]	Taken into account - text has been revised to improve clarity. This section emphasizes the components of the water cycle for which detection & attribution studies have been carried out.
10-591	10	30	10	30	11	It is stated that runoff and soil moisture are difficult variables to access trends in because these are sparsely observed. This is certainly the case for soil moisture observations, but runoff (or streamflow) is the only variable of the terrestrial water cycle that is monitored with relatively high spatial and temporal coverage, and regional and global river flow archives hold vital information for evidence-based assessment of past hydrological variability and change (Hannah et al., 2011). [Lena M. Tallaksen, Norway]	Accepted - text revised to distinguish between the difficulties in carrying out detection/attribution on soil moisture and runoff. Large decadal variability and non-climatic human diversions inhibit D&A assessment on runoff.
10-592	10	30	10	30	12	Soil moisture is sparsely observed, runoff generally is not, especially in comparison to soil moisture. By poorly observed, do the authors mean sparse again. There is a significant difference between sparse (widely spaced short records) and poor (weak methodology). [David Sauchyn, Canada]	Accepted - text clarified as suggested
10-593	10	30	22	30	25	Also refer here to the SREX (2012; see chapter 3, Box 3.3 of that report). [Sonia Seneviratne, Switzerland]	Accepted - text revised as suggested
10-594	10	30	29	30	29	The mechanisms were first proposed in early GFDL papers by Manabe, Wetherald and Stouffer eg Manabe and Stouffer 1980,JGR, 85, p5529-5554, Manabe and Wetherald, 1985, SCIENCE Volume: 232 Issue: 4750 Pages: 626-628 DOI: 10.1126/science.232.4750.626 [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this paragraph extensively revised
10-595	10	30	29	30	32	Similar statements were made by other researchers, e.g., "Stewart I T 2009 Changes in snowpack and snowmelt runoff for key mountain regions Hydrol. Process. 23 78–94" [Omer L. Sen, Turkey]	Taken into account - this paragraph extensively revised
10-596	10	30	29	30	56	Overall, this bit on changes in the cryosphere seems to be a bit less than comprehensive. [Francis Zwiers, Canada]	Taken into account - we have assessed and cited several additional snowpack-related references, although the principal section on cryospheric change comes later in this chapter (sec 10.5).
10-597	10	30	29	30		In this paragraph, recent studies addressing trends in runoff (annual and seasonal values) should be added (refer comments and references provided for Chapter 2). [Lena M. Tallaksen, Norway]	Accepted - text revised
10-598	10	30	29		32	The quoted comment is OK but doesn't seem especially illuminating and it is kind of odd to quote this at length when it could summarized as easily as it has been done with regard to other citations [Larry Thomason, United States of America]	Accepted - text revised
10-599	10	30	29		35	this was said several decades before Stott et al. Please get proper attribution or state 'it is well known that …'. [Tim Barnett, USA]	Accepted - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-600	10	30	33	30	35	This shifting of peak runoff to earlier in the year also is apparent in the northern Rocky Mountains where extensive winter snow exists at relatively low elevation and therefore near the freezing point. Mote (2006, Figure 5) shows this. [David Sauchyn, Canada]	Accepted - text revised to note other regions, including the northern Rockies, where timing of peak runoff has shifted.
10-601	10	30	37	30	56	There is no mention of the influence of ENSO and PDO on the inter-annual and interdecadal (respectively) variability in the western NA cordillera. This accounts for much of 'noise' that prevents the detection of trends from short records. [David Sauchyn, Canada]	Taken into account - in condensing this section we have kept discussion more general, indicating large interannual and decadal variability as a consideration in climate change assessment
10-602	10	30	37			type "trends been" [Albert Klein Tan k, Netherlands]	Editorial - typo fixed
10-603	10	30	37			St Jacques et al 2010 is a much more recent reference for western Canada than Zhang et al 2001. [David Sauchyn, Canada]	Accepted - this paper is assessed and cited in the SOD
10-604	10	31	1			Section 10.3.3: Suggest changing the title of this section to 'Circulation and climate phenomena' to better highlight its content and for consistency with the projections chapters [Julie Arblaster, Australia]	Title has been changed.
10-605	10	31	1			Section 10.3.3: The name for this section ('Climate phenomena') is not the best one. A better name would be 'Atmospheric circulation and patterns of variability'. This would be more consistent with the corresponding section in Chapter 2: Section 2.6 'Changes in Atmospheric Circulation and Patterns of Variability'. [Alexey Karpechko, Finland]	Title has been changed.
10-606	10	31	1			Where is PDO in this assessment of Climate Phenomena? It is considered in other chapters; 2 and 14 in particular. Why not here? The PDO is a "fundamental mode of variability" and "Associated with widespread anomalies in the surface air temperature and precipitation over the entire North American continent and extratropical North Pacific" (Box 14.2, page 14-11). [David Sauchyn, Canada]	PDO like other climate phenomena in the revised draft is not discussed here due to large uncertainties in observations and modelling precluding an attribution assessment in changes in the PDO at this stage.
10-607	10	31	12	32	17	Where is the D&A? All the cited info shows just how poorly we understand the tropical citculation The models are poor and the obs contradictory.So how can you possibily write lines 16-17 (pg 32)? This is the old IPCC con job [Tim Barnett, USA]	The text in this section has been revised including the deletion of the statement at lines 16-17.
10-608	10	31	14	32	17	I very much like the physical basis of this discussion, but unless I have missed something, there is no indication of the statistical significance (detection) which I suspect should it be possible to demonstrate or attribution (which I think suspect might be difficult to demonstrate) This material might be better split between the observational and modelling chapters with a brief summary here [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	The text has been heavily revised and shortened to concentrate on where model simulations with different forcings have been compared with observed trends, including a new figure.
10-609	10	31	18	31	28	"since the late 1970s ... widening estimates range between around 0° and 3° latitude per decade" and later "The observed widening of between about 2 and 5 degrees latitude between 1979 and 2005": the second sentence specifies better than the first one the amount and the timing of the widening: I suggest to eliminate the first sentence. [Claudio Cassardo, Italy]	Text has been revised and the text has been deleted.
10-610	10	31	22	31	28	Is it 0 to 3 degrees per decade or 2-5 since 1979? Please be consistent in units and implications [Susan Solomon, USA]	Text has been revised and there is no longer such a confusion of numbers.
10-611	10	31	31	31	31	Point missing after "2011)". [Christian-D. Schoenwiese, Germany]	Thank you but text revised heavily here anyway.
10-612	10	31		33		Section 10.3.3 deals with climate pheonamena, some of which relates atmosphere-"ocean" interactions and also affects "weather". I am wondering if this subsection can be moved below ocean section or merged to section 10.6. [Seung-Ki Min, Australia]	Ocean interactions do affect all atmospheric changes potentially but we have kept this sub section here since it relates to atmosphere and surface predominantly.
10-613	10	32	2	32	9	I think it is necessary to include results from at least one other reanalysis in this figure. My impression (perhaps erronious) is that the model used in the NCEP/NCAR reanalysis has rather distinctive tropical characteristics. [Francis Zwiers, Canada]	Agreed. The figure has been replaced with a new figure that includes multiple reanalyses.
10-614	10	32	9	32	9	The figure is not based on Seidel et al. (2008). I'm guessing it is from the work of Seok Woo Son and colleagues. [Dian Seidel, USA]	This is correct that it is not based on Siedel but the figure has been replaced in the SOD with a new

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							figure.
10-615	10	32	16	32	17	I don't think the last sentence is helpful. Why 'taking these lines of evidence together'? It seems that what you say is that there is good evidence for a role of ozone depletion and no clarity about other GHGs, so best to leave it at that; the additional sentence about 'anthropogenic influences' seems confusing. [Susan Solomon, USA]	Agreed. The last setence has been deleted and the text revised.
10-616	10	32	21	32	53	Cross reference back to Chapter 9.5.3.4.1 also needed [George Kiladis, USA]	This sub section has been deleted and replaced with a very short summary at the start of the section .
10-617	10	32	25	32	25	excercised - spelling [Peter Clift, United States of America]	Sub section has been deleted.
10-618	10	32	25	32	25	Typo, "exercised" (not "excercised"). [Christian-D. Schoenwiese, Germany]	Sub section has been deleted.
10-619	10	32	29	32	33	What do CMIP5 models say about projected ENSO activity? Neither of the projections chapters seems to have much to say on this topic, and Chapter 14 also says very little. [Francis Zwiers, Canada]	Sub section has been deleted given the lack of literature on this topic and replaced with low confidence summary statement at start of section.
10-620	10	32	32	32	32	may add foloowing reference after "Meehl et al., 2005b": "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]	Sub section has been deleted.
10-621	10	32	46	32	46	May add following sentence and reference: "Recent work pf Yeh et al. (2011) argued that it is possible that an increasing of occurrence frequency of CP El Niño during recent decades in the observation could be a part of natural variability in the tropical climate system." Yeh, S.-W., B. P. Kirtman, J.-S. Kug, W. Park, and M. Latif, 2011: Natural variability of the central Pacific El Niño event on multi-centennial timescales. Geophys. Res. Lett., 38, L02704, doi:10.1029/2010GL045886. [Zeng-Zhen HU, USA]	Sub section has been deleted.
10-622	10	32	48	32	48	may add following reference: Hu, Z.-Z., A. Kumar, B. Jha, W. Wang, Bohua Huang, and Boyin Huang, 2012: An analysis of warm pool and cold tongue El Niños: Air-sea coupling processes, global influences, and recent trends. Clim. Dyn., DOI: 10.1007/s00382-011-1224-9 (published online). [Zeng-Zhen HU, USA]	Sub section has been deleted.
10-623	10	32	55	33	11	A little clarification here would be useful, together with consistency with the use of AMO with other chapters. Chap 2 (Box2.4) seems to think that the AMO is a direct measure of the AMOC. It should be clarified here what the index is measuring (i.e.an area of SSTs). It should also be clarified if the radiative influences on the AMO suggested (by Mann and Emanuel 2006 and Booth et al 2011) are just impacting on the surface temperatures or via changes to the AMOC. i.e. The SSTs (AMO) could be influenced by the AMOC or by direct radiative forcing. And the AMOC has an internal variability component but could there also be a radiative forced component? [Gareth S Jones, UK]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at start of section.
10-624	10	32	57	32	58	Recommend pointing out that per Knight et al. (2005) the change in the AMO index, from peak negative values in the first decade or two of the 20th century to peak positive values in the last decade, is likely to represent a circa 0.1 C natural component of the 20th century rise in global mean temperature, in excess of 10% of the total. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at start of section.
10-625	10	32	57	32	58	Please include references (model and/or observational studies) for the statement that the AMO "has significant impacts on regional and hemispheric climate". References could include the following (1) Zhang and Delworth,GEOPHYSICAL RESEARCH LETTERS, VOL. 33, L17712, doi:10.1029/2006GL026267, 2006. (2) Latif and Keenlyside, Deep Sea Research Part II: Topical Studies in Oceanography, 2011, Volume: 58, Issue: 17-18, 1880-1894. [Chris Roberts, Uk]	Sub section has been delated and replaced with low confidence summary statement
10-626	10	32	57	33	3	Distinguish between the “AMO Index” defined in a specific way using detrended SST and the AMO. The issue is then whether there is a forced component in the Index, rather than on the AMO itself - which was not coined based on the specific index being used here. [Gavin Schmidt, USA]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at start of section.
10-627	10	32				The CGCMs generally do not produce a creditable ENSO. So while I applaud the D&A statement at the end of section 10.3.3.2, why waste so much space on the matter? [Tim Barnett, USA]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							start of section.
10-628	10	33	2	33	2	As both the lead author of the cited study, and the scientist who coined the "AMO" in 2000, I can confidently attest to the fact that the claim that Mann and Emanuel (2006) "suggested that the AMO is driven by changes in radiative forcing" is false. Mann and Emanuel (2006) simply argued that definitions of the AMO which involve a simple linear detrending of SST data suffer from a misallocation of forced and internal variability, and that the main changes in tropical Atlantic summer temperature over the past century are radiatively forced. They most certainly do NOT claim that the AMO itself (defined as originally by Delworth and Mann (2000) [Delworth, T.L., Mann, M.E., Observed and Simulated Multidecadal Variability in the Northern Hemisphere, Climate Dynamics, 16, 661-676, 2000] as an internal multidecadal oscillation centered in the North Atlantic with a heterogeonous pattern of warming and cooling) is radiatively forced. Please correct this statement so that is it factually defensible. [Michael Mann, USA]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at start of section.
10-629	10	33	6			Is it worth also noting Ottera, 2010 here? This is the other paper which breaks from the older paradigm, pointing towards greater role for forced changes. [Ben Booth, UK]	Sub section has been deleted and replaced with low confidence summary statement on changes in AMO at start of section.
10-630	10	33	13	34	53	NAM/NAO, SAM, IOD and monsoon changes are also covered in Chapter 14, cross references needed [George Kiladis, USA]	Noted: Cross-references have been added.
10-631	10	33	13			I find the decrease in confidence from AR4 to AR5 for NAO to be very instructive, and perhaps deserves a discussion of how to avoid premature claims in the future. Not just by arguing about the negative phase of NAO, but on the weakness of a methodology that permits this claim to be made. [Ramon de Elia, Canada]	Reject - As evidence is gathered it is to be expected that some will confirm previous statements and increase the confidence, while other evidence will contradict previous statements, and reduce the confidence level. This is the reason for the calibrated confidence language. For example 'likely' means a probability greater than 66%, it does not mean 100% probability.
10-632	10	33	13			Section 10.3.3.4: This section ignores the fact that most studies still find a positive NAM response in winter to GHG concentration increases in model simulations (see Chapter 14, Section 14.2.9), Morgenstern et al. (2010) being the only exception of which I'm aware. In RCP8.5 simulations, which I managed to analyze, the December-February NAM index (defined as the SLP difference between mid- and high-latitudes) averaged over available models (22 models) clearly increases over the 21 century, just as it did in the CMIP3 models (see Miller et al. 2006, Figure 8). It seems to be consistent with projected SLP decreases in high-latitudes and increases in mid-latitudes reported in Chapter 12 (Section 12.4.4.1). The presence of stratosphere-resolving models in CMIP 5 does not change the sign of the trend. [Alexey Karpechko, Finland]	We agree. This section has been revised to also include an assessment of NAM trends, which are positive in CMIP5 simulations as the reviewer asserts.
10-633	10	33	13			Section 10.3.3.4: I agree that presently it's not possible to attribute the NAM/NAO changes to anthropogenic forcing especially since the changes are not detectable when the recent years are taken into account. But I think it is important to mention that, in agreement with earlier findings, the results of the CMIP5 model suggest a positive change in the NAM index during the 21 century when global warming is projected to accelerate, because this implies a possibility that, presently, the forcing may be too weak to induce detectable changes. (There is actually a remarkable similarity between simulated increases of NAM index and global temperature – both timeseries are rather flat during the 20C and accelerate up during the 21C.) [Alexey Karpechko, Finland]	We agree. This section has been revised to also include an assessment of NAM trends, which are positive in CMIP5 simulations as the reviewer asserts.
10-634	10	33	20			unclear what has been tested exactly [Albert Klein Tan k, Netherlands]	The significance of the observed trend compared to internal variability. We thinkn this is clear.
10-635	10	33	39	33	39	What is the 'NAM itself' other than the NAM index? [Gavin Schmidt, USA]	The NAM is the mode of variability. The text has been revised to be clearer what is meant here.
10-636	10	33	47	33	48	in line with comment 3 [Laurent Terray, France]	I am not sure what this refers to.
10-637	10	33	47		48	Ineson et al (2011) show NAO/NAM response to Sun in a high-top model. [Joanna Haigh, UK]	This is a pure modelling study, not a detection and attribution study, so we do not assess it here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-638	10	33	50	33	15	Add Staten et al 2011, Breaking down the tropospheric circulation response by forcing, Climate Dynamics, early online to this discussion [Julie Arblaster, Australia]	Noted: This paper concludes that SSTs are the main driver but this is not an attribution study since it begs the question of what drives the SSTs. So this paper has not been cited.
10-639	10	33	52		57	Define SAM before line 57 [Larry Thomason, United States of America]	SAM has been defined at the start of the paragraph.
10-640	10	34	7	34	8	ref Miller et al (2006) [Gavin Schmidt, USA]	This reference has been added.
10-641	10	34	10		15	Roscoe and Haigh (QJRMS, 2007) show influences of different factors on evolution SAM index and demonstrate stronger influence of ozone depletion than GHGs. [Joanna Haigh, UK]	This reference has been added.
10-642	10	34	12	34	15	Same concern here as my comment 17 above. Please be clear and avoid the vague use of 'anthropogenic forcing'. There is evidence for a statistically significant trend in the SAM only in DFJ and MAM, with the DJF trend being due mainly to ozone depletion. It is not clear what is causing the MAM trend. [Susan Solomon, USA]	Noted: Text has been revised to avoid using the general term 'anthropogenic forcing' here.
10-643	10	34	32	34	53	In addtion to SST, some other factors, such as the thermal forcing of the Tibetan Plateau, might contribute to the long-term trend and/or interdecadal change of the East Asian summer monsoon. The following recently published papers in this area should be cited to clarify this point. Duan A. M. F. Li, M. R. Wang, and G. X., Wu. 2011: Persistent weakening Trend in the Atmospheric Heat Source over the Tibetan Plateau and its impacts on Asian summer monsoon. J. Climate, 24, 5671-5682. doi: 10.1175/JCLI-D-11-00052.1. Duan A. M. and G. X., Wu. 2009: Weakening Trend in the Atmospheric Heat Source over the Tibetan Plateau during Recent Decades. Part II: Connection with climate warming, J. Climate, 22, 4197-4212. Duan A. M. and G. X., Wu. 2008: Weakening Trend in the Atmospheric Heat Source over the Tibetan Plateau during Recent Decades. Part I: Observations, J. Climate, 21, 3149-3164. [Anmin Duan, China]	This sub section has been deleted and replaced with a very short summary at the start of the section .
10-644	10	34	46	34	49	With respect to discrepancies between observed and modeled late 20th century historical (CMIP3) behavior of the South Asian Summer Monsoon (SASM), please cite the work of Fan et al (2010) [Fan, F., Mann, M.E., Lee, S., Evans, J.L., Observed and Modeled Changes in the South Asian Summer Monsoon Over the Historical Period, J. Climate 23, 5193-5205, 2010] the abstract of which is as follows: The behavior in the South Asian summer monsoon (SASM) was analyzed in Coupled Model Intercomparison Project (CMIP3) multimodel historical (20c3m) simulations and in modern observational and reanalysis data. The CMIP3 simulations capture the observed trend of weakening of the SASM circulation over the past half century, but are unable to reproduce the magnitude of the observed weakening trend. While the observations indicate a slight decrease in SASM-related precipitation, the CMIP3 simulations indicate on average a very slight increase, albeit with very large intermodel and intramodel variabilities. The CMIP3 simulations reproduce the observed negative relationship between the SASM and ENSO. The observed weakening trend in this relationship in recent decades, which has been attributed in some studies to anthropogenic forcing, appears to be well within the variability of the CMIP3 multimodel ensemble. For some models, distinct realizations indicate both strengthening and weakening trends that are larger in magnitude than the observed weakening trend. [Michael Mann, USA]	This sub section has been deleted and replaced with a very short summary at the start of the section .
10-645	10	34	49	34	53	This is not really true - there also are a number of papers suggesting that the 1970s-80s Sahel drought was partly due to anthropogenic aerosol emissions. See Ackerley et al. (2011) and references therein. Ackerley, D., Booth, B.B.B., Knight, S.H.E., Highwood, E.J., Frame, D.J., Allen, M.R. and Rowell, D.P., 2011: Sensitivity of 20th century Sahel rainfall to sulphate aerosol and CO2 forcing. J. Climate, 24, 4999-5014 [David Rowell, United Kingdom of Great Britain & Northern Ireland]	This sub section has been deleted and replaced with a very short summary at the start of the section .
10-646	10	34	55			I'm surprised no mention is made of changes in ocean pH in this section. Even if no formal attribution work has been done, that situation could be mentioned. This is a high-profile topic among "Changes in Ocean Properties". [Dian Seidel, USA]	Attribution of ocean acidification is described section Chapter 3. We will include a refernece to that section in the revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-647	10	34				ditto 10.3.3.7 [Tim Barnett, USA]	Accepted
10-648	10	35	1	35	2	The attribution of changes in Ph in the ocean (acidification) should be delt with somewhere in the report. Now it is not in Chapter 10 nor in Chapter 3. After the sentence it should be said where in the report attribution of changes in Ph is delt with. [Øyvind Christophersen, Norway]	Attribution of ocean acidification is described section Chapter 3. We will include a refernece to that section in the revised text.
10-649	10	35	26	35	29	perhaps one needs to add a couple of sentences about the fact that the XBT problem is not solved yet … [Laurent Terray, France]	This is a data problem and we now refer to Chapter 3. We now include reference to the section in the revised text.
10-650	10	35	29	35	29	Consider changing "non-climate related artefact" to "observational artifact, not a climate signal" for clarity [Dian Seidel, USA]	Accepted
10-651	10	35	29			"artefact"; I think the intended word is "artifact", meaning a consequence of something not intrinsic, e.g., a distortion in an image or sound caused by a limitation or malfunction in the hardware or software. as opposed to, artefact 'a product of human art or workmanship'. [Stephen E Schwartz, USA]	Accepted
10-652	10	35	34	35	34	Should this be "Figure 10.12a"? [Gareth S Jones, UK]	Accepted
10-653	10	35	34	35	34	Figure 10.13a should be Figure 10.12a. [Zhaomin Wang, UK]	Accepted
10-654	10	35	34	35	35	Really? This seems to contradict what was said in the previous sentence and what is in figure 10.12a [Gareth S Jones, UK]	Clarified in the text
10-655	10	35	34	35	56	Where it is written Fig. 10.13 (lines 34, 51, 56), it refers to Fig. 10.12. [Claudio Cassardo, Italy]	Accepted
10-656	10	35	35	35	39	Roy Spencer showed (see http://www.drroyspencer.com/2011/06/more-evidence-that-global-warming-is-a-false-alarm-a-model-simulation-of-the-last-40-years-of-deep-ocean-warming/) that AR4 was misleading about ocean heat content and the climate sensitivity that can be derived from it. The increase in ocean heat content since 1955 suggests a very low climate sensitivity according to him. I hope Spencer will submit a paper about this, but regardless I think his blog post deserves a reaction from IPCC. [Marcel Crok, The Netherlands]	Rejected - not supported by the peer reviewed literature
10-657	10	35	37	35	42	The last two sentences of this paragraph should be amended to refer to the modelled response of the ocean to volcanic eruptions. As written, it is implicitly the actual ocean response that is being referred to; the actual response is not known and there is good evidence that AO-GCMs generally mix surface heat into the ocean too efficiently. Sokolov et al., 2003, Comparing Oceanic Heat Uptake in AOGCM Transient Climate Change Experiments, J.Clim. found a sample of 11 AO-GCMs to have effective ocean diffusivities between 4 and 25 cm^2/s, with a mean of 10. By comparison, Forest et al. (2006) found that observations constrained effective ocean diffusivity to no more than 4 cm^2/s (95% confidence) with a mode of 0.65. See also Forest et al, 2008, Constraining climate model parameters from observed 20th century changes. Tellus A, 60A, 911-920, and Hansen et al. (2011). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accepted - text was amended, references to be assessed at timeof writing
10-658	10	35	49	35	51	This sentence is a bit garbled [Gareth S Jones, UK]	Accepted - text was amended.
10-659	10	35	52	35	52	" … based on …" ("on" missing). [Christian-D. Schoenwiese, Germany]	Accepted - text was amended.
10-660	10	36	5	36	6	Here and elsewhere in this section, the language seems to have forgotten about solar forcing (there are consistent references to contributions from anthropogenic and volcanic sources, while CMIP3 20th century runs that would have been available for D&A used either ANT, NAT or ALL forcing ...). [Francis Zwiers, Canada]	Rejected - not supported by the peer reviewed literature, with contributions from solar forcing.
10-661	10	36	7	36	8	The evidence does not support very high confidence in the contributions of anthropogenic and volcanic sources to observed increases in global ocean heat content and virtual certainty that these increases can be attributed to anthropogenic and volcanic forcing. It may support some part of those increases being so attributable (i.e. detection, but certainly not 100% attribution thereto - or even >50% attribution thereto). The pre-ARGO ocean heat content data is simply too poor quality to support a very high confidence level. No matter how many studies there are, they are all based on the same original poor quality observations, and the	Rejected - the newer literature on data problems, and the assessment in chapter 3, along with the new papers on detection and attribution support high confidence. Accepted and now extremely likely

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						model-based studies involve models that are thought in general to substantially misestimate mixing of surface heat into the deep ocean. Sokolov et al., 2003, Comparing Oceanic Heat Uptake in AOGCM Transient Climate Change Experiments, J.Clim. found a sample of 11 AO-GCMs to have effective ocean diffusivities between 4 and 25 cm^2/s, with a mean of 10. By comparison, Forest et al. (2006) found that observations constrained effective ocean diffusivity to no more than 4 cm^2/s (95% confidence) with a mode of 0.65. See also Forest et al, 2008, Constraining climate model parameters from observed 20th century changes. Further, recent Argo data shows a much lower (near zero) rate of global ocean heat content increase, which is inconsistent with the claimed attribution. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-662	10	36	7	36	8	"...increases... attributed to ... volcanoes"? This is unclear. Either "changes" are attributed to volc+anthro or "increases" to anthro + "short term decreases" to volc. [Gavin Schmidt, USA]	accepted - text changed
10-663	10	36	12	21		Palmer et al. (2009) also point out that ocean advection, when combined with non-homogeneous and temporally varying sampling, might be problematic for global estimates of ocean warming. For example, in the case of the relatively well-observed N. Atlantic, if we have advected warm waters from less well-observed regions over the historical record, we might over-estimate global ocean warming (if we do not "see" the corresponding cooling associated with warm water export). [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	to be confirmed
10-664	10	36	14	36	14	A further study could be cited. Using an ocean model hindcast for 1958-2001, Grist et al. (2010) diagnosed the relative role of anomalies in advective ocean heat transport convergence and area-integrated surface net heat fluxes, for annual heat storage in three zones of the North Atlantic. At the interannual timescale, anomalies in heat transport convergence are found to dominate anomalies in surface heat flux outside the tropics. Reference: Grist, J. P., Josey, S. A., Marsh, R., Good, S., Coward, A. C., de Cuevas, B. A., Alderson, S. G., New, A. L., and G. Madec (2010). The roles of surface heat flux and ocean heat transport during four decades of Atlantic Ocean temperature variability. Ocean Dynamics, 60, 771-790. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	to be confirmed
10-665	10	36	14	36	18	the 14C isotherm only allows to analyze low to midlatitude changes (from the Eq. To 45-50°) not global ocean changes [Laurent Terray, France]	Accepted - minor text change
10-666	10	36	15	15		Suggest replacing "allowed the separation of" with "is designed to separate" [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted - minor text change
10-667	10	36	31	36	44	Given the VERY strong assessment that is made concerning ocean heat content, it would be useful to include results from a formal D&A study (Palmer et al) in this figure. [Francis Zwiers, Canada]	to be confirmed
10-668	10	36		38		Freshwater fluxes (runoff) are here estimated as Precipitation minus evapotranspiration globally. This is an interesting study that should be linked to Section 10.3.2.3 and other studies of global changes in observed or modeled runoff using large-scale hydrological models. [Lena M. Tallaksen, Norway]	Accepted - added link
10-669	10	37	5	37	7	These new analyses also show a clear enhancement of the high-salinity subtropical waters, and freshening of the high latitude waters (e.g., Figure 10.13a, lower panel and middle panels).' Use of the word enhancement here is ambiguous, do the authors mean an increase in salinity of the high-salinity subtropical waters or an increase in the volume of these waters without a change in salinity, or some combination of both of these? Please clarify. [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	Accepted - text clarified
10-670	10	37	11	37	11	still valid for tropical Pacific and Atlantic SSS for 1950-2009 and 1970-2009, respectively using the ORE-SSS datasets [Laurent Terray, France]	Rejected - reference is used later
10-671	10	37	14	37	14	you can reference also our paper Terray et al. Jclimate 2012 where we show the projections for all CMIP3 models and the assessment of the pattern scaling hypothesis [Laurent Terray, France]	Rejected - text is about figure panel b, not c.
10-672	10	37	17	37	19	The positive correlation shows that ocean regions with currently high rainfall are becoming fresher and that the dry regions are becoming saltier.'	Accepted - text

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						The text here refers to the correlation discussed earlier in the para which is between mean surface salinity and temporal changes in surface salinity. Hence, the statement, as it stands, is an over-interpretation of the results presented as correlation with rainfall has yet to be discussed. To be accurate it should be replaced with: 'The positive correlation shows that ocean regions with currently low salinity are becoming fresher and that high salinity regions are becoming saltier.' If the statement is to be extended to include relationships with rainfall, then some discussion of relevant results on correlation between rainfall and temporal changes in surface salinity is needed. [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	
10-673	10	37	24	37	55	As shown in Fig. 10.13B, the amplification of the hydrological cycle suggested by surface salinity amplification is observed to be around twice the rate in CMIP3 simulations. The text states "The reasons for this difference is (sic) explained below". In the subsequent two paragraphs, the difference is not clearly explained, in my opinion. The explanation should be made explicit. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Accepted - text modified to make clearer deleted the offending text and added new text in the relevant paragraph.
10-674	10	37	26	37	36	replace Terray et al. 2011 (in press) with Terray et al. 2012 (the paper appeared in the last issue of J. Climate) [Laurent Terray, France]	Accepted - bibliography updated
10-675	10	37	30	37	30	Typo, "anthropogenic" (not "Anthropogenic). [Christian-D. Schoenwiese, Germany]	Accepted
10-676	10	37	37	37	55	The argument here seems to be somewhat contradictory to a previous argument about why we should not expect global precipitation rates to increase according to Clausius-Clapeyron (see lines 34-44, page 28). Also, the assessment here seems to be stronger than the assessment for precipitation (either over land or globally). I think this discrepancy needs to be addressed. The amount of insitu data supporting the salinity assessment is probably smaller than the amount supporting the precipitation assessment. There does seem to be greater consistency with models, but the chapter mentions only one D&A study on salinity change (Terray et al, and it's results would appear to be somewhat equivocal, with detection in some regions but not all. [Francis Zwiers, Canada]	Accept - the text has been modified to explain the difference between the land and ocean results. The amount of saility data is in some ways better than point weather stations, but it is true that a global time series is not easy to create. Ocean salinity is an integrator of all E-P changes, terrestrial measurements cannot do his.
10-677	10	37	38	37	40	The global models project changes (Figure 10.13a, upper panel) in the north-south variation of precipitation minus evaporation that broadly coincide with apparent freshwater fluxes inferred from the observed changes (Helm et al., 2010b). These estimates agree to within error estimates.' a.) As noted in Ch. 3.3 (p. 3-11, lines 15-20), in order to infer (surface) freshwater fluxes from observed salinity changes it is also necessary to consider the effects of poleward migration of isopycnals. Such an analysis has not yet been made. The discussion here thus needs to be made consistent with the conclusion in Ch3.3 on this point. Given the conclusion reached in Ch 3.3, my suggestion is that the upper and middle panels of Fig. 10.13a and associated text should be removed. b.) If, however, the upper and middle panels of Fig. 10.13a and associated discussion are retained then the text needs to be revised to state more clearly where the model changes in P-E agree with those 'inferred' from the observations. The top panel appears to show model-based changes that differ from zero by more than the specified error range only south of about 50 S. The observation 'inferred' P-E change differs from zero by more than the specified error range south of about 45 S and north of about 45 N. Thus, the conclusion reached from comparison of these two panels is that there is some agreement between model and observation inferred P-E changes south of 50 S (elsewhere the change is either not significantly different from zero in both cases or cannot be inferred). The text should therefore be modified to clearly state this point. The FOD text uses the term 'broadly coincide' which implies agreement in changes across a wide range of latitudes and is not supported by a rigorous examination of the two panels. [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	The middle panel was removed and the text has been changed to reflect the changes along with text that discuss within error bars… (the next draft will have a new panel based on newer results).
10-678	10	37	43	37	44	"an amplification of the oceanic hydrological cycle to be about 8 ± 5%": what means an "amplification" of the oceanic hydrologic cycle? Does it mean a growth of each component (e.g. evaporation, precipitation, …) by about 8 ± 5%? [Claudio Cassardo, Italy]	Accept - the text was clarified

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-679	10	37	48	37	48	"terrestrial stations": on land, or on islands? If on land, it does not matter too much as this section deals with ocean precipitation, and patterns may be different over land and sea. [Claudio Cassardo, Italy]	Accept - the text was clarified ot enable
10-680	10	37	54	37	55	yes and it supports comment number 1 [Laurent Terray, France]	Accept - reference added.
10-681	10	38	17	38	17	the 40 member CCSM3 was not used in the D&A study(they are used in the paper for other diagnostics), CMIP3 Control integrations were used instead to derive the two estimates of internal variability [Laurent Terray, France]	Accept - caption modified.
10-682	10	38	27	38	40	Comparing the two concluding sentences in these two paragraphs, which summarize the AR4 and AR5 views on attribution of sea level rise, it a big problematic. At first blush, there seems to be a big leap in certainty, but on second reading one realizes the sentences parse differently. Is this intentionally written to be cagey? Can the AR5 statement be made more comparable to the AR4? [Dian Seidel, USA]	To be determined
10-683	10	38	35	38	35	Fig 10.12a does not show sea levels [Gareth S Jones, UK]	Accept - reference to figure removed
10-684	10	38	42	38	51	Much of this is in Chpt 13- I don't think it adds much here [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	To be determined, and whether text removed.
10-685	10	38	49	38	49	"Small negative forcing from anthropogenic aerosols" is a bit of an understatement, is it not? Maybe cross-reference to Chapter 8. [Francis Zwiers, Canada]	Accept - small removed
10-686	10	38	53	39	7	This could probably be said in two sentences- Attempts to understand regional changes have led to different conclusions- detection on a reginal scale requires more sophisticated approaches than currently available [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accept - given state of science
10-687	10	38	59			Walker not walker [Larry Thomason, United States of America]	Accept
10-688	10	39	49	39	50	I don't really understand what this sentence is intended to say. It seems to suggest that detection and attribution can be undertaken without models. There is an extensive review paper that discusses the role of models in D&A (Hegerl and Zwiers, 2011), which is discussed in the chapter. It would be useful to cross-link to that discussion, and to discuss D&A here in the context of that discussion. [Francis Zwiers, Canada]	Account- text revised to conform
10-689	10	39	52	39	52	Rapid declining of Arctic sea ice extent/area or thickness was also studies by Zhang et al. (2008) and Polyakov et al. (2012). These two papers may be added in the citations in line 52. [Xiangdong Zhang, United States of America]	Accept Polyakov added
10-690	10	39	55			It seems too much to dwel on a single year, especially after the comments made in 10.3.3.4 [Ramon de Elia, Canada]	Account but the sentence is about the last 5 years
10-691	10	40	2	40	2	"… see Figures …(not "and Figures"). [Christian-D. Schoenwiese, Germany]	Editorial
10-692	10	40	6	40	6	Consistency of evidence must necessarily support detection and attribution assessments, so I don't see how this represents "another approach". [Francis Zwiers, Canada]	Account changed
10-693	10	40	17	40	17	The sensitivity of the Arctic doesn't necessarily mean higher signal to noise ratios or greater likelihood of detection and attribution of the influences of external forcing on the climate system. The mechanisms that lead to the high Arctic sensitivity presumably also lead to amplified natural internal variability. [Francis Zwiers, Canada]	account changed
10-694	10	40	17	40	18	This is a completely different definition of forcing to elsewhere in the chapter. Rewrite to distinguish direct impact of real forcings (BC, CO2, ozone) and indirect teleconnections from temperature rise elsewhere. I don't think the 'rise in global temperatures' can be a forcing in any sense. [Gavin Schmidt, USA]	Accept text revised
10-695	10	40	23	40	26	Again, the mechanisms were proposed by Manabe and Stouffer 1980 - JGR, 85, p5529-5554, [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Account We focus on recent references
10-696	10	40	30	40	30	The chapter should use the terms detection and attribution in a consistent way throughout. The word "attribution" seems to be being used in a different sense here (understanding mechanisms that link one source	Accept changed text

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of variability to another), than the sense in which it is used throughout the rest of the chapter (quantification of the contributions from external forcing to observed patterns of change). [Francis Zwiers, Canada]	
10-697	10	40	44	40	46	Clarify what is meant by "recent rapid decreases". Does this refer to a single year or the last 2-3 years, last 5 years or last 30 years? [Gareth S Jones, UK]	Accept changed text
10-698	10	40	44	40	46	This is an overstatement. There is no question that the bulk of the trend is anthropogenic even in the Kay et al results, the issue is whether there is a significant component of internal variability (which there is). This sentence implies there is it could all be internal, which is not supportable. [Gavin Schmidt, USA]	Accept changed text
10-699	10	40	54	40	54	typo, should be "Annular" mode [Michael Mann, USA]	Editorial
10-700	10	40	54	40	54	Typo, "Annular" (not "Annula"). [Christian-D. Schoenwiese, Germany]	Editorial
10-701	10	40	56			Is it appropriate to use submitted papers (as opposed to published or 'in press') [Larry Thomason, United States of America]	Account it is appropriate
10-702	10	41	2	41	4	This only relatively to the 30-40 past years, and not fr the early 20th century period. [Claudio Cassardo, Italy]	Noted
10-703	10	41	5	41	18	These issues are addressed in Chapters 11 and 12. [Thierry Fichefet, Belgium]	Noted
10-704	10	41	11	41	18	This seems to be off-topic for Chapter 10 (it deals with projections of future ice extent). [Francis Zwiers, Canada]	Account refers to contemporary conditions
10-705	10	41	21	41	30	For consistency with the other chapters it would be welcome to present model results based on the new RCD scenarios (instead of SRES). [Christian-D. Schoenwiese, Germany]	Accept Revised
10-706	10	41	32	41	34	This senstence appears inconsistent with chapter 9, page 36, line 38-40 and chapter 12 page 39, line 14-20 [Hugues Goosse, Belgium]	Accept Revised
10-707	10	41	32	41	39	In (Pavlova et al., 2011) it is shown that the multi-model ensemble mean of the 12 CMIP5 models almost excellently reproduces the observed trend of September Arctic sea-ice extent (See Figure 9.24, which is based on (Pavlova et al., 2011)). Pavlova, T. V., V. M. Kattsov, and V. A. Govorkova, 2011: Sea ice in CMIP5 models: closer to reality? Proceedings of Voeikov Main Geophysical Observatory, V. 564, P. 7-18 (in Russian). [Petr Sporyshev, Russian Federation]	Account changed text
10-708	10	42	2	42	4	See previous comments concerning the use of the word "significant". To avoid confusion, it would be best to avoid this word except in the statistical sense. If statistical significance is meant here, then "very likely ... is a significant contributor" seems like double talk since you're assigning a likelihood to something - the outcome of a statistical test - that is a direct description of the data (either a change is significant according to a defined criterion, or is it not, so assigning a likelihood qualifier seems inappropriate). [Francis Zwiers, Canada]	Accept
10-709	10	42	12	42	14	It is not clear what figure is being discussed here (figure numbering seems to be off for some figures). The Figure 14 that is provided in the figures file appears to be for the Arctic, not the Antarctic. [Francis Zwiers, Canada]	Account
10-710	10	42	16	42	38	Liu and Curry (2010) also underline the potentail role of precipitation changes to explain sea ice trends. Their work should be briefly discussed. [Hugues Goosse, Belgium]	Accept
10-711	10	42	25	42	31	This seems too strong for the result of a single study with a single model. Please change to the use of appropriate qualifiers e.g., "There has been only one study of the response of sea ice to stratospheric ozone depletion using a coupled AOGCM, and this work suggests a decrease rather than increase....." [Susan Solomon, USA]	Accept
10-712	10	42	26	42	31	Seems to be a problem with a reference on line 30. The discussion starts by describing Sigmond and Fyfe's simulation of decreasing Antarctic sea-ice extent in response to stratospheric ozone depletion, and then says that the same authors find sea-ice extent increase in an eddy-resolving model. Seems the latter must be due to another set of authors. [Francis Zwiers, Canada]	Accept removed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-713	10	42	30	42	31	References are definitely needed here for the sentence after 'but'. This sentence needs to be rephrased also. [Zhaomin Wang, UK]	Accept removed
10-714	10	42	40	42	48	The highest quality study shows that there has been no significant warming in East Antarctica, and barely significant warming in West Antarctica, over 1957-2006, and should be cited. O'Donnell, Lewis, McIntyre and Connon (2011): Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al (2009) Antarctic Temperature Reconstruction, J.Climate, vol.24, p2099-2115, DOI: 10.1175/2010JCLI3656.1 (of which I am one author). That study showed that the significant continental and regional Antarctica warming found by Steig et al. (2009) over 1957-2006 was an artefact of faulty mathematical methodology, and that with corrected methodology, using the same data, trends outside the peninsula were far lower. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accept Reference added
10-715	10	42	44			Sections 10.5.2. and 10.5.3: as far as I can oversee this field of research, an adequate reflection of current research. In several cryosphere research fields (especially in glacier and permafrost research) formal D&A studies are virtually non-existing (which is rather surprising). In 10.5.2.2 this is stated. It may also be stated for permafrost. Furthermore, in these impact fields, the methods described in section 10.2 are only marginally applied or taken as a reference. I'm wondering whether the section 10.2. should also be a guidance and reference for these cryosphere sections. [Christian Huggel, Switzerland]	Noted
10-716	10	42	46	42	46	Perhaps irreversible if strong forcing persists for a long period of time - but that would be a projection. I suggest inserting "potentially" ahead of irreversible, to avoid making the suggestion that changes observed to date are irreversible. [Francis Zwiers, Canada]	Accepted
10-717	10	42	52	42	52	The evidence doesn't show that the West Antarctic ice sheet is exhibiting apparent sensitivity to changes in ocean temperatures. Rather, changes in ocean currents, of unknown cause, have brought warmer water into increased contact with that ice sheet. There is no evidence that such warmer, sub-surface, water has been warmed by human influences. Further, there is evidence that the melting observed of West Antarctic ice shelves has been going on for many decades, perhaps over a century, which argues for a natural origin of whatever changes in currents or wind patterns have led to the influx of warmer water, not a human one. (Jenkins et al., 2010, Observations beneath Pine Island Glacier in West Antarctica and implications for its retreat, Nat Geosci.) [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accepted text change
10-718	10	42	54	43	6	The conclusions drawn in this period seem very strong for the relatively short length of the records used. This seems way over done to me and the arguments should be strengthened or the conclusions softened significantly. The following paragraph (43, 8-25) seems more balanced. [Larry Thomason, United States of America]	Account made a tone change for introduction
10-719	10	42	54	43	25	There might be too much focus on individual years (2010 and 2011). In the long run and in terms of climate, it is not very important what happened in a single year. However, I understand the interest of explaining the extreme melting of 2010 and 2011. [Borgar Aamaas, Norway]	Noted
10-720	10	43	3	43	6	Again, too strong, this time because the data are over a very short period. I don't think a statement of 'acceleration' in changes should be made based on only two decades of climatic data in AR5. Please drop the last sentence about 'it is fair to say'....since we do not have a long enough reoord for this to be a very robust remark. [Susan Solomon, USA]	Account While not a long record, recent Greenland changes are large; Need a balanced presentation
10-721	10	43	5	43	6	Greater than variability on what time scale - decade to decade? Based on what observations? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	accepted text changed
10-722	10	43	8	43	8	grammatical error "data FIT the conceptual model…" [Michael Mann, USA]	Editorial
10-723	10	43	18	43	19	Does "former" refer to larger internal atmospheric variability or low -altitude melting? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted changed text
10-724	10	43	40	43	48	O'Donnell et al (O'Donnell, Ryan, Nicholas Lewis, Steve McIntyre, Jeff Condon, 2011: Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al. (2009) Antarctic Temperature Reconstruction. J. Climate, 24, 2099–2115.) show that the calculations of Steig et al(2009) improperly spread warming observed in the Antarctic Peninsula to West and East Antarctica. The statement: "Mean surface temperature	Accepted Dropped text

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						trends in both West 47 and East Antarctica are weak positive for 1957–2006, and this warming trend is difficult to explain without 48 the radiative forcing associated with increasing greenhouse-gas concentrations (Steig et al., 2009)" is not supported. If Steig is referenced, O'Donnell should be referenced as well. [Stephen Gaalema, USA]	
10-725	10	43	40	43	48	There is a lot of other information and papers about Antarctic temperature trends. That shouldn't be assessed here, but rather in the observations chapter. Steig et al. did not do a complete attribution study. Delete this here, it doesn't belong in a cryosphere section anyway and is dealt with elsewhere. [Susan Solomon, USA]	Accepted, text removed
10-726	10	43	45	43	46	SAM also needs to be invoked to explain the warming in continental west Antarctica. Associated with the upward trend in SAM index, there is deepening of Amundsen Low, which causes more warm air advection on the eastern side of Amundsen Low and possiblly more warm advection in the ocean as well. (see comment 60 also) [Zhaomin Wang, UK]	Accepted, text removed
10-727	10	43	46	43	48	The claim that East Antarctica has also warmed has been refuted by O'Donnell, Ryan, Nicholas Lewis, Steve McIntyre, Jeff Condon, 2011: Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al. (2009) Antarctic Temperature Reconstruction. J. Climate, 24, 2099–2115. This is another controversial topic that requires an objective and careful treatment by IPCC. [Marcel Crok, The Netherlands]	Accepted, text removed
10-728	10	43	47	43	47	grammatical error "are WEAKLY positive…" [Michael Mann, USA]	Accepted, text removed
10-729	10	43	50			Section 10.5.2.2: Please use the term "glaciers" instead of "mountain glaciers" to be consistent with Ch. 4 and 10. [Frank PAUL, Switzerland]	Accepted
10-730	10	43	52	43	55	You should perhaps mention that dynamical processes within glaciers are affecting glacier length regardless of climate variability and climate change, such as surging. For surging glaciers, a slow retreat of the terminus is expected through most of its life. [Borgar Aamaas, Norway]	Accepted text changed
10-731	10	43	52	44	13	I recall (but am not sure) a big to-do regarding some claims (perhaps inaccurate or erroneous) about glaciers in AR4. Shouldn't these be addressed forthrightly in this section or is it elsewhere in the AR5 document? I may be wrong on this…. [Larry Thomason, United States of America]	noted
10-732	10	43	55	43	56	I'm not sure whether the reason for only few formal D&A studies in glacier reserch is primarily due to contrasting scales. Rather, I think, the reason is that research traditionally takes the reverse approach: assessing the impact of climate (change) on glaciers, rather than attributing glacier changes to a change in climate variables. Most glaciologists are unfamiliar with D&A concepts.The problem of scales is definitely an important one in this regard but I don't think it is the reason for having so little D&A studies in glacier research (I believe that the Reicher et al 2002 paper is still the only formal D&A study for recent glacier changes). [Christian Huggel, Switzerland]	noted
10-733	10	44	5	44	7	Doesn't seem to be a sentence - Perhaps "That is, they use local and regional…."? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted changed text
10-734	10	44	15			Sec 10.5.3, this section has missed the work of Andreas Roesch in the JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 111, D15111, 18 PP., 2006 "Evaluation of surface albedo and snow cover in AR4 coupled climate models". He found a correlated positive surface albedo bias among all the AR4 models that he was able to globally and annually average. He attributed the bias mainly to spring snow melt and snow cover fraction at high lattitudes. When downward shortwave ratio is applied to the bias it amounts to more than 3W/m^2. The implications of this easily quantified correlated error should be considered throughout the chapter. [Martin Lewitt, United States of America]	accepted reference added
10-735	10	44	17	44	17	"Satellite measurement of annual snow cover extent over the Northern Hemisphere has substantially decreased": --> "Satellite measurement show that the 20th century, …" [Claudio Cassardo, Italy]	accepted
10-736	10	44	17	44	18	Not satellite measurement but snow cover has decreased. [Christian-D. Schoenwiese, Germany]	Accepted
10-737	10	44	17	44	18	I believe it is the snow that has substantially decreased, not the satellite measurement. Please correct the	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						English here. [Susan Solomon, USA]	
10-738	10	44	29	44	29	"…studies …" (not "study"). [Christian-D. Schoenwiese, Germany]	Editorial
10-739	10	44	29	44	30	Are there formal detection and attribution studies other than the Pierce et al study? If so, they should be discussed and assessed. [Francis Zwiers, Canada]	Account extensive text is sufficient as is
10-740	10	44	38	44	48	The paragraph is a adequate and concise description/assessment of permafrost studies as related to D&A. Formal D&A studies have not yet been done (I believe) and this could be mentioned. Also, the paragraph is not considering mountain permafrost. There are'nt any D&A studies either for mountain permafrost but a couple of sentences should be added on recent development in mountain permafrost (check with chapter 4). [Christian Huggel, Switzerland]	noted
10-741	10	44	38	44	48	Surface temperature and snow amount from the CMIP3 models output were used as input parameters for a permafrost model in (Pavlova et al., 2007). It was shown that the simulated permafrost boundaries are in reasonable agreement with observational estimates. It was also shown that the multi-model ensemble mean trends of seasonal soil freezing and thawing depths in Northern Eurasia are consistent with observations. Pavlova T.V., V.M. Kattsov, E.D. Nadyozhina, P.V. Sporyshev, V.A.Govorkova, 2007: Terrestrial cryosphere evolution through the 20th and 21st centuries as simulated with the new generation of global climate models. Kriosfera Zemli (Earth Cryosphere), V. 11, No. 2, P. 3-13 (in Russian). [Petr Sporyshev, Russian Federation]	Accepted reference added
10-742	10	44	39			Units for the trends here are deg C per year, but elsewhere are usually deg C per decade. Try to be consistent in the trend units. [David Karoly, Australia]	Accepted, consistency has been improved.\
10-743	10	44	42	44	44	Does Arzhanov provide an estimate of change in permafrost temperature that can be compared with the values reported earlier in the paragraph? If not, is it useful to mention this paper in the context of a detection and attribution chapter? [Francis Zwiers, Canada]	Taken into account
10-744	10	44	46	44	46	seems to be missing word, "…in A stronger snow insulation effect…" [Michael Mann, USA]	Editorial
10-745	10	44	52			May manifest. This chapter is partly about whether climate change is occurring, which makes this unconditional statement seem out of place. [Dáithí Stone, United States of America]	Agree, text edited.
10-746	10	44		44		It would be valuable to add a summary of the findings for Section 10.6. [Lena M. Tallaksen, Norway]	Agree, Summary is now added
10-747	10	45	4			Section 10.6.1 It would be really useful to have a discussion defining "extremes" compared with "extreme weather events". Daily maximum temperatures, number of days with daily minimum temperatures below a threshold are all extremes in this section, but later on in S10.6.2 climate events are said to be "extreme weather events". To many laypeople the latter are really extremes but not the former. With the growing interest in "extreme weather events" we have to be somewhat more careful about the language used. [Gareth S Jones, UK]	Noted. Chapter 2 discusses the definition of extremes. We have also added one line in section 10.6.1, indicating extremes discussed in 10.6.1 are more moderate compared with events discussed in 10.6.2
10-748	10	45	4			Section 10.6.1.: This Section does not always consistently refer to the SREX (2012) assessment, there are for instance no references to that report in Sections 10.6.1.1 and 10.6.1.2, although it is referred to in Sections 10.6.1.3, 10.6.1.4 and 10.6.1.5. [Sonia Seneviratne, Switzerland]	Noted, SREX is now referenced more consistently.
10-749	10	45	9	47	51	It doesn't seem like sections 10.6.1.1 and 10.6.1.2 use the AR5 certainty formulation in a consistent waay [Larry Thomason, United States of America]	Noted. Language modified
10-750	10	45	11	46	13	I found this to be a well-articulated summary of recent work on temperature extremes. I have one request, pertaining to the last sentence of this section regarding the scaling factors needed to fit model and observed magnitudes. Is the implication that the mode data, which is subjected to a scaling greater than 1 for cold extremes but less than 1 for warm extremes, simulates greater mean warming than observed? [Martin Hoerling, USA]	Noted. Models do not necessarily simulate greater mean warming.
10-751	10	45	19	45	20	This sentence might read more easily if it started " On examining the change in frequency of rare seasonal mean….." [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Noted, text edited.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-752	10	45	19			What is the message of this sentence? Why does "examining" suggest human influence? [Albert Klein Tan k, Netherlands]	Noted, text edited.
10-753	10	45	28			Note more upfront that nearly all studies here refer to modest extremes, which are extremes rather far away from the values in the tails of the distributions which usually cause the strong impacts [Albert Klein Tan k, Netherlands]	Noted. Text modified in the lead paragraph of 10.6.1
10-754	10	45	42			Refer to Ch2, Section 2.7 [Albert Klein Tan k, Netherlands]	Noted, text edited.
10-755	10	45	46	45	47	I wasn't sure I understood this- does it mean that TN90 variations are well correlated with variations in mean temperature - ie TN90 and mean temperature are correlated [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Text edited to be clearer
10-756	10	45	49	45	50	It would be nice if this figure could be multi-model rather than (I assume) providing results from single models, with different models used for intensity and frequency. [Francis Zwiers, Canada]	The figure does include data from both CanESM2 and HadGEM1 (there was an error in the figure caption which implied it was just CanESM2, caption has been revised).
10-757	10	45	54	45	56	This could be a good place to define the use of the terms "ALL" + "ANT" throughout the rest of the chapter and clarify they are shorthand terms not meant to imply absolutly all known forcing factors are looked at. [Gareth S Jones, UK]	Noted, the term "ALL" and "ANT" are removed from the main text.
10-758	10	45				At the end of line 26 may add: “Xu et al. (2010) reported that the annual and seasonal maximum temperatures are increasing significantly in Blue Nile region, while the annual minimum temperature and minimum temperature in dry seasons are decreasing. The difference between maximum and minimum temperature is increasing in all the seasons. Net solar radiation in the region shows a significant increasing trend in all seasons, which corresponds well with the changes of maximum temperature.” Xu, C-Y, Zhang, Q., M. El Hag El Tahir, Zhang, Z., 2010. Statistical properties of the temperature, relative humidity and net solar radiation in the Blue Nile-Eastern Sudan region. Theoretical and Applied Climatology, 101:397–409. [Chong-Yu Xu, Norway]	Noted, but the paper is not relevant to the context fo this section.
10-759	10	46	9			True, but likewise the distribution of all daily maxima in a year may not describe the extremes in an adequate way. [Albert Klein Tan k, Netherlands]	Noted, text edited.
10-760	10	46	10	46	11	I disagree with that statement that the annual extreme is poorly sampled. A robust body of statistical theorey, and extensive practical experience from a multiplicity of applications, argues otherwise. The words seem to imply that Christidis et all fitted the full distribution. Doing so does not necessarily lead to a good representation of the deep tails - in fact, this is one of the justifications for extreme value theory. In fact, Christidis et al used the peaks over threshold approach which leads to the Generalized Pareto distribution). There is discussion that this leads to improved use of data (by modelling more than one peak per year), but there are also issues that need to be dealt with, such as the declustering of observed extremes, the choice of the appropriate threshold, etc. Overall, the jury as to which approach is better remains open. If you dismiss work that is done on the basis of the block-maximum (annual maximum) approach with the word "poorly", then you dismiss a large fraction of the literature and many practical engineering applications. [Francis Zwiers, Canada]	Noted, text edited to reflect the two approaches being complementary.
10-761	10	46	27	46	30	Agree with the "high confidence" statement regardingextreme temperature changes. I recommend a reconsideration of wording of the ending part of this sentence which again rings as being deterministic, namely that "increasing frequency of warm days and nights and a reducing frequency of cold days and nights is attributable to human influence". Is the time series of these extreme event statistics wholly determined by the time series of anthropogenic GHG forcing? Is there be zero contribution from natural forcing, or natural inernal variability, as the sentence implies? If the answer to the latter question is no, then revise the sentence to read "....primarily attributable to human inflence". [Martin Hoerling, USA]	Agreed, text edited.
10-762	10	46	32	46	32	The paper Stephens, G. L., T. L’Ecuyer, R. Forbes, A. Gettlemen, J.-C. Golaz, A. Bodas-Salcedo, K. Suzuki,	Noted. This paper is of relevance to Chapter 9 :

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						P. Gabriel, and J. Haynes (2010), Dreary state of precipitation in global models, J. Geophys. Res., 115, D24211, doi:10.1029/2010JD014532 is relevant here as well. Extreme precipitation in models is far lower than in reality. Why would I believe an increase in a probability density function of extreme rainfall if the absolute rainfall is so far away from reality? [Marcel Crok, The Netherlands]	Evaluation of Climate Models. The attribution assessment here takes account of modelling and observational uncertainties
10-763	10	46	32	46	32	It would be useful to provide a definition of what constitutes a precipitation extreme. [Francis Zwiers, Canada]	Edits have been made to the start of the section discussing what is meant by extremes.
10-764	10	46	39	46	40	Is the "more evidence" of a type to modify the AR4 conclusion which had stated that "is is more likely than not" that anthropgenic influences contributed to global trends toward increases in the frquency of heavy pcpn events in the second half of the 20th Century? If so, state so here. Also, would the revision (if there is one) be a function of having addiitonal "data knowledge", namely 2000-2010? Would the revision (if there is one) be a function of having re-analyzed the data during the second half of the 20th Century, and aplpied new more sophisticated D&A methods? Would the revision (if there is one) be a function of new climate simulations that have altered the pattern of expected responses in heavy precipitation to anhropogenic forcing? What are other factors? This needs to be clearly articulated, and similar approaches need to be applied for other re-assessments/revisions/updates to the AR4 confience and uncertainty language related to the attribution of changes in frequency/occurrence and intensity of extremes. [Martin Hoerling, USA]	Noted. This sentence has been deleted. Instead the overall assessment is made at the end of this sub section drawing together the evidence and comparing with the SREX report.
10-765	10	46	48	46	53	The CC-Relation is invoked, in what appears as a 2-step attribution, for the behavior of precipitation extremes. Of cource this is by no means new knowledge, and so taken alone would does not support an assessment of increased confidence or reduced uncertainty regarding the observed behavior of precipitation extremes. New observational analysis, since the AR4 (e.g. Simmons 2010) indicates no increase in atmospheric water vapor (over land areas) since 1998. The authors need to integrate this fact with the 2-step attribution arguement of how upward trends in heavy precipitation may be related to anthropogenic forcing. [Martin Hoerling, USA]	Noted. Note that this assessment does not suggest stronger confidence than in AR4. This paragraph has been substantially revised.
10-766	10	46	49	46	49	I presume this means the constraint deduced from the CC equation is better understood, not the CC equation itself. Clarify [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Yes, noted. Sentence deleted in revision.
10-767	10	46	50	46	53	"The thermodynamic constraint is a good predictor for extreme precipitation changes in a warmer world where the circulation changes little (Pall et al, 2007),..." In [Hardwick-Jones, R., S. Westra, and A. Sharma (2010), Observed relationships between extreme sub-daily precipitation, surface temperature and relative humidity, Geophysical Research Letters, 37(L22805).] we found the importance of moisture availability in constraining extremes. This is different from any changes in circulation. In particular, it is concluded in Chapter 12 that near surface relative humidity over land is 'likely' to decrease in many parts of the world, whereas the Clausius-Clapeyron scaling hypothesis is predicated on a constant relative humidity. [Seth Westra, Australia]	Noted. We have edited text to reflect that C-C relation works under the assumption of constant relative humidity. We also added Jones et al. 2010 to the references indicating that the scaling also depeneds on tempertaure.
10-768	10	46	53	46	53	A reference, Chou et al. (2009, J. Climate, 1982-2005), can be added here. [Chia Chou, Taiwan, ROC]	Noted, reference added
10-769	10	46	53	46	57	There are other studies that report precip sensitivity, including Kharin et al (2007, J Climate) who provide a multimodel intercomparison. [Francis Zwiers, Canada]	Noted, text edited.
10-770	10	46	56	47	14	A recent publication, Chou et al. (2012, in press (Chou, Chia, Chao-An Chen, Pei-Hua Tan and Kwan-Ting Chen, 2012: Mechanisms for global warming impacts on precipitation frequency and intensity. J. Climate , doi:10.1175/JCLI-D-11-00239.1)), can be added here. This study discusses not only changes in precipitation frequency and intensity, but also mechanisms for these changes. [Chia Chou, Taiwan, ROC]	Noted, reference added
10-771	10	46				at the end of line 13 add: "In the study of changes of climate extremes in a typical arid zone (the Tarim River Basin) in Central Asia, Yang et al. (2011) reported that most warm (cold) extreme temperature indices have shown significantly positive (negative) trends in the Tarim River Basin in past five decades. Ensemble of five CGCM models in Phase 3 of the Coupled Model Intercomparison Project (CMIP3) based on the BMA method suggests that the increasing consecutive dry days (CDD), together with the decreasing frost day (FD) and increasing warm nights frequency (TN90) may lead to more frequent droughts in Tarim in future." The reference for the added text is: Yang, T., X. Wang, C. Zhao, X. Chen, Z. Yu, Q. Shao, C-Y. Xu, J. Xia, and W. Wang: 2011. Changes of climate extremes in a typical arid zone: Observations and multimodel ensemble projections, J. Geophys. Res., 116, D19106, doi:10.1029/2010JD015192. [Chong-Yu Xu, Norway]	Noted, but the papers suggested here are not relevant

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-772	10	47	1	47	3	Revise sentence to read "….could exceed (or fall behind) moisure content increases due to changes in vertical motion…" [Martin Hoerling, USA]	Rejected.The paper only discussed "exceed".
10-773	10	47	3	47	3	It should be "Sygiyama", not "Shiogama". [Chia Chou, Taiwan, ROC]	Noted, reference corrected
10-774	10	47	3	47	3	(Shiogama et al. 2010) may be (Sugiyama et al. 2010) Sugiyama, M., H. Shiogama, and S. Emori, 2010: Precipitation extreme changes exceeding moisture content increases in MIROC and IPCC climate models. Proceedings of the National Academy of Sciences of the United States of America, 107(2), 571-575. [Hideo Shiogama, Japan]	Noted, reference corrected
10-775	10	47	3	47	5	"Elsewhere, dynamical changes could lead to precipitation extremes less than expected from simple thermodynamics, which may explain why there have not been increases in precipitation extremes everywhere, although low signal to noise ratio may also play a role." The paper by [Westra, S., and S. A. Sisson (2011), Detection of non-stationarity in precipitation extremes using a max-stable process model, Journal of Hydrology, 406, 119-128.] explicitly accounted for the signal-to-noise ratio, and found that for daily or longer duration precipitation there was very little evidence that Australian precipitation increased in line with thermodynamics. In contrast, much shorter duration precipitation seemed to increase at or exceeding thermodynamic scaling rates. Two conclusions can be made in the context of this paper: (1) it is necessary to distinguish between different timescales when discussing whether extreme precipitation is likely to change; and (2) that signal to noise ratios can be explicitly accounted for through confidence intervals, to allow for more formal approaches to hypothesis testing. [Seth Westra, Australia]	Noted, reference added
10-776	10	47	4	47	5	I agree that circulation could play a role. That does not preclude the possibility that thermodynamic changes have increased the likelihood of extreme events in most places - even if increases have not been observed everywhere. [Francis Zwiers, Canada]	Noted, but there is no conflct between what is purposed here and what was in the text.
10-777	10	47	8	47	11	The Allan & Soden result has effectively been retracted by Allan & al (2010): I suggest deleting the whole sentence [William Ingram, UK]	Noted, text deleted
10-778	10	47	16	47	19	2 of the 4 papers cited are "new" since AR4. And, it is worth noting that the results of Zhang et al. (2007) emphasize the disagreement between OBS and model simulated trends in zonal mean precipitation in the latitude band of 30N-50N, which is also the zone where other studies have suggested increases in intensity of heavy precipitation (e.g. Min et al. 2011). There is also the difficulty in understanding the modeling results in Min et al, namley that the ALL-Forcing runs do not detect a change in preicpitation for the NH, though the Anthro-Forcing runs alone do. Barring a sound physical explanation as to how the NAT-Forcing could have such a significant (and apparently opposite-signed to GHG) impact on the 1951-1999 pcpn trends, the Min et al. findings need to be given low confidence. And, the studies of Zhang and Min have focused on data only through the end of the 20th Century. The authors need to indicate more completely the limitations of these analyses, and the open question as to whether some trends in precipitation, that may have been detected for data through the late 20th Century, have in fact continued in the subsequent decade of observations to 2010. [Martin Hoerling, USA]	Noted, text is modified. 1) one additional reference on seasonal trend precipitation is added. 2) assessment based on Min is softened, stating clearly that Min detected ANT more robustly than ALL.
10-779	10	47	19	47	19	"Since the variability of precipitation is related to the mean": it is intended the mean value of the precipitation itself? [Claudio Cassardo, Italy]	Noted, text is modified with reasoning.
10-780	10	47	19	47	22	What is the reference for this statement about variabillity of precip being linked to the mean? [Susan Solomon, USA]	Noted, text is modified with reasoning.
10-781	10	47	24	47	24	A..model analysis with .. simulations shows that…. [Helga Nitsche, Germany]	Noted, text edited.
10-782	10	47	26	47	26	missing word? …attribution study that <?compared?> observed and.. [Helga Nitsche, Germany]	Noted , text edited
10-783	10	47	26	47	29	Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think these 2 sentences should be removed. (There is no Fig 10.20) [William Ingram, UK]	Noted. Text edited to state what was found in Min (e.g. more robust detection for ANT than ALL), and to not to highlight the paper by removing the figure.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-784	10	47	30	47	30	to correct the figure numer: 10.16, not 10.20, see line no 38 and 39 below. [Helga Nitsche, Germany]	Not applicable anymore, the figure has been removed.
10-785	10	47	34	47	36	...increased the likelihood of events 'similar to' the August 2000 floods in the UK ... [Larry Thomason, United States of America]	Rejected. The event attribution is about attributing "the event" to possible causes. Therefore, it is unknow if that can be extended to "similar" events.
10-786	10	47	38	47	47	Fig. 10.16 is never recalled in the text in this section (it is recalled at page 28). [Claudio Cassardo, Italy]	This figure is removed.
10-787	10	47	49	47	51	I think these conclusions do not summarize exactly the previous discussions. On a global scale, there is some evidence of the influence of anthropic activities on the increased number of extreme precipitation events, while at smaller scales this evidence becomes progressively less clear. [Claudio Cassardo, Italy]	Agree, text modified to reflect that is at the global scale.
10-788	10	47	49	47	51	The authors must reconcile their use of "confidence language" with the AR4 use of "quantified measure of uncertainty language". Here one reads, what appears to be a synthesis statement, that there "is medium confidence that anthropogenic forcing has contributed to a trend towards increases in the frquency of heavy pcpn events over the second half of the 20th Century" On pg 10-46, lines 34-40 the reader is told that AR4 conlcuded that "it is more likely than not that anthropogenic influence had contributed to a global trend towards increases in the frquency of heavy precipitation events over the second half of the 20th century". Please use clear language to indicate whether or not the state of being certain regarding causes for trends in heavy precipitation has changed. [Martin Hoerling, USA]	Noted. But note that there is no direct comparson between AR4 and AR5 because of the changes in the uncertainty language implemented after the AR4.
10-789	10	47	49	47	51	Again, "heavy precipitation events" basically refers to modest events which occur every year and may not be representative for the extreme events linked to the impacts [Albert Klein Tan k, Netherlands]	Not applicable anymore, text has been modified.
10-790	10	47	49	47	51	The sentence near the beginning suggests that a stronger assessment (or at least not weaker) would be possible based on more evidence, etc. See lines 39-40. But this appears to be a weaker assessment (medium confidence, and no attempt to qualify likelihood). I'm fine with a weaker assessment than given in the AR4 (which did give an estimate of likelihood as >50%) - but if that is the view of the chapter, then the text on page 10-46 needs to say something a bit different and an explanation for the weakened assessment should be given. Reference to the SREX would also be helpful here. [Francis Zwiers, Canada]	Noted. Sentence at start of subsection (page 46 lines 39-40) has been deleted.
10-791	10	47	53	48	44	If this chapter is meant only to include meteorological drought this needs to be clearly stated. If hydrological drought is to be inlcuded, more references to studies on e.g. low flow indices need to be included. [Hege Hisdal, Norway]	Noted. The drought is not limited to meteorological drought, however, the lack of mentioning causes of low flow changes is due to the lack of literatures.
10-792	10	47		47		Similar to elsewhere in the chapter, more references to studies on runoff should be included. Studies on hydrological extremes (e.g. low flow indices) could be added (refer comments and references provided to Chapter 2). [Lena M. Tallaksen, Norway]	noted, see response to above comment
10-793	10	48	5	48	7	"The difference in the use of “more likely than not” and ”medium confidence” in the two assessments is due to the implementation of new IPCC uncertainty guidance note": this means that the two statements are absolutely equivalent? [Claudio Cassardo, Italy]	This is not the case. Sentence has been deleted.
10-794	10	48	5	48	7	This is an invalid statement. It is correct that early versions of the SREX chapter 3 mentioned that that chapter considered the terms "medium confidence" and "more likely than not" as equivalent (as they both provide only a direction of change). But following reviewer comments and internal chapter discussions, the chapter team came to the conclusion that these terms were not equivalent within the new uncertainty guidance ("more likely than not" referring to a case with high-quality evidence but low signal-to-noise ratio, while "medium confidence" specifically indicates some uncertainty in the evidence as underlying cause for the lack of precision of the assessment). Hence, the respective sentence on this point was removed from the chapter. We cannot exclude that AR4 authors also included aspects related to the quality of evidence in providing a "more likely than not" assessment, however, this is impossible to determine a posteriori. The text should state instead that the use of the new uncertainty guidance makes the direct comparison of AR4 and post-AR4 assessments difficult in some cases, as noted in the IPCC SREX SPM. [Sonia Seneviratne, Switzerland]	Agree. Text edited: 1) state the assessment in the released version of the SREX, 2) delete the comparison between AR4 and SREX assessments.
10-795	10	48	5	48	7	I don't think this is true - medium confidence is a different assessment. In the AR4 it was very clear that more likely than not meant "better than even odds", and in my mind, that was the assessment we were making.	Agree. Text deleted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Medium confidence does not provide a probabilistic assessment. I'm fine with a different assessment - but not with mis-characterization of the uncertainty language. [Francis Zwiers, Canada]	
10-796	10	48	10	48	11	Replace "non-climate conditions" with "non-atmospheric conditions"; soil moisture and land surface conditions are an inherent part of the climate system. [Sonia Seneviratne, Switzerland]	Agree, text edited.
10-797	10	48	25	48	28	these are 2 examples of 2 studies with different findings in different regions - it does not mean that these are the main or only reasons for changes in drought conditions in these regions? [Helga Nitsche, Germany]	yes.
10-798	10	48	26	48	26	wrong typing: Australia [Helga Nitsche, Germany]	corrected
10-799	10	48	27	48	28	Unclear what is being said….revise. It is particualrly unclear what is meant by "consistent with observations"....what is consistent with observations? . Modelling studies indicate that drought indices over certain regions of the US are sensitive to SST variability, as inferred by output from AMIP runs. And, some aspects of the temporal variability of observed drought indices are simulated in such AMIP runs that span the 20th Century (refer to Hoerling, M., X. Quan, and J. Eischeid: 2009: Distinct causes for principal US droughts of the 20th Century. Geophys. Res. Lett., 36). However, the magnitude of observed drought variations is considerably weaker than the SST0forced signals estimated from AMIP runs. This is likely a consequence of the appreciable random (internal) variation of climate that generate US drought, though model biases cannot be discounted. A better articulation of the known impact of SSTs on US drought is required. [Martin Hoerling, USA]	noted, the line is deleted.
10-800	10	48	31	48	31	"change in real time" is jargon … best to avoid. [Francis Zwiers, Canada]	agree. Text deleted
10-801	10	48	31			Why do we need to know this in real time? [Albert Klein Tan k, Netherlands]	noted, text deleted
10-802	10	48	41	48	42	This is inconsistent with the assessment of observed changes in the observations of drought presented in Ch2 (see page 2-5). [Albert Klein Tan k, Netherlands]	Agree. The text has been modified.
10-803	10	48	44	49	17	The link between storms and extreme ocean surface waves could be further explored. Extreme wave events are directly related to the storms and have huge impacts on coastal regions. [Eduardo Siegle, Brazil]	Literature on wave heights is assessed.
10-804	10	48	46	48	51	Please indicate over which period of record these poleward shifts have been observed. Also, if the poleward shift in the NH is asociated with changes in the NAM as indicated, but given that more recent data (since 1999) which indicates that the prior trend in the phase of the NAM to a positve polarity has appreciable weakened, then is it still true that the storm tracks in the NH have shifted poleward? [Martin Hoerling, USA]	agree. Text edietd. NOTE that We need to COORDINATE WITh Ch2 on this
10-805	10	48	55	49	1	Please indicate over which region the SST increases were uniform? Please state, also, that this is an idelaization of the actual change in observed SST patterns. In the spirit of providing useful information, please remove the sentence beginning "Strengthened SST gradients near the subtropical jet may lead to a meridional shift in the storm track either towards the pole or equator..." [Martin Hoerling, USA]	agree. Text edited to improve clarity.
10-806	10	48				At the end of line 39 may add the following: “Zhang et al. (2009) reported that the Pearl River basin in south China tends to be dryer in the rainy season and comes to be wetter in winter. However, no fixed relationships can be observed between moisture content changes and number of wet months in the rainy season, indicating that more than one factor can influence the dry or wet conditions of the study region.” Zhang, Q., Xu, C-Y, Zhang, Z.X., 2009. Observed changes of drought/wetness episodes in the Pearl River basin, China, using the Standardized Precipitation Index and Aridity Index. Theoretical and Applied Climatology, 98, 89-99. [Chong-Yu Xu, Norway]	noted, but space limitation does not allow the inclusion of all published papers.
10-807	10	49	19	49	19	The phenomenon is "TROPICAL Cyclones" not "Tropic Cyclones" [Michael Mann, USA]	Noted, text edited.
10-808	10	49	19	49	19	Tropical [Gavin Schmidt, USA]	Noted, text edited.
10-809	10	49	19	49	19	Probably "Tropical Cyclones" is meant here rather than "Tropic Cyclones" [Sonia Seneviratne, Switzerland]	Noted, text edited.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-810	10	49	19	49	31	I like the manner in which the authors begin each subsection with a synthesis of the AR4 and SREX (2012) assessments of the phenomenon in question. This one, regarding tropcial cyclones, is particularly clear. [Martin Hoerling, USA]	thanks
10-811	10	49	21	49	22	Continuing on the theme of my other 2 comments noting that the discussion mixes up past assessments, it might be useful to note here that the IPCC AR4 statement is not actually claiming that there is a detectable anthropogenic influence as far as I can tell. Literally, their statement could be interpreted to mean that there is only a miniscule anthropogenic influence on tropical cyclone activity. The fact that most readers would not interpret it that way reflects a problem of vagueness. Therefore I strongly urge that any statements about changes in tropical cyclone activity be explicit about whether it is claimed to be highly unusual compared with expected levels of natural variability or not. An example of such as statement would be that of Knutson et al (2010): "Therefore, it remains uncertain whether past changes in tropical cyclone activity have exceeded the variability expected from natural causes." This leaves much less room for misinterpretation. [Thomas Knutson, U.S.A.]	Noted. The citation was from the AR4. In this assessment, we will make the language as clear as possible.
10-812	10	49	21	49	57	cross reference Box 14.3 and Section 11.4.2.5.3. [George Kiladis, USA]	Noted, but the comment does not seem to be relevant for Chapter 10.
10-813	10	49	27	49	31	This discussion mixes up the history of assessments on this topic. Let me clear up what the CCSP 3.3 said, and (in another comment following it) what the Knutson et al (2010)/WMO expert team concluded. CCSP's finding (on p. 81 of CCSP3.3, ref. Gutowski et al) was: It is very likely that the human-induced increase in greenhouse gases has contributed to the increase in sea surface temperatures in the hurricane formation regions. Over the past 50 years there has been a strong statistical connection between tropical Atlantic sea surface temperatures and Atlantic hurricane activity as measured by the Power Dissipation Index (which combines storm intensity, duration, and frequency). This evidence suggests a human contribution to recent hurricane activity. However, a confident assessment of human influence on hurricanes will require further studies using models and observations, with emphasis on distinguishing natural from human-induced changes in hurricane activity through their influence on factors such as historical sea surface temperatures, wind shear, and atmospheric vertical stability." [Thomas Knutson, U.S.A.]	Agree, text edited.
10-814	10	49	27	49	31	Again, this discussion mixes up the history of prior assessments actually concluded. Here is the main conclusion of Knutson et al. (2010)/ WMO expert team on TC/climate change detection: " Therefore, it remains uncertain whether past changes in tropical cyclone activity have exceeded the variability expected from natural causes." [Thomas Knutson, U.S.A.]	agree, text edited.
10-815	10	49	46	49	49	The claim that changes in potential intensity are governed by "relative SST" comes largely from one particular group (GFDL) and is contested, on theoretical grounds, by others such as Emanuel. See e.g. Emanuel, K. (2010). Stratospheric cooling and tropical cyclones. 29th Conference on Hurricanes and Tropical Meteorology [Available: http://ams.confex.com/ams/29Hurricanes/techprogram/paper_168302.htm]. The contested nature of this argument must be reflected in any balanced assessment. [Michael Mann, USA]	noted, however, we would only include peer-reviewed publications
10-816	10	50	3	50	3	repeated reference to Seneviratne et al (see page 49, line 57) [Helga Nitsche, Germany]	noted, extra lines deleted.
10-817	10	50	3			Such comparisons with SREX have not been made systematically in all sections of 10.6 [Albert Klein Tan k, Netherlands]	Noted, comparison with SREX is now made systematically in all subsections of 10.6.
10-818	10	50	7	53	4	I don't want to sound like a Luddite or anything but I am uncomfortable with the tone in this section. It seems to me (and I am nothing if not cautious) that there is a big difference between saying that extreme events like the 2000 UK floods are definitely due to climate change than that such events are 2 or 5 times more likely (a point made in previous sections and with which I am totally comfortable). That is a big difference. It is about like saying that the Jan/Feb 2010 snowmageddon event in DC was proof of no global warming (claims made by real Luddites). While this section doesn't make categorical statements regarding attribution, it edges way too close to that edge for my comfort. Please consider how this section is constructed; make use of the AR5 certainty protocol. Maybe something more straightforward would be something on the order of 'the likelihood of this event occuring without climate change is XXX whereas it is XXX with those forcings included' or similar would helpful. [Larry Thomason, United States of America]	Noted: Nowhere is it said that an individual weather event is definitely due to climate change. The problem with the XXXs is that absolute probabilities are harder to quantify than changes

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-819	10	50	16	50	16	Change beginning of sentence to "Two alternate approaches have been used to quantify and communicate ….". The approaches might be viewed as distinct, but they are also complementary. In making their assessments, both methods also rely in part on similar estimates, e.g., of mean changes (together with uncertainties) due to external forcing. [Randall Dole, United States of America]	Accepted: paragraph will be clarified
10-820	10	50	16	50	17	Sentence needs a re-write to propoerly reflect current science-based methods. I suggest "Two distinct approaches have been used for quantifying and communicating the causes of extreme weather events" [Martin Hoerling, USA]	Accepted: paragraph will be clarified
10-821	10	50	20	50	21	Sentence needs a re-write. "Other studies (Perlwitz et al. 2009, Dole et al. 2011) considered how different physical factors contributed to the magnitude of the event, or more specifically, how forcing may have increased the magnitude of an event of a given occurrence-probability" [Martin Hoerling, USA]	Accepted: paragraph will be clarified
10-822	10	50	20	50	22	Suggest changing this sentence to "In contrast, Perwitz et al. (2009) and Dole et al. (2010) consider how various factors contributed to the magnitude of the event, including both human and natural forcings and internal unforced variations." [Randall Dole, United States of America]	Accepted: paragraph will be clarified
10-823	10	50	29	50	33	This paragraph presents a very weak rationale for emphasizing the FAR approach. If this is the decision, it might be more straightforward to simply say that because most studies to date have used this approach they will be emphasized. There are several problems with the rationale presented here, but here are just a few: 1) The Fischer et al. (2007) study that is one argument used against what is called a linear assumption in the "magnitude approach" is actually much more akin to physically-based studies such as Perlwitz et al. (2009) and Dole et al. (2010). In fact, this land surface feedback was called out as a likely amplifying factor in Dole et al. (2010). It has been established as an important feedback amplifying summer heat waves for quite some time, and can occur entirely through natural processes (Fischer et al. 2007 provide no evidence otherwise). Using physical/diagnostic approaches, the relative importance of feedbacks such as this can be assessed during event life times (e.g., Lyon and Dole 1995, Black et al. 2004). Put simply, a physical/diagnostic approach is often quite well-suited to assessing feedbacks and their relative importance for individual events. It is certainly not grounds for neglecting such studies. 2) Great care must be taken in trying to draw a distinction between the various approaches based on linearity arguments. The FAR approach uses nonlinear climate models, but so does the physical/diagnostic or "magnitude" approach. Most FAR studies to date simply apply a linear shift in probability distributions to estimate changes in tail probabilities due to external forcing. Of greater concern is the argument that "it may be impossible to say how much smaller an event would have been in the absence of human influences." This may be true, but then one would have to question if attribution is possible at all. If the system behavior is patently nonlinear, then a potentially unmeasurable or imperfectly modeled factor may make it impossible to obtain robust estimates of responses to a given forcing, including through probability distributions such as used in FAR (for example, if the system is intransitive or almost intransitive). Fortunately, there is much evidence that while the climate system is non-linear, for many purposes it's behavior can be well approximated by a linear system with non-linearities folded into stochastic terms. This quasi-linear behavior makes attribution both possible and meaningful. [Randall Dole, United States of America]	Noted: we will stress the key reason for emphasising the FAR approach is the greater prevalence of literature using it it
10-824	10	50	29	50	37	This paragraph contains various misinterpretations and misunderstandings. Please remove, or revise. First, it should be noted that this Chapter, being part of WG I, is concerned with a physical basis for climate change, whereas the matter of impacts is the pervue of WG II. As such, the speculations in the first sentences regarding impacts are inappropriate here. Second, the authors misunderstand Fisher et al. (2007). Their analysis of the impact of soil moisture in the 2003 European heat wave identifies a natrual feedback that is common to most heat waves, as has been widely documented for many other. Further, that study is itself an example of a physically-based investigation, and as in Dole et al, for example, provides a physical understanding of the mechanisms involved in heat waves. Unsubstantiated claims about nonlinearity are made later in this paragraph. One simple recent example is illustrative of the ultility of linear approximations for assessing extremes. It is found that the magnitude of the Russian heat wave in 2010 could have been "predicted" using a simple linear regression model of the relationship between a blocking index and summertime sfc temperatures, drawn from historical data of 1880-1970. Linear relationships are indeed powerful, even for events of extreme magnitudes. Nor is the argument valid, also given in this paragraph, that nonlinearity in some fashion undermines the utility of physically-based methods for attribution. The two	See 823

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						examples in Perlwitz et al and Dole et al. relied extensively on sophisctated global climate models which encapsulate the nonlinearity of atmospheric motions, feedbacks, and sensitivties believed to exist in nature. It should also be noted that if extreme events could only be understood as fundamentally nonlinear, by which I mean nonlinear in relation to forcing as the authors' last sentence implies, then statistical methods of estimated tail-probabilities are problematic. In sum, this paragraph is compromised by FAR too many assertions that are unsubstantiated, that are incorrect, and in the end appear to be self-serving for advancing a single approach to assessing and commuicate the causes of extreme weather events. [Martin Hoerling, USA]	
10-825	10	50	29	50	44	While agreeing that many impacts do result from thresholds being crossed, others do not, but rather follow a more continuum behavior. In any case, impact behavior is not a compelling rationale for discussing only some studies here, as Working Group I, and specifically this chapter, is about attribution of physical causes irrespective of impacts. Impacts are more appropriately discussed in Working Group II. The paragraph beginning on line 39 is helpful in distinguishing various uses of the term "risk", but could probably be simplified; for example, "Note that "risk" as used here refers to event probability, whereas in hazards research the term "risk" also includes a measure of consequence or vulnerability to the event (Hulme et al. 2011)." [Randall Dole, United States of America]	See 823
10-826	10	50	30	50	32	Add a reference to SREX chapter 3 (Section 3.1.4), which addresses this aspect in detail. [Sonia Seneviratne, Switzerland]	Accepted
10-827	10	50	32	50	32	Seneviratne et al. (2006, Nature) provided the initial evidence for the importance of such feedbacks processes, in particular in the context of climate change. It identified that the contribution of soil moisture feedbacks to summer temperature variability was up to 60% in Mediterranean climate in late 20th century conditions, and of similar percentage in Central and Eastern Europe in late 21st-century conditions. [Sonia Seneviratne, Switzerland]	Accepted
10-828	10	50	34	50	37	This is an important warning which also applies in the context of the modest versus real extremes in my comment 34, etc. and should be repeated there. [Albert Klein Tan k, Netherlands]	Accepted
10-829	10	50	35	50	36	Very useful statement, thanks. [Susan Solomon, USA]	Accepted
10-830	10	50	39	50	44	This paragraph should be removed. It again appears to concern the use of FAR-based analyses for impacts and decision making. Such a debate should not be opened in this WG1 document. [Martin Hoerling, USA]	Noted: this is here to avoid confusion with WG2 regarding the interpretation of "risk" in FAR.
10-831	10	50	48	50	50	I found this sentence confusing- the rest of the paragraph seems to be making two related points-(1) the distribution may not be gaussian due to feedbacks (2) The shape of the distribution (gaussian or otherwise) may change if the feebacks are non-linear with temperature. If so, state as two points. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted -- paragraph will be clarified
10-832	10	50	52	50	55	Remove or revise the sentence beginning "Fischer et al show how…" The occurrence of land surface feedbacks during heat waves has long been known, and the positive feedbacks associated between drought and heat waves through land surface interactions is one of the salient, and natural features of heat waves. The authors' appear to be suggesting that dry soil moisture was some unique reinforcing process, not occurring previously in the historical record. Nonsense! The assessment is not at all consistent with the content of Fisher et al. 2007. Nor is there any detection and attribution evidence for an anthropogenic drying of summertime Europe that might allow one to suggest that climate change has increased the intensity or frequency of such potential feedback processes. [Martin Hoerling, USA]	Noted, and this sentence will be revised
10-833	10	50	54	50	55	The statement that the Schar et al assumption of “normal summer temperatures” is made "irrelevant" by the possibility of land surface feedbacks seems both unsupportable and overly argumentative. If the author wishes to claim that the assumption can be called into question because of land surface feedbacks, then say so. But to call the work "irrelevant" hints of an axe to grind here, and that is troubling in what is supposed to be an objective assessment. [Michael Mann, USA]	Noted, language will be revised. Note that Schaer et al (2004) themselves made this observation about their nominal return times.
10-834	10	50	55			"irrelevant" seems far too strong. What if a large number of the events which constitute the statistical distribution of normal summer temperatures are already under the influence of the amplification described?	Noted, language will be revised. Note that Schaer et al (2004) themselves made this observation about

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Albert Klein Tan k, Netherlands]	their nominal return times.
10-835	10	50				I think I commented on this on the ZOD, but it strikes me as surprising that none of the attribution work by Yiou, Cattiaux, and Vautard using synoptic profiling techniques is discussed anywhere in this chapter. It may be that it is deemed more appropriate in section 10.6.1, but it would think it is highly appropriate for this chapter. [Dáithí Stone, United States of America]	Noted: this literature must be assessed
10-836	10	51	1	50	5	This paragraph must be revised. It gives the impression that, whereas estimating the absolute probability of an event is very uncertain, estimates of the relative probability are not. As is well known , the latter is also fraught with high uncertainty as it requires knowing the statistics of tail-event behavior in a control world, and also in the forced world. These estimates are difficult to come by, are highly sensitive to models used, and thus are prone to large error bars in their estimates. The reader is given no sense of the difficulties involved. Please revise accordingly. [Martin Hoerling, USA]	Accepted, we will make clear this paragrap refers to relative confidence in relative probabilities ()
10-837	10	51	7	52	22	This could be more succinct- eg page 52 lines 1-5 could go on the previous page and subsumed in the paragraph ending on line 38 [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Noted ()
10-838	10	51	10	51	10	Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think this example is best replaced with another, or just dropped. [William Ingram, UK]	Noted ()
10-839	10	51	16	51	17	The short sentence misses a key point, and must be revised. The point being that for rare events, FAR may not be a desirable approach given that Po and P1 are each exceedingly difficult to estimate with desired accuracy. It should also be made clear, in the revisions, that a two-step approach would require a physical basis upon which to anticipate how extreme events could change in response to a forcing, which may have affected a large scale climate condition to which such events are sensitive. I therefore suggest adding a summary of how a two-step approach was explored in Dole et al. There, the Russian heat wave was a extreme event of the type highlighed in this paragraph. However, it was shown from a physical basis that hot summers were typically linked with anticyclonic blocking in the free atmosphere, based on historical data. As such,while a FAR approach to the heat wave as a single step would be prone to uncertain estimates of Po and P1, a two-step approach was explored, in which the cause for the blocking was pursued. There, based on survey of existing literature and on new model simulations, no discernable link of AGW and summertime European blocking was identified in Dole et al., further clarifying the role (or lack thereof as it turned out) of anthropogenic greenhouse gas forcing in that event. This change is required. [Martin Hoerling, USA]	Noted: paragraph will be clarified ()
10-840	10	51	38	51	38	Fig. 10.21 in truth is Fig. 10.17. Also at Page 52 line 38. [Claudio Cassardo, Italy]	Accepted ()
10-841	10	51	38	51	38	Should be Figure 10.17? [Gareth S Jones, UK]	Accepted ()
10-842	10	51	38	51	38	change figure no into 10.17 instead of 10.21, see line 44. [Helga Nitsche, Germany]	Accepted ()
10-843	10	51	38	51	40	"... in other colours"? There is only one other colour ... green. [Gareth S Jones, UK]	Accepted ()
10-844	10	51	38			typo "Figure 10.21" > "Figure 10.17" [Albert Klein Tan k, Netherlands]	Accepted ()
10-845	10	51	44	51	55	there may be a reference missing to 'Otto,2011' ? [Helga Nitsche, Germany]	Accepted ()
10-846	10	51	51	52	12	Is change a the frequency and/or intensity and/or likelihood of floods within the WG1 purview? [Francis Zwiers, Canada]	Accepted ()
10-847	10	51	57			Has the uncertainty been well enough sampled for a quantitative statement in Pall et al? [Albert Klein Tan k, Netherlands]	Noted: a caveat will be added on this ()
10-848	10	51		52		Details of an interesting flood diagnostic study over England and Wales are given. There may not be many similar studies discussing the attribution of anthropogenic climate change to increased risk of (hydrological) extremes in general, but reference to those that do (e.g. Krakauer and Fung, 2008) should be included (given that not only climate extremes are included, in which case the flood study would not fit either). [Lena M. Tallaksen, Norway]	Accepted ()

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-849	10	52	1	52	1	"increase" -> "increased" [Gareth S Jones, UK]	Accepted ()
10-850	10	52	1	53	4	Excellent section on the different approaches to 'event attribution'. [Susan Solomon, USA]	Accepted ()
10-851	10	52	10	52	12	This is somewhat unsatisfactory because, in other words, the diagnostic chosen determines the result. [Albert Klein Tan k, Netherlands]	That's the way it goes ()
10-852	10	52	16	52	16	The 'mainly natural in origin' statement was not deducible from the analysis included in Dole et al where they asked whether there was medium term predictability of the event, which is entirely beside the point. Clarity on what analyses were done and what may be concluded from them in this assessment will be welcome. [Gavin Schmidt, USA]	Noted ()
10-853	10	52	16	52	19	Jones et al. 2008 also showed observed warming trends in the mediterranean, Northern europe, central asia and northern asia Giorgi regions for summer near surface temperatures, albeit up to and including 2006. [Gareth S Jones, UK]	Accepted ()
10-854	10	52	22	52	27	The other possible paper to be referred is Matsueda (2011). Matsueda (2011) looked at predictability of Euro-Russian blocking and the associated heatwave using medium-range ensemble forecasts. Matsueda, M., 2011: Predictability of Euro-Russian blocking in summer of 2010, Geophys. Res. Lett., 38, L06801, doi:10.1029/2010GL046557. [Shoji Kusunoki, Japan]	Accepted ()
10-855	10	52	29	52	29	Rahmstorf and Connou (2011) is missing in the reference list. [Shoji Kusunoki, Japan]	Accepted ()
10-856	10	52	38	52	38	change figure no, see above. [Helga Nitsche, Germany]	Accepted ()
10-857	10	52	38	52	38	The figure is a bit confusing because the third panel has not relation to the first two panels. Neither the region or variabile treated are comparable. Two panels are related to flood risk in the UK, the other is related to temperature extremes over western Russia. [Francis Zwiers, Canada]	Rejected: we are keen to include an attribution assessment for more than one event type ()
10-858	10	52	44	52	47	I don't understand this as it seems it should not matter if one considers the change in return level or (going in the other direction) if one considers the change in return period whether or not the change is significant. [Albert Klein Tan k, Netherlands]	Noted: the difference is vertical versus horizontal offset in the return-time plots. Will clarify ()
10-859	10	53	6			Section 10.7 .While it is important to use models to try and understand the reconstructions of temperature, one should resist the temptation to overinterpret the results. The section is long. it copuld, more briefly, use past reconstructions to estimate multidecadal variability due to unforced and naturally forced variations. I had a number of issues concerning Figure 10.18 in this respect - I could not find the supplement referred to in the caption.(1) The reconstructions use different domains- but the model data (I presume) is hemispherically averaged. (2) the hemispheric data reconstructions (Moberg) reconstruction shows more variability than the reconstruction over land 30-90N- I would expect the smaller domain to show more interannual variability. (3) the model variability in panel 2 is presumably year to year variability whereas it is models multi-decadal variability that we like to be able to assess (4) I am concerned that that by using data after 1850, the anthropogenic CO2 spike may distort the fit to the natural forcings (5) It would be helpful to see the total forcing used, as well as the individual components (6) there is no estimate of goodness of fit (on, say, decadal timescales as well as annual timescales). [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. The section has been shortened to emphasize detection and attribution. We agree that there is substantial uncertainty, but also, as discussed in 10.7's introduction, the longer term record provides the only non-model based estimate of internal climate variability. (1) The supplement has been lost in transition in the FOD but is now available (CHECK). (2) The models are evaluated over the target of reconstruction which provides a like with like comparison, which is now clarified. An underlying paper has been submitted (CHECK) which will provide further support (note that the figure was an update on an earlier, published paper) and material (4). (3) Panel two has meen removed and a further panel added for multi-decadal variability. (5) The effect of excluding the late 19th century has been explored. We find detection and attribution analysis provides an assessment of fit that accounts for variability, but correlations, e.g. are shown in the submitted paper.
10-860	10	53	8	53	14	Is there no scope for using mid-holocene or LGM runs in a D&A analysis? It is not obvious to me that you need to have transient forcing simulations. [Gavin Schmidt, USA]	Rejected. It is correct that such analyses would be interesting and feasible, but not literature exists covering this. Text has been amended to clarify this.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-861	10	53	13	53	13	"assesses" -> "assess" [Gareth S Jones, UK]	Accepted
10-862	10	53	13			typo "assesses" [Albert Klein Tan k, Netherlands]	Accepted
10-863	10	53	23			Section 10.7.1 This section is entirely about challenges and gives a nice discussion of uncertainties [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Noted, thanks.
10-864	10	53	40	53	40	cite Schmidt et al (GMD 2011; GMD 2012) [Gavin Schmidt, USA]	Acceted, good point
10-865	10	53	40	53	42	I do not think that Gray et al 2010 makes the claim about models not accounting for spectral variations in TSI. What they did say is "Most current climate models include a representation of TSI variations, but their upper boundary does not extend sufficiently high to fully resolve the stratosphere, so most do not include the UV influence." i.e. regardless of whether models include TSI spectral variations many don't have a good enough stratosphere for it to make a difference. Re-phrase this sentence? [Gareth S Jones, UK]	Accepted. Text changed.
10-866	10	53	45	53	48	Mann et al 2011 is not not listed in the references as far as I can tell. An additional reference that could be added relating to volcanoes influencing tree ring proxies is Robock "Cooling following large volcanic eruptions corrected for the effect of diffuse radiation on tree rings", GRL 2005 [Gareth S Jones, UK]	Accepted. Reference fixed and Robock cited.
10-867	10	53	47	53	48	Since all annually-resolved reconstruction of hemispheric mean temperature make use of tree-ring data, the implications may be more general than implied. A more relevant statement would be that "Temperature reconstructions that make use of tree-ring records may underestimate the response to large volcanic eruptions (Mann et a, 2012)". Note also that Mann et al (2011) is now in press. The updated reference is: Mann, M.E., Fuentes, J.D., Rutherford, S., Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures, Nature Geosciences (in press). [Michael Mann, USA]	Accepted (partly) sentence rephrased. Noted for citation, thanks.
10-868	10	53	49	53	50	I feel this sentence puts a too strong association of the LIA with GHG forcing. Was the drop and rise in GHG forcing coincident with the start and end of the LIA (which would be surprising considering how poorly defined the period of the LIA is), or was there a short period of drop in GHG sometime during the period of the LIA (which may not be surprising or interesting)? Recommend saying what period (dates) the GHG forcing drop happened rather than linking it to the LIA - unless there is some evidence of a real association. [Gareth S Jones, UK]	Partly accepted. Time period given. Note that some papers claim a contribution by CO2 drop to nothing.
10-869	10	54	6			Section 10.7.2 Given the uncertainties and my comments on Fiigure 7.18- this section could be shorter and more focussed on attribution. Section 10.7.2.3 does not add much to overall attribution. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. The entire section is being shortened. Section 10.7.2.3 has been considerably shortened.
10-870	10	54	10	54	11	the use of "highly" as a modifer of "uncertain" is unwarranted. It is uncertain, but the uncertainties are small compared to, say, the amplitude of the modern warming trend. [Michael Mann, USA]	Accepted
10-871	10	54	10	54	11	Use the favoured term "Medieval Climate Anomaly" as appropriately adopted in the section heading of 10.7.4.2 in place of the outdated and generally now disfavoured term "Medieval Warm Period" [Michael Mann, USA]	Accepted and changed throughout, also to make consistent with chapter 5.
10-872	10	54	10	54	11	I don't know what 'early millennium' means. Use calendar date ranges instead. Replace MWP with MCA. [Gavin Schmidt, USA]	Accepted
10-873	10	54	12	54	12	The main warming in most hemispheric reconstructions takes place in the 20th century, not the 19th century. [Michael Mann, USA]	Accepted. Text changed.
10-874	10	54	19	54	19	Insert "variability" after "temperature". [Francis Zwiers, Canada]	Accepted. Sentence now verbatim reflects AR4 wording. Well caught thanks.
10-875	10	54	25	54	25	Definition of "all" in "all forcings"? Use here could be ambiguous, either fingerprint of each individual forcing or fingerprint of all the forcings combined was detected? [Gareth S Jones, UK]	Accepted
10-876	10	54	27	54	28	It might be noted that the fact that the observed response is often smaller than the modeled response is, in the case of volcanic forcing (which dominates the pre-anthropogenic interval) consistent with the underestimation bias in tree-ring based estimates of volcanic cooling identified by Mann et al (2012) [Mann, M.E., Fuentes,	Accepted. Text has been updated

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						J.D., Rutherford, S., Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures, Nature Geosciences (in press)]. [Michael Mann, USA]	
10-877	10	54	29	54	32	An equally plausible explanation for the poorer match for earlier centuries is the leverage of the massive AD 1258 eruption which is estimated to be several times larger in its radiative forcing than any other of the past millennium. The fact that the response to this eruption is essentially absent in most hemispheric temperature reconstructions leads to a huge data/model mismatch. Mann et al (2012) [Mann, M.E., Fuentes, J.D., Rutherford, S., Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures, Nature Geosciences (in press)] reproduce this as a consequence of threshold biological tree growth responses which limit the potential cooling recorded by tree-ring reconstructions to about 1C. [Michael Mann, USA]	Noted. Perfect model sensitivity tests in Hegerl et al, 2006 however show that removing a single short-lived spike, eve a large one, does not impact detection and attribution results based on multi-century analysis much at all.
10-878	10	54	36	54	36	I think it would be useful to say a bit more about how uncertainty is taken into account in the full data assimilation/external forcing set up. I think the important aspect here is what is learned about the need for (and amplitude of) forcing when a model is constrained by the reconstructed observations. [Francis Zwiers, Canada]	Accepted. Text has been sharpened and revised. CHECK - not sure we have space for this.
10-879	10	54	46	54	46	"Figure 10.19" is not the correct figure reference here [Gareth S Jones, UK]	Accepted
10-880	10	54	48	54	48	Use the favoured term "Medieval Climate Anomaly" as appropriately adopted in the section heading of 10.7.4.2 in place of the outdated and generally now disfavoured term "Medieval Warm Period" [Michael Mann, USA]	Accepted.
10-881	10	55	31	55	31	What about orbital forcing? The long term pre-industrial cooling trend may well be attributable [Gavin Schmidt, USA]	Accepted. List removed from header. Note that no studies are avaliable on attributable orbital forcing.
10-882	10	55	39	55	39	Definition of "all" in "all forcing"? [Gareth S Jones, UK]	Accepted. Rephrased.
10-883	10	55	40	55	42	Is the drop in temperature in Fig10.18 (3rd panel) after ~1600 in the scaled GHG simulation real or could it be explained by internal variability, i.e. is the cooling significant? It is such a small drop isn't it overstating it that it contributes to the cold conditions in the LIA. Also isn't "cold conditions in the Little Ice Age" a tautology? [Gareth S Jones, UK]	Partly accepted. The paper cited shows a small, but sustained cooling influence that contributes to detectability of CO_2 over the period up to 1950. Tautology rephrased.
10-884	10	55	48	55	50	Feulner, GRL 2011 doi:10.1029/2011GL048529 claims that "Large TSI variations are inconsistent with the climate record" Should include to contrast with Jungclaus ref. [Gareth S Jones, UK]	Noted and cited.
10-885	10	55	48	55	50	I am not sure the Jungclause 2010 ref supports the first part of this sentence. In the paper it says about comparing models with strong/weak solar forcing :- "To draw any conclusion, which of the ensembles give a more realistic representation would, however, require a careful analysis of the observed data sets, which is beyond the scope of this paper." [Gareth S Jones, UK]	Rejected. The sentence is phrased with caveats so consistent with the paper. CHECK
10-886	10	55	50	55	53	Note that most LM simulations do not include O3 responses to solar that will show up strongly in the 20th C interactive simulations. There is a possibility (probability) that this will change the pattern response, and thus cause problems for D&A based purely on the long transient runs [Gavin Schmidt, USA]	Noted. Text clarified to make clear that results refer to studies using the temporal patternwhich is less affected by changes in dynamics.
10-887	10	55	51	55	51	does ‘high end’ include Shapiro et al (2011)? (see Feulner (2011) as well though) [Gavin Schmidt, USA]	Accepted and clarified in text. CHECK
10-888	10	56	3	56	3	It is an important distinction that the relationship is with *tropical* volcanism, not all volcanism. [Michael Mann, USA]	Accepted and clarified.
10-889	10	56	4	56	4	It is worth noting that the circulation anomalies in question are broadly related to (though not identical) to the NAO/AO/NAM. [Michael Mann, USA]	Rejected as the discussion is shortened in order to accommodate the page limits and reduce discussion of qualitative material
10-890	10	56	7	56	7	"medieval climate anomaly" ... is that defined somewhere? Chap5? Should the first letters be in capitals? [Gareth S Jones, UK]	Accepted. Definition now further up and referring to chapter 5 - ADD detailed reference.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-891	10	56	8	56	8	"medieval warm period" - do you mean "medieval climate anomaly" Do they mean different things? - in which case carefully explain difference. If they are different names for the same thing I hope this is explained somewhere. [Gareth S Jones, UK]	Accepted. MCA use trhoughout now.
10-892	10	56	8	56	8	"warm conditions" is misleading, because the patterns show warm conditions in some regions and cold conditions in others. Better to say something like "heterogenous pattern of warmth in some regions and cold in others". [Michael Mann, USA]	Accepted. Text revised although using different wording CHECK THIS MAY BE SHORTENED AWAY
10-893	10	56	8	56	8	"Medieval Warm Period" is used again, only one sentence after the preferable term ("Medieval Climate Anomaly") has been used. The discussion in this paragraph actually emphasizes why "Medieval Warm Period" is a poor term. The inferred anomalies in the tropical Pacific were, by many assessments, consistent w/ cold La Nina-like conditions, and the NAO/AO/NAM pattern led to cooling in many regions, and enhanced warming in others. Simplest in this case to just eliminate "warm" so that it is reads "medieval period", with greater accuracy, and one less word. [Michael Mann, USA]	Accepted. Fixed.
10-894	10	56	9	56	13	It should be pointed out that while the highest temperatures are reconstructed in the 10th and 11th centuries, maximum medieval warming is found in most models in the 12th and 13th century when the transition to the Little Ice Age is already underway in the reconstructions. [Fredrik Charpentier Ljungqvist, Sweden]	Accepted. CHECK This may be shortened
10-895	10	56	11	56	13	"inconclusive" is an imprecise term in this context. 'inconclusive" with regard to what? The comparison can certainly inform whether certain features in model and observations are broadly compatible or broadly incompatible, and thus can inform our understanding of the dynamical mechanisms that may be important (for example, does the pattern look like El Nino or La Nina?). If one is looking for some measure of statistical significance in e.g. some numerical measure like a pattern correlation, etc. then of course more work is necessary. But it is not the case that nothing is learned by qualitative comparisons--either here, or indeed in many branches of science. [Michael Mann, USA]	Partly accepted. Sentence deleted to accommodate comment. However, this chapter is focussed on detection and attribution and hence quantitative comparisons.
10-896	10	56	15			Sections 10.7.3, 4,5 Again, although of interest, given the uncertainties, this adds little to the overall issue of detection and attribution,.It could at least be shortened. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Section is shortened. It still adds to forcings vs variability information on regoinal scales, hence kept but shortened.
10-897	10	56	21	56	21	Is the Mann et al (2009) result independent of Luterbacher et al? [Gavin Schmidt, USA]	Accepted. Clarified in text.
10-898	10	56	33	56	34	The reference should be to Mann et al (2009) not Mann et al (2008) [Mann et al (2008) provide only hemispheric mean reconstructions. Only Mann et al (2009) produce a spatial reconstruction from which a European sector average can be diagnosed] [Michael Mann, USA]	Accepted. Thanks.
10-899	10	56	49	56	50	Several studies have also detected influence of external forcing on the Chinese temperatures from 1500 onwards or even from 1000 onwards. [Fredrik Charpentier Ljungqvist, Sweden]	Now assessed. IF FOUND
10-900	10	56	54	56	54	Be consistent with the use of capitals in the names "Little Ice Age" and "Medieval Climate Anomaly" and define MCA/LIA for use later. [Gareth S Jones, UK]	Accepted.
10-901	10	56				10.7.4.1. Could de-forestation have played a role in the LIA? Section 8.4.5.2. says it could have. [Gareth S Jones, UK]	Accepted. Cross referenced. No d+a results available though.
10-902	10	56				10.7.4.1 Should probably also reference Miller et al. GRL, doi:10.1029/2011GL050168 [Gareth S Jones, UK]	Accepted. CHECK
10-903	10	57	1	57	2	Is the LIA global, in the Northern Hemisphere or parts of the northern hemisphere land? The term is not always very helpful to describe a period when not all regions was cold at the same time. [Gareth S Jones, UK]	Noted. The discussion of this period is material for chapter 5 though cross -referenced CHECK
10-904	10	57	6	57	6	I could not find Gregory et al 2011 in the references [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Fixed.
10-905	10	57	10	57	12	Another more recent reference would be Feulner, GRL 2011 doi:10.1029/2011GL048529 [Gareth S Jones, UK]	Accepted
10-906	10	57	10	57	12	Does larger solar forcing mean larger (negative) anomaly in solar forcing? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Clarified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-907	10	57	12	57	15	I am not sure Hegerl 2007a says specifically what indivdual forcings contributed to the "little ice age"? [Gareth S Jones, UK]	Rejected. As stated in sentence this is indirectly referred from detected fingerprints over longer time.
10-908	10	57	26	57	26	Suggest reverting from the Medieval Climate Anomaly to its longstanding and more descriptively-accurate name, the Medieval Warm Period, as used even in the Zero Order Draft (ZOD) of this Chapter. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. Terminology adopted from Chapter 5
10-909	10	57	28	57	29	Was the MCA warmer everywhere than during the LIA? [Gareth S Jones, UK]	Rejected. Discussion of regional reconstructions chapter 5 material. See also comment 914
10-910	10	57	28	57	40	Was not the wording in the ZOD more accurate as regards the widespread global nature of the Medieval Warm Period? It read: "Conditions in the early centuries of the last millennium were generally warmer than at present". [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. The present text much better relfects the reconstructions during the MCA and also is in much better agreement with chapter 5 where this is drawn from anyway
10-911	10	57	29	57	31	When say "during the second half of the 20th century" do you mean average over 1950-1999 or some sub-period. Some may go away thinking that temperatures could have been as warm as during 1990s? If that is the case should be more careful how expressing this. [Gareth S Jones, UK]	Accepted. Reworded.
10-912	10	57	30	57	31	Cite also Mann et al (2009) for evidence that some regions in or neighboring the North Atlantic might have been as warm as the late 20th century, but they show that this is NOT true for the northern hemisphere mean. [Michael Mann, USA]	Accepted.
10-913	10	57	31	57	33	We actually have too limited evidence to satisfactorily assess whether warm conditions occurred at different locations at different times or not during the Medieval Climate Anomaly. Too few, noisy and poorly replicated proxies still simply preclude an adequate assessment of global or even hemispheric temperatures in medieval times. It has been shown in Ljungqvist et al. (2012), considering virtually all available proxy evidence from the Northern Hemisphere, that the medieval warmth was rather geographically coherent, at least on centennial time-scales, with maximum warming over nearly the whole Northern Hemisphere in the 10th century. The full reference to Ljungqvist et al. (2012) is: Ljungqvist, F. C., Krusic, P. J., Brattström, G., and Sundqvist, H. S.: Northern Hemisphere temperature patterns in the last 12 centuries, Clim. Past, 8, 227–249, doi:10.5194/cp-8-227-2012, 2012. [Fredrik Charpentier Ljungqvist, Sweden]	Text shortened to remove discussion of issue, which is more within the remit of chapter 5 anyway.
10-914	10	57	31			Please, rephrase “the early millennium” since the Medieval Climate Anomaly started in the latter part of the first millennium CE. [Fredrik Charpentier Ljungqvist, Sweden]	Accepted.
10-915	10	57	33	57	35	When say "similar to the late 20th century" do you mean average over 1950-1999 or some sub-period. Some may go away thinking that temperatures could have been as warm as during 1990s? If that is the case should be more careful how expressing this. [Gareth S Jones, UK]	See response to 911
10-916	10	57	33	57	39	I think there is too large a focus on Europe. Evidence for medieval climate conditions is also abundant in China and North America and should be discussed. [Fredrik Charpentier Ljungqvist, Sweden]	Accepted. Material from Europe shortened. CHECK IF CHINA ADDED
10-917	10	57	33			Briffa et al. (2002) is lacking in the reference list. Moreover, such an old study is scarcely an appropriate reference to a statement of a heterogeneous medieval warming since subsequent research has had access to much more data when revisiting the subject. [Fredrik Charpentier Ljungqvist, Sweden]	Accepted. Sentence deleted.
10-918	10	57	36	57	36	please change "medieval warm period" to "medieval period". Nothing is gained by using the potentially misleading and loaded 'warm' moniker. [Michael Mann, USA]	Accepted.
10-919	10	57	36	57	36	This statement is misleading. Goosse et al (2006) were talking ONLY ABOUT EUROPE AND DURING SUMMER. The statement that the radiative forcing was similar ONLY holds for that specific case, as it has to do with land use changes that were specific to parts of Europe. They were not talking about the Northern Hemisphere or globe in general, nor were they talking about the winter season. [Michael Mann, USA]	Accepted. Sentence deleted.
10-920	10	57	39	57	40	model dependent and requires the assumption that local radiative forcing has an exclusively local temperature impact (which is not justified) [Gavin Schmidt, USA]	Accepted. Sentence deleted.
10-921	10	57	42	57	44	I think it ought to be pointed out that the mismatch between model results and the reconstructions during	Partly accepted. Discussion of timing discrepancy has

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						medieval times is larger in the newer reconstructions since they show a larger amplitude of warming in the 10th and 11th centuries whereas the models still produce the strongest warming in the 12th and 13th centuries. [Fredrik Charpentier Ljungqvist, Sweden]	explicitly been added. Discussion of newer reconstructions being warmer than MCA has not been added as this is not universally true. CHECK
10-922	10	57	42	57	44	Do small volcanoes cause warming? I know that this is not what this means so should clarify that it is the lack of large volcanoes that cause relative warming? [Gareth S Jones, UK]	Accepted and reworded.
10-923	10	58	4	58	4	there isn't a hugely impressive agreement here either. Rewrite this line so that it feels less like you are grasping at straws, [Gavin Schmidt, USA]	Revised.
10-924	10	58	11	58	13	Note that Mann et al (2005) [Mann, M.E., Cane, M.A., Zebiak, S.E., Clement, A., Volcanic and Solar Forcing of the Tropical Pacific Over the Past 1000 Years, Journal of Climate, 18, 447-456, 2005] reproduce the alternation between La Nina-like and El Nino-like periods recorded in the corals as a consequence of the forced response of ENSO to past volcanic+solar radiative forcing. [Michael Mann, USA]	Rejected. Text has been shortened and material deleted.
10-925	10	58	40	58	43	Has it been discussed the issue of uncertainties in the forcings (in particular solar and volcanic) in this section? [Gareth S Jones, UK]	Text has been revised.
10-926	10	58	42	58	43	The statement here isn't an accurate reflection of Mann et al (2011--now in press). They don't argue that the eruption was overestimated but, instead, that proxy records (primarily temperature-sensitive tree-ring data) used to reconstruct the volcanic cooling signal suffer from an intrinsic underestimation bias due to the existence of a cooling threshhold (about 1C relative to the pre-anthropogenic baseline) beyond which the trees do not record. The statement should be corrected to reflect this distinction. [Michael Mann, USA]	Text revised.
10-927	10	58	43	58	43	This is not what Mann et al (2012?) concluded. Indeed, M2012 suggests that the paleo data are not registering the event. [Gavin Schmidt, USA]	Accepted. Revised.
10-928	10	59	6	59	7	Consistency of use of terms MCA/LIA? [Gareth S Jones, UK]	Accepted. Revised.
10-929	10	59	6	59	7	change "medieval warm period" to "MCA" for consistency with the rest of the chapter. [Michael Mann, USA]	Accepted.
10-930	10	59	8	59	8	What is meant by "all forcings" here? Does it include land use, biomass burning aerosols ...? [Gareth S Jones, UK]	Text revised. Text added above to clarify forcings.
10-931	10	59	10	59	10	Do you mean significant in a statistical sense? [Francis Zwiers, Canada]	Accepted. Text revised to clarify.
10-932	10	59	11	59	12	should change "warm conditions early in the millennium" to "warm conditions early in the millennium in some regions" since the regional evidence for warmth is greatly heterogenous, and there is evidence instead of unusual cold in some regions. [Michael Mann, USA]	Text revised.
10-933	10	59	12	59	15	To say "they have contributed to " Northern Hemisphere temperature seems almost indisputable- do you mean that external forcing combined with internal varaibility as estimated by climate models are very likely (sufficient) to explain Northern Hemisphere variability ....."? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
10-934	10	59	55	59	57	(Fig. 10.19 caption) The variables considered should be specified more clearly. [Christian-D. Schoenwiese, Germany]	The figure has been deleted.
10-935	10	60	2	60	4	Complete sentence. [Seung-Ki Min, Australia]	Text has been deleted from here. Barnett et al is now discussed in section 10.3
10-936	10	60	2	60	7	Very long run-on sentence that needs some work. [Francis Zwiers, Canada]	Text has been deleted from here. Barnett et al is now discussed in section 10.3
10-937	10	60	9	60	11	This reference is incorrect. I think it should be Stott et al 2008b (Stott, Sutton and Smith) [Gareth S Jones, UK]	Agreed. The whole section has been substantially revised to reflect it being a synthesis of evidence from across the climate system
10-938	10	60	28	60	31	Even if the instrumental records for each element of the climate system are independent in terms of	The point being made here is that the evidence from

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						observation errors, it does not follow that the internal variability of the elements being recorded are independent, particularly over longer periods. Also, the joint interpretation may be dominated by one particular element and the contributions of the others may be illusory, with no increase in confidence justified. For instance, in the Forest et al. 2006 study of climate parameters, almost all the influence on the final posterior PDFs arose from the 4 zone surface temperature record, with virtually none from the atmospheric layers multi-zone temperature record and little from the deep ocean temperature record. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	multiple observational sources that is in agreement provides additional confidence as formalised in the IPCC Guidance Note for Lead Authors on the consistent treatment of uncertainties. It is important to assess what additional information is provided by multiple indicators and that is what is assessed in this section.
10-939	10	60	35	60	25	Heading- I think "Climate system synthesis" might be better- it covers just the physical system [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Partially accepted. The section and subsection titles have been renamed.
10-940	10	60	43	60	43	This statement needs to be qualified with uncertainty language I think. It is currently stated with certainty! [Francis Zwiers, Canada]	Accepted. Text amended.
10-941	10	60	48	60	48	"This is important" - check typo. [Seung-Ki Min, Australia]	Accepted. Text amended.
10-942	10	61	9	61	9	It would be best not to write in the present tense, since discussino of observed changes will necessarily concern the past when the report is published. [Francis Zwiers, Canada]	Accepted. Text amended.
10-943	10	61	17	61	20	Much of this is due to ozone loss. So I don't see how it adds to an overall view of what GHG have done; please clarify or delete from this section. [Susan Solomon, USA]	Accepted. Text deleted.
10-944	10	61	21	61	21	Correct spelling is "millennium" [Gareth S Jones, UK]	Noted. Text amended
10-945	10	61	24	61	24	I don't recall that there was much discussion in the chapter about implications for heatwaves from detection and attribution of changes in temperature extremes. [Francis Zwiers, Canada]	Rejected. There is sufficient evidence assessed in the chapter that human influence has altered the probability of some observed heatwaves.Text amended to be consistent with table.
10-946	10	61	36			Include Forest et al. (2008, Tellus) which demonstrates this as well. [Chris Forest, USA]	Accepted.
10-947	10	61	39			replace "CO2 concentration" with "radiative forcing" [Chris Forest, USA]	Partly accepted. Radiative forcing is now referred to as well (CHECK) However ECS is defined via CO2 doubling, not radiative forcing more broadly, and using radiative forcing would invoke efficacy which may not be the same for different forcings.
10-948	10	61	44			Does "transient" refer to "climate sensitivity" or to "transient climate response"? This is confusing. [Chris Forest, USA]	Accepted. Sentence reworded.
10-949	10	62	1	62	12	The Held et al. (2010) study used a very simple two-box model of the ocean, rather than the usual mixed layer plus diffusion (with or without upwelling) global model, which appears to be much more realistic (see, e.g., the seminal paper Hoffert, 1980, The role of deep sea heat storage in the secular response to climatic forcing, GRL). The lack of a diffusive element in Held's model results in a different, probably unrealistic, time response profile to warming. If the study is to be cited at all, a clear caveat to this effect should be included. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. Despite their widespread use, it is not at all clear that box-diffusionmodels are a physically superior simplified representation of ocean heat uptake than are the slab models such as used by Held et al. (2012). It is well known the heat transfer into the interior ocean occurs to a large extent advectively or deep meridional overturning. Contrasting the rather efficient advective ventilation with slow diffusion, the slab approximation might indeed be superior to the box-diffusion models, and also provides a better representation of the long-term deep-ocean warming than do the diffusion models (Li et al., 2012). Apart from that, Held et al. (2010) is cited to introduce a conceptual model for the separation between transient sensitiviy and ECS, and to explain why TCR is applicable beyond the 1%

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							increase case. The model is not used further here.
10-950	10	62	1			insert "large-scale" to read increase in large-scale radiative forcing [Chris Forest, USA]	Accepted (CHECK)
10-951	10	62	13	62	13	Do we really need yet another climate sensitivity type notation "NTCR"? I think the number of ones we have already is plenty to keep us on our toes. NTCR is only used three times in the whole of WG1 so if you want to mention it at all here stick to "normalised TCR". [Gareth S Jones, UK]	This paragraph will be reworded and shortened, but we are keen to point out that TCS, TCR and NTCR are variants of the same climate system property.
10-952	10	62	16	62	18	"it is not necessary to introduce new notation" ... then why did you? [Gareth S Jones, UK]	Fair point, see 952
10-953	10	62	19	62	19	"constraining" doubling. [Christian-D. Schoenwiese, Germany]	Reword to "constraining TCR is a crucial…" (comment unclear)
10-954	10	62	31	62	33	Need to include Forest et al. (2008) (see Figure 6.) and Libardoni and Forest (2011). Both provide pdfs of TCR. Probabilistic projections are given in Sokolov et al. (2009) and Webster et al (2011) using the MIT IGSM. Libardoni and Forest (2011) provide sensitivity estimates of the TCR and ECS pdfs to the choice of the surface temperature observational data. [Chris Forest, USA]	These studies will be included in the SOD (CHECK)
10-955	10	63	5	63	5	Move brackets to just around "2011"? [Gareth S Jones, UK]	Accepted
10-956	10	63	7			It should be noted that the range of TCR is related to the combined climate sensitivity and rate of ocean heat uptake with the lower bound being more directly related to ECS with the added the difficulty in constraining the rate of deep-ocean heat uptake. The upper bound is more directly related to the observed temperature increase and forcing uncertainty. [Chris Forest, USA]	We will clarifyin the SOD as space permits
10-957	10	63	22	63	25	Also is the linear anthropogenic warming used by Camp and Tung 2007 consistent with a high TCR? ... probably not. Should mention the possible inconsistency. [Gareth S Jones, UK]	This caveat will be mentioned, with the countercaveat that it could be consistent with a strong aerosol forcing with a somewhat smoother time evolution than normally used.
10-958	10	63	27	63	30	This conclusion fails to caution that the TCR may be specific to different forcings. Since models are used throughout this publications in this section, they generally couple CO2 to the whole mixing layer as if IR penetrate meters like solar rather than mere microns. The TCR to CO2 forcing cannot be assumed based upon model evidence and basic principles of nonlinear systems to be equivilent to solar forcing which couples spatially and chemically differently to the climate system. [Martin Lewitt, United States of America]	The depth of solar penetration into the ocean is not relevant on these timescales, but the fact that TCR to a shortwave forcing may be different to TCR from a longwave forcing is relevant, and will be noted (CHECK).
10-959	10	63	42	63	45	Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, & the fact that Allan & al (2010) effectively retracts Allan & Soden (2008) & leaves open the possibility that the signal that does still appear is also spurious, I think it best to omit these 2 sentences. If not, they should be caveatted appropriately to indicate that these results, though suggestive, are not to be trusted. [William Ingram, UK]	Material has been reduced and heavily caveated.
10-960	10	63	43	63	44	"a best-estimate regression coefficient of 2–3 but an uncertainty range that includes one": it is not clear to me how a regression cofficient can assume values larger than one. [Claudio Cassardo, Italy]	Sentence changed, deleting the discussion of scaling factors.
10-961	10	63	45	63	45	A reference, Liu et al. (2009, GRL, 35, doi:/10.1029/2009GL040218), can be added here, along with these two references. [Chia Chou, Taiwan, ROC]	Accepted. (CHECK)
10-962	10	63	47	63	47	This is exactly where the MH simulations might come into play. [Gavin Schmidt, USA]	Noted. See response to comment above.
10-963	10	63	47	63	50	The failurre of models documented by Wentz to represent more than one third to one half the increase in the precipitation, is correlated error and under represents the negative feedback from the cycling of the water cycle. The implications of this correlated error should reduce the confidence of all model based and model ensemble based conclusions throughout the chapter. All further model agreement and consistency is called into question. If it can't be quantified, then 0.58W/m^2 of energy imbalance can't be attributed or projected. [Martin Lewitt, United States of America]	Rejected. The rest of that same paragraph discusses why no such conclusions can be drawn from the results referred to in these first few lines of the paragraph. Note that discussion of precipitation response is now removed for brevity.
10-964	10	63	55	63	55	No such reference – 2002 is presumably meant [William Ingram, UK]	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-965	10	63	55	63	55	Remove ([William Ingram, UK]	Rejected, Meaning of comment not clear (what should be removed - no clear indication)
10-966	10	64	19	65	35	Very good section [Susan Solomon, USA]	Noted. Thanks !
10-967	10	64	21	64	21	Define this as the Charney sensitivity because ECS is ambiguous since neither vegetation, nor ice sheets nor oceans are in equilibrium in these conditions. Depending on the study, issues with feedbacks related to short-lived species (O3, aerosols, dust etc.) might also be important and are being neglected. [Gavin Schmidt, USA]	Rejected. It was decided that a further technical term for ECS would not be helfupl. Instead it is clarified more clearly in the text that ECS as defined here does not relate to long-term earth system feedbacks such as vegetation and ice sheet change.
10-968	10	64	21	64	22	How is ECS related to the climate feedback parameter and TCR? [Gareth S Jones, UK]	Partly accepted. TCR now related to, but adding the climate feedback parameter here adds unnecessary complication at this point
10-969	10	64	23			Add "and the uncertainty in the radiative forcing" after "ocean heat uptake" [Chris Forest, USA]	Accepted. Radiative forcing added in the next sentence after rephrasing this particular one.
10-970	10	64	24	64	24	Definition of "effective climate sensitivity". Not sure you can just leave this important concept to the glossary. [Gareth S Jones, UK]	Rejected. The term effective climate sensitivity is not used, as the sensitivity as estimated here is effective sensitivity, differentiation to very long response and ESS in text.
10-971	10	64	25	64	27	The statement that estimating ECS requires comparing observed change with model results is too sweeping. Comparisons of changes in net TOA radiative fluxes with changes in surface temperature can permit estimation of ECS without much or any input from model results. See section 10.9.3.2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. Sentence has been rephrased. However a wide use of the term model is intended here, including a simple conceptual physical model - such a model at the minimn is required to relate surface temperature to radiative fluxes.
10-972	10	64	41	64	42	It could be touched upon that there are alternative approaches to quantifying uncertainty. Tanaka et al. (2009, Geophysical Research Letters, doi:10.1029/2009GL039642) quantifies the climate sensitivity uncertainty by using an optimization approach (without directly estimating a probability density) -- they use a shape of the cost function for optimization, which approximately indicates the shape of a probability densitiy. Krieger (2005, PhD dissertation at Potsdam University) use an imprecise probability approach. Then it would also become clear why their results are not shown in Figure 10.20. [Katsumasa Tanaka, Switzerland]	Accepted, sentence rephrased
10-973	10	64	42	64	47	Estimates using Bayesian statistics do not have to involve prior information or prior beliefs to any material extent. There is a large body of statistics literature about selecting prior distributions for Bayesian inference that are noninformative, that is which do not involve using prior information or prior beliefs and instead allow even weak data to dominate the resulting estimated posterior distributions. Jeffreys' priors are a well known example of a noninformative prior. (Note that the shape of noninformative priors reflects the characteristics of the likelihood functions involved and has no interpretation in probabilistic terms.) It is very surprising that use of noninformative prior distributions have not become standard in climate science; without their use objective inference is impossible. The commonly used uniform in ECS distribution can be expected to be highly informative for estimating ECS in most cases, leading to an upwards bias in central Bayesian estimates of ECS and to a much fatter upper tail to their probability density functions than that implied by the evidence. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. A sentence pointing out weak constraints resulting from flat priors has been added. However there is no body of literature that is broad enough to use other than non-informative priors on the comparison figures and little has been published on the use of the non-informative prior in S.
10-974	10	65	10	65	11	One could consider citing Tanaka and Raddatz (2011, Climatic Change Letters, DOI: 10.1007/s10584-011-0323-2), which explores how much warming is hidden by the aerosol forcing under different climate sensitivity and different emissions scenarios. [Katsumasa Tanaka, Switzerland]	Accepted
10-975	10	65	14	65	16	Would it be simplier to say "using spatial information..." as I assume that is what specifically is different to the "global mean diagnostics"? [Gareth S Jones, UK]	Accepted
10-976	10	65	16	65	18	The negative tone of this sentence is certainly not warranted. Let me quote from Hegerl et al 2007b (i.e. AR4), page 678: "The large uncertainty in total aerosol forcing makes it more difficult to accurately infer the climate	Sentence revised. However the message that global only diagnostics make it more difficult to arrive at tight

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						sensitivity from observations etc." [Henning Rodhe, Sweden]	constraints is preserved as supported by the literature.
10-977	10	65	17	65	19	This insight is hardly unique and certainly not original to Schwartz et al 2010. Please give more appropriate citations. [Gavin Schmidt, USA]	Sentence revised. A further citation has been added.
10-978	10	65	19			change to "enable less uncertain estimates of future warming" [Henning Rodhe, Sweden]	Sentence revised.
10-979	10	65	21	65	24	There is no Figure 10.24 [Gareth S Jones, UK]	Revised.
10-980	10	65	30	65	32	The sensitivity to OHC data was also shown in Sokolov et al. 2009, Clim Dyn, DOI 10.1007/s00382-009-0556-1 [Chris Forest, USA]	Accepted.
10-981	10	65	34	65	35	high or low relative to what - AR4? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence revised.
10-982	10	65	39	65	50	Please add a few sentences to explain how present day observations can (or can't) give insight to ECS as opposed to TCS, or something in between. What happens in the longer term if e.g. the latitudinal distribution of clouds changes due to slow changes in SSTs? [Susan Solomon, USA]	Accepted. Discussion added (CHECK IF KEPT)
10-983	10	65	40	65	47	Is the definition of the climate feedback parameter consistent across the chapters? Chap 13 seems to use lambda and alpha symbols for the climate feedback parameter. Then chapter 8 uses lambda for "climate sensitivity" ! [Gareth S Jones, UK]	Accepted. Use of notation to be clarified with chapter 13
10-984	10	65	47	65	47	"An estimate is shown in Figure 10.20": I do not think that actual Fig. 10.20 is the one intended here. If it is the case, could you please add some details about how to interpret. [Claudio Cassardo, Italy]	Accepted. Figure caption has been clarified.
10-985	10	65	55	66	2	Reference should be made to Lindzen and Choi, 2011, On the observational determination of climate sensitivity and its implications. Asia-Pacific J. Atmos. Sci., 47(4), 377-390. In that paper Lindzen and Choi addressed the criticisms and limitations of Lindzen and Choi (2009) and showed that their 2009 results - showing negative rather than positive overall feedback - held up, implying that climate models were substantially exaggerating climate sensitivity. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Discussion of Lindzen and Choi added.
10-986	10	66	20	66	21	Higher values of S?? [Francis Zwiers, Canada]	Accepted. Type fixed.
10-987	10	66	21	66	21	"values of S": what is S? [Claudio Cassardo, Italy]	accepted. Typo fixed
10-988	10	66	21	66	21	What is "S" when it is at home? Define "S". [Gareth S Jones, UK]	Accepted. Type fixed.
10-989	10	66	21	66	27	It should be pointed out that the fact that AO-GCMs generally perform well in simulating individual volcanic eruptions despite, as considerable evidence indicates, mixing surface heat into the ocean substantially too efficiently must imply that the ECS they exhibit are generally excessive. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. The finding that models mix too effectively is not robustly established, and if true, it would not necessarily directly related to ECS in models (see cited paper Kuhlbrodt and Gregory) ,
10-990	10	66	27	66	27	Move brackets to just around "2008"? [Gareth S Jones, UK]	Fixed. Problem due to endnote.
10-991	10	66	27	66	29	Tung et al. (2008) derived their ECS estimate from their TCR range of > 2:5 to 3:6 K, based on the change in a spatial pattern of surface temperatures with Total Solar Irradiance. That TCR range is above and does not overlap with the recent estimate by Gillett 2011a, which uses a long (1851-2010) period of temperature observations and concludes that it is extremely unlikely that the TCR exceeds 1.8 C. Importantly, Shaviv found that the total radiative forcing associated with solar cycles variations is about 5 to 7 times larger than just those associated with the TSI variations, Shaviv (2008): Using the Oceans as a Calorimeter to Quantify the Solar Radiative Forcing. GRL, VOL. 113, A11101, 13 PP. That implies that Tung et al's estimated sensitivity is invalid, and needs to be divided by a factor of 5 to 7 times. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence revised to reflect that uncertainty, and further uncertainties in that estimate
10-992	10	66	29	66	29	"TRC"? [Gareth S Jones, UK]	Accepted. Typo fixed.
10-993	10	66	30	66	33	Foster et al (2008) was in fact the first published response to Schwartz et al (2007) making these points.	Accepted. Citation added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Please cite it here: Foster, G., Annan, J.D., Schmidt, G.A., Mann, M.E., Comment on 'Heat Capacity, Time Constant, and Sensitivity of Earth's Climate System,' by S. E. Schwartz, , J. Geophys. Res., 113, D15102, doi: 10.1029/2007JD009373, 2008. [Michael Mann, USA]	
10-994	10	66	33	66	33	Also Foster et al (2008) [Gavin Schmidt, USA]	Cited.
10-995	10	67	8	67	8	What is an "EMIC"? [Gareth S Jones, UK]	Now spelled out
10-996	10	67	10	67	10	What is "PMIP2" [Gareth S Jones, UK]	Now spelled out
10-997	10	67	11	57	11	What is meant by "all forcings" here? Does it include land use, biomass burning aerosols ...? [Gareth S Jones, UK]	Forcings missing in PMIP2 for the LGM (!) are now spelled out
10-998	10	67	27	67	27	define ESS [Gavin Schmidt, USA]	Now spelled out and back related to chapter 5
10-999	10	67	42			I suppose you mean Schwartz, 2007 (not Schwartz et al., 2007) [Henning Rodhe, Sweden]	citation fixed
10-1000	10	67	44	67	45	There is an egregious omission here, since Foster et al (2008) was in fact the first published response to Schwartz et al (2007) making all of these points. Please cite it here: Foster, G., Annan, J.D., Schmidt, G.A., Mann, M.E., Comment on 'Heat Capacity, Time Constant, and Sensitivity of Earth's Climate System,' by S. E. Schwartz, , J. Geophys. Res., 113, D15102, doi: 10.1029/2007JD009373, 2008. [Michael Mann, USA]	citation fixed
10-1001	10	68	2	68	2	The response to Henriksson et al by Annan and Hargreaves should be cited here, and the conclusions of Hendriksson et al, perhaps tempered a little. [Gavin Schmidt, USA]	The response has been added. Sentence revised
10-1002	10	68	11	68	13	The evidence is too conflicting for it to be possible to conclude that it is very likely that ECS is larger than 1.5 C. If the studies Lindzen, Spencer etc. are correct then ECS is very probably below 1.5 C. The statement that ECS is likely in the range 2-4.5 C should be qualified by making clear that, even if that statement is correct, the most likely value is in the range 2-3C (as implied by the probability density function graphs, taking them all together), as otherwise most readers will assume that it is in the centre of the range, i.e. 3-3.5 C. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Discussion of most likely value has been added elsewhere. Results providing very small estimates are assessed to have large uncertainties, assumptions whose validity is unclear, and sometimes methods that don't evaluate if applied to climate models with known sensitivity, hence the overall assessment of very unlikely below 1.5 remains
10-1003	10	68	41	69	2	I found this section somewhat disappointing. Is there nothing more than can be said about aerosol RF from D&A studies? This contradicts the promising statement made on page 61, lines 44-45. [Olivier Boucher, France]	Section revised, but space and results don't permit a much more extensive discussion
10-1004	10	68	41			Section 10.9.4: There are studies that explore the relationship between climate sensitivity and aerosol forcing estimates (Andreae et al., 2005, Nature; Chylek et al., 2007, JGR; Knutti, 2008, GRL; Tanaka et al, 2009, GRL; Armour and Roe, 2011, GRL; Tanaka and Raddatz, 2011, Climatic Change Letters). From the title of this subsection, it seems to me that these studies are worthy of discussion. [Katsumasa Tanaka, Switzerland]	Section revised, but due to space constraints not all could be added.
10-1005	10	68	43	68	47	It should also be pointed out that Forest et al. (2008) estimated total aerosol forcing for the 1980s to have 90% bounds of -0.70 to -0.27 W/m^2, a much tighter band than Murphy et al. obtained for the 1970s, 1980s and 1990s combined. The two studies' uncertainty bands only just touch, at -0.7 W/m^2. This is well below the total net aerosol forcing used by most AO-GCMs, providing further evidence suggesting that most AO-GCMs overestimate ECS (necessary for the AO-GCMs to match historical warming with greater negative aerosol forcing, exacerbated by excessive ocean heat uptake rates). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Partly accepted, text expanded and revised.
10-1006	10	68	46	68	46	"earth" - should this be "land"? [Gareth S Jones, UK]	Accepted.
10-1007	10	68	49	68	51	How does "climate sensitivity -Seff" relate to TCR, ECS and climate feedback parameter? What is its definition? [Gareth S Jones, UK]	Text revised.
10-1008	10	68	49			See note 8 regarding sensitivity of probability distributions for ocean heat uptake to ocean heat content data in Sokolov et al. (2009) Climate Dyn. Clim Dyn DOI 10.1007/s00382-009-0556-1 [Chris Forest, USA]	Taken into account

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1009	10	68	52	69	10	It should be pointed out that estimates of ECS that involve AO-GCMs are likely to be biased upwards, not downwards, by the general tendency of AO-GCMs to mix surface heat into the ocean too efficiently, for which there is considerable evidence in addition to that in Forest et al. (2008). Even if too fast mixing of heat into the ocean does not directly affect the estimate of ECS, it is very likely to have led to model parameterisation reflecting, inter alia, excessive ECS and aerosol forcing, since with correct values for those climate system properties but excessive mixing of heat into the ocean a AO-GCM would significantly underestimate past warming, particularly over the second half of the 20th century. See Sokolov et al., 2003, Comparing Oceanic Heat Uptake in AOGCM Transient Climate Change Experiments, JClim., which found a sample of 11 AO-GCMs to have effective ocean diffusivities between 4 and 25 cm^2/s, with a mean of 10. By comparison, Forest et al. (2006) found that observations constrained effective ocean diffusivity to no more than 4 cm^2/s (95% confidence) with a mode of 0.65 cm^2/s. See also Hansen et al. (2011). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Partly accepted, text revised. The Forest et al 2006 finding that models mix heat too effectively into the deep ocean has, however, been found sensitive to ocean heat content data uncertainty , which is clarified in the revised text.
10-1010	10	68	58	69	2	Hansen et al. 2011 also conclude that climate models "mix heat too efficiently into the deep ocean" and that aersol forcing is large. Mention also? [Gareth S Jones, UK]	See response to comment 1009, this only relates to estimates originating when estimating ECS/TCR
10-1011	10	69	6	69	10	What are "emission floors"?? [Gareth S Jones, UK]	Accepted, clarified
10-1012	10	69	6	69	33	Sub-section need to present assessment of recent studies rather than review of literature [ABHA CHHABRA, INDIA]	Taken into account
10-1013	10	69	17	69	30	Assuming TtC is terraTons of carbon, how can degC/TtC be proper units? CO2 forcing is proportional to ln(CO2). [Stephen Gaalema, USA]	Rejected- ths is the sensitivity of the climate syste to the the accumulation of CO2 in the atmosphere. Thus this sensitivity times the accumulated co2 in the atmosphere will give the temperature change, around the mean temperature of the earth. The radiative forcing depends on natural logarithm of CO2.... a quite different parameter. Text is unchanged.
10-1014	10	69	36	71	40	FAQ 10.1: This FAQ covers the relevant issues in a way which I think is generally understandable to a non-specialist reader. In some specific places I've recommended removing references (for consistency with the standard WG1 FAQ style), or recommended providing some explanation of technical terms. [David Wratt, New Zealand]	Taken into account - Revised FAQ is very considerably condensed, with references removed and fewer technical terms. Subsequent specific responses by this reviewer are addressed below.
10-1015	10	69	41	69	54	"Attribution" is merely based on correlation which can never prove cause and effect. What is "most likely" is the subjective opinion of people who have been paid to provide the information, who therefore have a conflict of interest. [VINCENT GRAY, NEW ZEALAND]	Reject - We disagree with this statement. Attribution is based on considerably more than correlation and is based on objective statistical analysis, not subjective judgement.
10-1016	10	69	41	69	54	This "definition" seems more limited than the text in 10.2.1, which includes other "more flexible" approaches [Albert Klein Tan k, Netherlands]	accepted and reconciled
10-1017	10	69	45	69	50	This should probably be qualified a bit, even in this summary paragraph. It is true for temperature, but perhaps not so clear for some other variables. [Francis Zwiers, Canada]	Agreed - the chapeau text has been generalized and simplified.
10-1018	10	69	52	69	52	This is potentially confusing because confidence assessments are not quantified. Thus this differs substantially from the "very likely most" assessment. [Francis Zwiers, Canada]	Agreed - we removed the phrase "with high confidence"
10-1019	10	69	56	69	56	An amazing confession that immediately violates the validity of all the computer models oif the climate. as they all assume the earth is in an impossible steady state. [VINCENT GRAY, NEW ZEALAND]	Reject - Computer simulations of climate can be, and are, set up to explicitly address the causes of climate change.
10-1020	10	69				FAQ 10.1: Chapeau could be made more concise - avoid any technical language in the opening chapeau (e.g., space time structure). [Thomas Stocker/ WGI TSU, Switzerland]	Agreed - the revised chapeau is much shorter and technical language has been avoided
10-1021	10	69				FAQ 10.1, Fig 1: An improved option for this figure might be to show the current panel with the observed change, then a stack of different panels for individual model runs for the 'natural' and 'all' forcing runs, linking	Taken into account - This figure is being revised for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						to the ensemble results plotted currently on the right hand panel. Alternatively consider a second figure that better illustrates the idea of 'fingerprinting''. [Thomas Stocker/ WGI TSU, Switzerland]	
10-1022	10	70	2			What is meant here by the term "subtle" ? Maybe expand this sentence to explain. [David Wratt, New Zealand]	Taken into account - We have condensed, rather than expanded, the FAQ text here to describe causes of climate change more concisely. The term 'subtle' no longer appears in the revised text.
10-1023	10	70	3			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 5 be dropped ? [David Wratt, New Zealand]	Agreed - References to other chapters have been removed
10-1024	10	70	12			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 14 be dropped ? [David Wratt, New Zealand]	Agreed - References to other chapters have been removed
10-1025	10	70	15	70	17	The solar variations are over an approximate 11 year cycle. This can vary between 9 to 14 years in practice. [Gareth S Jones, UK]	Taken into account - Discussion of different forcings has been much condensed in the FAQ so this level of detail is no longer present in the description of any of the forcings.
10-1026	10	70	15	70	17	Solar activity should be addressed in some more detail. So, solar brightness varies in both cycles (11, 22, 76 etc. yr) and episodes of "quiet sun" (low level for a relatively long period, e.g. so-called Maunder Minimum). Furthermore, note that the climate forcing is not due to sunspots which are relatively cool aresas at the photosphere but side effects like solar flares and protuberances. [Christian-D. Schoenwiese, Germany]	Taken into account - Discussion of different forcings has been much condensed in the FAQ so this level of detail is no longer present in the description of any of the forcings. Radiative forcing due to solar variability is treated in much more detail in Chapter 8, but space limitations prevent us from this level of analysis in the FAQ.
10-1027	10	70	17			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 8 be dropped ? [David Wratt, New Zealand]	Agreed - References to other chapters have been removed
10-1028	10	70	20			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 7 be dropped ? [David Wratt, New Zealand]	Agreed - References to other chapters have been removed
10-1029	10	70	30			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 8 be dropped ? [David Wratt, New Zealand]	Agreed - References to other chapters have been removed
10-1030	10	71	9	71	11	The wording "dominant contributor" is too strong; suggest replacing by "largest contributor", and making clear that there is considerable uncertainty in all attribution studies. Also "strongly modified by the cooling associated with increased aerosol concentrations" is too strong; some on the studies that constrain total aerosol forcing most tightly (e.g. Forest et al. 2006) point to it being only 10%-25% of greenhouse gas forcing; a number of other studies are consistent with the possibility that aerosol forcing could be anywhere in this range. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account - This paragraph has been completely rewritten and the text commented upon here has been condensed.
10-1031	10	71	9	71	15	It would be helpful to distinguish those features unique to greenhouse gases (eg tropospheric warming/stratospheric cooling) from those which rule out some sources of warming (eg surface temperature patterns which are inconsistent with natural modes such as the AMOor PDO) or the ocean as the heat source (decrease of warming with depth) Also, some features cited are consistent with a number of possible causes (more warming over land and in high latitudes). The point is that of all the possible causes considered, only anthropgenic greenhouse gases are consistent with ALL these features. [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted - The final two paragraphs of the FAQ are being rewritten to try to clarify the text along these lines. The differential change of troposphere/stratosphere temperatures will be clarified in a revised figure 1 (still under construction)
10-1032	10	71	9	71	22	Among the numerous studies, I would like to report this study just published by Attanasio et al., [A contribution to attribution of recent global warming by out-of-sample Granger causality analysis, by Alessandro Attanasio, Antonello Pasini and Umberto Triacca, available on http://onlinelibrary.wiley.com/doi/10.1002/asl.365/abstract in which a different method not related to GCM or atmospheric models produces a result quite similar to those referenced in this chapter. [Claudio Cassardo, Italy]	Taken into account - Although FAQ style protocol precludes explicit references to other studies.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1033	10	71	13			A general reader might not understand the terms "troposphere" and "stratosphere" - so I suggest you at least identify the altitude ranges for each (in brackets). [David Wratt, New Zealand]	Accept - we have removed these terms in the (temporary) revision of the FAQ text, but plan to reintroduce these terms with appropriate clarification when a revised figure is complete.
10-1034	10	71	25	71	38	Once more the "natural"; forcings do not include the most important influences on the record. They are the various ocean oscillations and the various urban and land use changes that have talen place. If these are included the two graphs are almost identical [VINCENT GRAY, NEW ZEALAND]	Oceanic variability is part of "internal climate variability" as defined in the FAQ, so this component of the comment is rejected. Land use and urban effects affect local climate but the FAQ addresses large scale change.
10-1035	10	71	43	74	10	FAQ 10.2: I think this FAQ is a good effort at explaining in relatively simple terms some quite complex ideas, and have no substantial changes to suggest. [David Wratt, New Zealand]	Taken into account - we have tried to keep the gist of our answer while condensing the text considerably for the SOD.
10-1036	10	71	45	74	13	Is this FAG more suited to Chapters 11 or 12? This is a projection/prediction question of sorts - unless the chapter can point to examples where detected human influence on climate is already apparent on local scales (e.g., tropical temperatures?). [Francis Zwiers, Canada]	Reject - FAQs are intended to extend across chapters, and the answer to this question explicitly involves comparison of forced long-term trends with current variability, which fits squarely within the purview of D&A (Ch 10)
10-1037	10	71	45	74		FAQ 2: This FAQ considers detection of CC at the local scale. It is suggested to expand this consideration to policy relevant questions in the context of adaptation as adaptation usually addresses the local scale. One relevant question might be the rationale to think about adaptation at the local scale long before detection of climate change is possible with some reasonable confidence at the local scale. Chapter 9 (evaluation of models) informs that there is some, although limited information about the skill of models to simulate the frequency and magnitude of extreme events. That information might be also considered in the important questions raised with respect to the rationale of adaptation to the impacts of climate change at the local scale, in addition to information provided in chapter 11 on projections and predictability. [Klaus Radunsky, Austria]	Taken into account - We mention extreme events in our answer, but it seems outside the purview of WG1 to delve too deeply into a discussion of adaptation.
10-1038	10	71	47	71	56	Human influences on the climate are around us all the time with our buildings, cities, agriculture; everything we do. Our main influence on the overall climate is in our interference with the cooling due to convecrion and water evaporation. The exclusive concern with emissions of greenhouse hases is misguided [VINCENT GRAY, NEW ZEALAND]	Reject - The focus here is on large scale climate change, not local changes. We do not concern ourselves exclusively with GHG; the point of attribution studies is to compare GHG-forced changes with other possible mechanisms for large scale change, including unforced variability.
10-1039	10	71	51	71	51	Please change to 'Recent studies', instead of 'recent assessments' to keep this important distinction. [Susan Solomon, USA]	Accept - This bit of text is removed altogether as part of our attempt to condense the text.
10-1040	10	71				FAQ 10.2, Fig 1: We consider the current map to be very problematic due to the use of country/regional boundaries, and the sometimes dramatic differences between adjacent regions (e.g. Indonesia red, Northern Australia green). We expect the term 'committed warming' will be a confusing and misunderstood term so we would favour removing the central map entirely. Instead we suggest a simple base map showing the locations linking with the time series plots positioned around the map. The 'emergence' could be indicated/highlighted on these plots. Please consider and ensure consistency with the results provided in Chapter 11, Fig 11.13. [Thomas Stocker/ WGI TSU, Switzerland]	Accept - The figure has been redrawn to remove political boundaries from central map, with new results based on CMIP5 projections used for the map and time series. We prefer to keep the depicted temperature change on the central map, at least for now pending feedback from the technical writer.
10-1041	10	72	21			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 2 be dropped ? [David Wratt, New Zealand]	Accept - References to other chapters have been removed
10-1042	10	72	44	72	44	The variability envelop moves with the mean - so "emerge" isn't quite the right word here. The question is when the range of variability experienced in some location will be different (not overlap??) with the range of variability felt in the current climate. [Francis Zwiers, Canada]	Taken into account - However "emergence" is the term used in the scientific literature to describe this feature of climate projections, despite the time dependence of the variability. The time series shown in Fig 1 shows exactly what the reviewer describes.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1043	10	72	48			Change "trends" to "trend" [David Wratt, New Zealand]	Editorial - typo fixed.
10-1044	10	73	10	73		The important impact on freshwater resources is left out although many of the anticipated impacts of climate change on society will operate through water. [Lena M. Tallaksen, Norway]	Taken into account - We do not intend to neglect freshwater resources but the principal examples of local attributable climate change (which has already happened or is imminent) are temperature-related. Attribution of hydrologic variables is much more limited, as discussed at length in Sec 10.3.2.
10-1045	10	73	27	73	28	This is too strong a statement, It might be true in the current climate (see the related FAQ in Chapter 3 of the SREX), but that doesn't necessarily mean it will also be so. [Francis Zwiers, Canada]	Accept - this wording will be changed in the revision.
10-1046	10	73	41	73	45	Seems to agree that the only :"evidence" is based on personal opinion, not on science [VINCENT GRAY, NEW ZEALAND]	Reject - Evidence is quantified, what is subject to personal opinion is just the definition of "obvious", a nonscientific term.
10-1047	10	73	50	73	50	Again it would be welcome to relate these considerations on the new RCP scenarios (instead of SRES). [Christian-D. Schoenwiese, Germany]	Accept - We are constructing a new figure based on CMIP5 simulations driven by the RCP scenarios
10-1048	10	75	1	99	2	At the end of many of these references there is ".-." I guess this is some odd typo. [Gareth S Jones, UK]	Formatting of references has been corrected.
10-1049	10	75	1	99	2	A number of the references have a "+" where the last page should be. [Gareth S Jones, UK]	Formatting of references has been corrected.
10-1050	10	78	36	78	37	use full initials of authors: Delworth, T.L., Mann, M.E., Observed and Simulated Multidecadal Variability in the Northern Hemisphere, Climate Dynamics, 16, 661-676, 2000 [Michael Mann, USA]	Formatting of references has been corrected.
10-1051	10	80	23	80	23	Typo in reference: "Frohlich" should read "Fröhlich". [Georg Feulner, Potsdam]	Formatting of references has been corrected.
10-1052	10	83	8	83	14	"Hegerl et al 2007b" this is such a good reference you have included it twice! [Gareth S Jones, UK]	Duplication eliminated.
10-1053	10	83	8	83	14	Duplication of the reference [Omer L. Sen, Turkey]	Duplication eliminated.
10-1054	10	86	20			Section 10.7.6 This would be more helpful if one could compare the spectrum of variability in the model simulations [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Spectra of variability in the models is included in Chapter 9 (CHECK) and is referred to in chapter 10.
10-1055	10	87	5	87	5	Lau and Kim - This like many of your references are incomplete. You really need to provide DOI numbers [Peter Clift, United States of America]	Formatting of references has been corrected.
10-1056	10	88	11	88	14	Use full initials of authors: Mann, M.E., Zhang, Z., Hughes, M.K., Bradley, R.S., Miller, S.K., Rutherford, S., Ni, F., Proxy-Based Reconstructions of Hemispheric and Global Surface Temperature Variations over the Past Two Millennia, Proc. Natl. Acad. Sci., 105, 13252-13257, 2008. [Michael Mann, USA]	Formatting of references has been corrected.
10-1057	10	88	15	88	16	Use full initials of authors: Mann, M.E. et al, Global Signatures and Dynamical Origins of the “Little Ice Age” and “Medieval Climate Anomaly”, Science, 326, 1256-1260, 2009. [Michael Mann, USA]	Formatting of references has been corrected.
10-1058	10	94	56	94	57	This reference is duplicated in the next page as "2004b" [Omer L. Sen, Turkey]	Duplication eliminated.
10-1059	10	98	39	98	40	Repeated citation and should be deleted. [Xiangdong Zhang, United States of America]	Duplication eliminated.
10-1060	10	100	10	101	5	An analysis of IASI satellite data has yielded TOA radiative forcing fluxes from CFC11 and CFC12 of 0.12 and 0.26 W/m2. W. F. Evans, Observations of Climate Radiative Forcing from Ground and Space, in Fourier Transform Spectroscopy, OSA Technical Digest (CD) Optical Society of America, (2009). This change in the energy balance of the atmosphere is entirely attributable to the activites of human society because the concentrations of these artificial gases were zero before 1930. The satellite fluxes are in agreement with overpass measurements taken at the ground at mid latitudes. The surface radiative forcing fluxes from CFC11,CFC12 and CFC22 have been measured at 0.11,0.24 and 0.012 W/m2 with an FTS at 45 N . Since these gases were non existant in 1750, the presence of these fluxes in the present represent an independent proof that man has altered the radiation balance of the atmosphere and	Noted but not relevant for chapter 10.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						increased the greenhouse effect. W.F.J. Evans and E. Puckrin, Observation of the Atmospheric Thermal Emission Spectrum of Dichlorodifluoromethane (CCl2F2), Can. J. Applied Spec., 39, pp 85-90, (1994).W.F.J. Evans, and E. Puckrin, An Observation of the Atmospheric Thermal Emission Spectrum of Trichlorofluoromethane (CFC-11), Geophys. Res. Lett., 21, pp 2,381-2,384, (1995). W.F.J. Evans and E. Puckrin, A Measurement of the Greenhouse Radiation Associated with CCl4, Geophys. Res. Lett., 23, pp 1,769-1,772, (1996). E. Puckrin, W.F.J. Evans, Jiangnan Li and H Lavoie, Comparison of Clear-Sky Greenhouse Fluxes Simulated With Radiative Transfer Models, Can. J Remote Sensing, 30 pp 903-912, 2004. These values are consistant with the NOAA AnnualGreenhouse Gas Index on the GMDL website.The index is computed with IPCC formulae from annual measurements of GHG mixing ratios. [Wayne Evans, USA]	
10-1061	10	100				Table 10.1 Could the respective sections associated with each statment be included? [Gareth S Jones, UK]	Accepted and done.
10-1062	10	113				Figures general: Please provide sufficient information on the original source in figure captions (applies to several figures in Chapter 10) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted and done.
10-1063	10	113				Figures general: Comment on multi-panel figures: we suggest to avoid combining too much information into single figures, unless there is a valid and logical reason for doing so of course (e.g. Fig 10.12 and 10.13) [Thomas Stocker/ WGI TSU, Switzerland]	Some of the multi-panel figures have been redrawn to be clearer and in some cases eliminating some information
10-1064	10	114	1	114	2	Figure 1 is too small [Helga Nitsche, Germany]	Accepted. The figure has been improved to high res image, made clearer and enlarged.
10-1065	10	114		114		Figures and labels are too small. [Seung-Ki Min, Australia]	Accepted. The figure has been improved to high res image, made clearer and enlarged.
10-1066	10	114				Figure 1 This figure is very poor, it is very difficult to understand what is being represented. [Gareth S Jones, UK]	Accepted. The figure has been improved to high res image, made clearer and enlarged.
10-1067	10	114				Box 10.1 Figure 1(b) I couldn't read the labels of the axes [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. The figure has been improved to high res image, made clearer and enlarged.
10-1068	10	114				Box10.1 Figure 1 © It would help to have the x -axis (horizontal line through (0,0)) drawn in solid [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Accepted. Done
10-1069	10	114				Poor figure quality, labeling is much too small [Larry Thomason, United States of America]	Accepted. The figure has been improved to high res image, made clearer and enlarged.
10-1070	10	115	1	115	1	In the third row, left figure, I cannot distinguish well the grey curves as in the upper two figures. Maybe, in producing this figure, red lines have been plotted after grey ones. Also I cannot see well grey lines in the right figures, second and third row, while in the first row they are very thin with respect to left figures. I think it may be better to standardize these figures. [Claudio Cassardo, Italy]	Figure has been revised with different colors making it easier to see the differences betweeh tne cmip3 and cmip5 ensembles
10-1071	10	115	4	115	7	Once more the "natural"; forcings do not include the most important influences on the record. They are the various ocean oscillations and the various urban and land use changes that have talen place. If these are included the two graphs are almost identical [VINCENT GRAY, NEW ZEALAND]	Rejected. The natural simulations do include ocean oscillations and other sources of internal variability as do all the simulations. Land use changes are included in many of the simulations including both anthropogenic and natural forcings. It is not the case that if the effects mentioned are included the two graphs are almost identical.
10-1072	10	115	4	115	12	This simulation has omitted the main influences on the mean global surface anomaly record. which are the ocean oscilations and the undoubted influence of urbanisation and land-use change. Ocean oscilllations had a pronounced upwards trend after 1970 which would explain most of the difference betrween the two records. Urbanisation and land-use change provided a positive bias throughout which is ignored. [VINCENT GRAY, NEW ZEALAND]	Rejected. The natural simulations do include ocean oscillations and other sources of internal variability as do all the simulations. Land use changes are included in many of the simulations including both anthropogenic and natural forcings. It is not the case that if the effects metioned are included the two graphs are almost identical.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1073	10	115		115		Is the net forcing estimation available for Natural forcing runs from CMIP3? [Seung-Ki Min, Australia]	We only have available to us the forcing for the CMIP5 runs.
10-1074	10	115				Figure 10.1 Why is the spread in net greenhouse gas forcing so wide? OR is this net anthropogenic forcing (including aerosols) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Fig 8.25a shows that the uncertainty comes from the non c02 well mixed ghgs.
10-1075	10	115				Fig 10.1. caption line 4 refers to this as temperature. It is _anomaly_. That needs to be explicitly stated. But essential also to plot temperature out of the models, not temperature anomaly, and compare to actual GMST. See Tredger E (2009) On the evaluation of uncertainty in climate models. PhD thesis, London School of Economics, London http://cats.lse.ac.uk/homepages/edward/TREDGER_Thesis.pdf; Figure 3.1 p. 71. 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. These figures show that the spread in GMST of AR4 models greatly exceeds the change in GMST over the twentieth century and indeed over expected temperature change in the 21st century, about 3 K This would have major effects on ice lines, vegetation, etc, and ultimately in climate response to forcing. So it is misleading to present only temperature anomaly and not temperature itself. The departures of modeled temperature from observations and its implications must be shown and discussed. [Stephen E Schwartz, USA]	Partially accepted. Text amended to refer to anomalies. The issue of global mean temperatures is assessed (CHECK) but we do not show absolute temperatures in the figure because the spread of the absolute temperatures of the cmip3 and cmip5 models (eg as shown in G.S.Jones et al, 2012) is not informative; for example the bias of the model's representation of absolute global mean temperature is not a predictor of a model's TCR.
10-1076	10	115				Given the concerns noted on page 10-15 lines 19 ff, it would seem imperative to present the forcings and sensitiviities of the models used in the figure and assess whether the anticorrelation between forcing and sensitivity found in the prior models obtains here. It is not enough to show the forcings. They must be identiified so that one can associate with specific models. Further, the sensitivities must be presented as well, again attributed. I suggest a Kiehl type plot (GRL 07) to assess any anticorrelation. Use the approach of Forster and Taylor to adduce the forcings and sensitivities. [Stephen E Schwartz, USA]	Rejected. The issue of the implication of any anticorrelation between forcing and sensitivity in the all forcings runs for attribution is assessed and found not to be critical for attribution so we have not added any additional figures.
10-1077	10	115				In assessing the ability of the current suite of models to reproduce the twentieth century, it seems essential to exercise each model over the best estimate range of forcings. For any given model this will result in a set of temperature trends over the twentieth century. Then repeat for all models and this will yield an accurate assessment of the ability of the current set of climate models to represent temperature change over the twentieth century. (This is elementary uncertainty propagation.) Failure to do this means that the process is again flawed and subject to criticism of selectivity in choice of forcing to get good match to the twentieth century. [Stephen E Schwartz, USA]	Rejected. The CMIP5 ensemble is an ensemble of opportunity. But nevertheless with a wider range of forcings and processes included there is a wider spread of temperatures simulated by the CMIP5 simulations with both anthropgenic and natural forcings than there was with CMIP3 simulations. The issue of possible model tuning is assessed in the chapter.
10-1078	10	115				May I recommend that whenever you have a time series figure such as this, you make publicly available a file that presents the data shown in the graph, together with identification of the model run or other identifier, This will allow intersted investigators to download the data and analyze them. Identification of which run comes from which model will also allow comparisons with forcings from those models. [Stephen E Schwartz, USA]	Along with the figure comes a detalied recipe of how it is produced. Ultimately repliction depends on downloading the identical data from the CMIP5 archive. The CMIP5 archive of data is publically avaiable to all interested researchers.
10-1079	10	115				Its almost impossible to see the gray forcings, top right panel. It is not clear that all forcings are referenced to the same time period as the temp changes; 1880-1919; they should be, and this should be specified. It looks like a factor of 6 spread among the forcings. Need to specifiy how many forcing data sets. And need to be identified by model. HiRes figure is no better. [Stephen E Schwartz, USA]	Partially accepted. The figure has been revised to make clearer. Models have not been identified as the intention here is to show the spread of the models and the differences between ensembles with different forcing combinaations. But temperature changes from individual models are shown in G.S. Jones et al, 2012.
10-1080	10	115				I am concerned that the forcings indicated in the figure and as used in the model runs do not represent the present assessment of these forcings. Examination of Chapter 8 (Forcing) shows that most of the key figures meant to indicate best current thinking on forcing are placeholders, suggesting that these best estimates were not available to the modelers in time to be incorporated into the model runs represented in Fig 10.1 At the very least the document needs to speak to the sources of the forcings used in the models and how they compare to the forcings adduced in the subsequent version of Chapter 8, and to the consequences and	Rejected. Forcings in models are within range of estimates of forcings from chapter 8 (Fig 8.25a)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						implications of any differences. [Stephen E Schwartz, USA]	
10-1081	10	116	4	116	9	See my note 151. [Claudio Cassardo, Italy]	Unclear what this refers to.
10-1082	10	116	4	116	9	I imagine the grey areas indicate absence of data available in the HadCRUT3 (or other?) dataset. Better to specify it in the figure caption. [Claudio Cassardo, Italy]	Accepted. Figure caption revised.
10-1083	10	116	4	116	9	Linear trends are unsatisfactory treatment of irregular climate data as it conceals variability. [VINCENT GRAY, NEW ZEALAND]	Rejected. Both timeseries and trends are shown in the chapter and space and time patterns assessed.
10-1084	10	116				Figure is OK but very small and hard to read without blowing it up. [Larry Thomason, United States of America]	Noted, but we need to show this information to illustrate the different time periods and the different forcings and observations.
10-1085	10	117	4	117	17	Once more the "natural"; forcings do not include the most important influences on the record. They are the various ocean oscillations and the various urban and land use changes that have talen place. If these are included the two graphs are almost identical [VINCENT GRAY, NEW ZEALAND]	Rejected.The natural simulations do include ocean oscillations and other sources of internal variability as do all the simulations. Land use changes are included in many of the simulations including both anthropogenic and natural forcings. It is not the case that if the effects mentioned are included the two graphs are almost identical
10-1086	10	118	14	118	14	Should be "HadCRUT2, HadCRUT3" not "HadCRUT2v, HadCRUT3v" [Gareth S Jones, UK]	Noted. Amended. Figure also updated.
10-1087	10	119	4	119	8	"... 17.5 year cycle, SAO and AO from Lean" these indices are not used in Lean and Rind GRL 2009. What are SAO and AO? Where have these come from and what is the 17.5 year cycle supposed to represent? The Lean method just uses solar irradiance, a volcanic index, ENSO index and an estimate of anthropogenic forcing from a climate model. Have extra indices been included in the regression for the IPCC? [Gareth S Jones, UK]	accepted, now clarified the terms
10-1088	10	119	7	119	8	"e) other factors": in this figure, it is not clear what is indicated in the plot: there are two curves (green and blue) but three variables are mentioned. [Claudio Cassardo, Italy]	Caption revised to clarify
10-1089	10	119				Figure 10.5: Should also include multivariate fits from Lean and Rind GRL 2008 doi:10.1029/2008GL034864 as that covers wider period than Lean and Rind 2009. See also Lean WIREs Clim Change 2010 1 111–122. [Gareth S Jones, UK]	To be determined
10-1090	10	119				It should be flagged somewhere that the estimate of the solar contributions seem to have a difference in several years of when the solar cycle peaks/troughs for the different methods, suggesting the methods may be over-fitting the forcing indices to the observations. [Gareth S Jones, UK]	To be determined
10-1091	10	119				Figure 10.5: There appears to be a much wider spread in the anthropogenic contribution than in any of the other contributing factors looked at. Does this suggest lower skill in calculating lower frequency contributing factors than higher frequency ones? [Gareth S Jones, UK]	To be determined
10-1092	10	120	0	120	0	Can the satellite data be added to this graph as chunks? [Susan Solomon, USA]	Rejected. There is an additional figure which shows the satellite data (from Santer et al, 2012 CHECK)
10-1093	10	120	2	120	7	Fig 10.6. This figure is misrepresentative and is actually a bit unreadable. If it remains the readers should we also see the same figure for 1979-2010 (or 2011 whatever) in which the main disagreements between the observations and models are evident during a period when the greenhouse effect is alleged to be strongest in elliciting an atmospheric response. Leaving out such a figure will provide justifiable criticism that the IPCC is cherry-picking to give models a break. We can be certain that the figure based on a 1979-2012 time frame will be broadcast far and wide no doubt to undermine the IPCC here if it is left out. Just be transparent and put it in (I note the stratospheric time series only begins in 1979.) [John Christy, USA]	An additional figure has been provided showing the comparison between models and satellite data over the satellite period.
10-1094	10	120		120		Adjusting colors consitentt with Fig. 10.4 (making natural forcing runs blue) would be helpful. [Seung-Ki Min, Australia]	Figure has been revised. (CHECK)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1095	10	121	4	121	11	please add 'in the lower stratosphere' to 'temperature anomalies'. [Helga Nitsche, Germany]	Accepted. Done.
10-1096	10	121				Fig 10.7 caption needs to say what variable is being considered closer to the start of the caption. It only ever refers to T4 temperature, but should say lower stratospheric temeprature. [David Karoly, Australia]	Accepted. Done
10-1097	10	122				Figure 10.8 The axis and the continent lines are difficult to see [Gareth S Jones, UK]	This figure has been deleted
10-1098	10	122				Figure 10.8 Which level? - near surface? [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	This figure has been deleted
10-1099	10	122				Figure quality is low, map outlines are too thin [Larry Thomason, United States of America]	This figure has been deleted
10-1100	10	122				Fig 10.8: the caption and figure are rather unclear. We suggest using titles to make it clear what the individual panels and columns are showing. [Thomas Stocker/ WGI TSU, Switzerland]	This figure has been deleted
10-1101	10	123	16	123	17	Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think those results should be removed from this figure, & so the relevant part of this caption also removed. [William Ingram, UK]	Rejected. The Min et al results are included for completemess and their robustness is assessed in the text.
10-1102	10	123				Figure 10.9: top right panel. Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think those results should be removed from here [William Ingram, UK]	Rejected. The Min et al results are included for completemess and their robustness is assessed in the text.
10-1103	10	123				Figure 10.9 Upper right panel, what do "ALLc","ALLz" and "ALLv" represent? [Gareth S Jones, UK]	Caption revised to clarify
10-1104	10	123				Figure 10.9 Upper left panel, what does "ALL4" represent [Gareth S Jones, UK]	Caption revised to clarify
10-1105	10	123				Upper right figure labeling is too small [Larry Thomason, United States of America]	Figure revised to be clearer.
10-1106	10	125	12	125	12	How are is the confidence range of the ensemble mean trends caculated - it is not mentioned in Gillett 2005? Why is it interesting? [Gareth S Jones, UK]	Information has been provided explaining this (CHECK)
10-1107	10	126	4	126	17	Some consistency of the use of "ALL" would be helpful here, "ALL", "All" and "all" are used. Note any major differences in the make up of "all forcings" e.g. some models have indirect aerosol/land use and others don't - or refer to somewhere else that discusses this? [Gareth S Jones, UK]	Noted. Text and captions make consistent (CHECK)
10-1108	10	126	15	126	17	Does "SS" in the middle panel represent "sub-sampled"? [Gareth S Jones, UK]	Yes. Figure revised.
10-1109	10	126		126		Please check if same colors have been applied for b and c. They are confusing as is. [Seung-Ki Min, Australia]	Figure revised.
10-1110	10	126				Figure 10.12 What is black line in top panel? [Gareth S Jones, UK]	Caption revised to clarify
10-1111	10	126				Figure 10.12 Middle panel - This is a very complex figure to include. Would it be possible to summarise it to just show three bars (e.g. NoV_C, ALL_C, V_C)? [Gareth S Jones, UK]	Accepted. Fewer bars shown. (CHECK)
10-1112	10	126				Key labeling in middle figure is too small; lower figure is fuzzy [Larry Thomason, United States of America]	Figure revised.
10-1113	10	127	4	127	21	Are the 20C3M models used forced with "all" forcings or "anthropogenic only" forcings, The caption is ambiguous. [Gareth S Jones, UK]	Caption revised to clarify
10-1114	10	127	7	127	13	There is not enough details about what this figure, what do orange and pale blue represent? The gradients in the figure represent the amplifications I presume? [Gareth S Jones, UK]	Caption revised to clarify
10-1115	10	127	18	127	19	I guess if this figure is taken from a paper that the following should have been noted then but ... if higher truncations are needed for a couple of the cases why not use higher truncation for all the cases? [Gareth S Jones, UK]	Rejected. This is discussed in the paper (CHECK). There is a limit to what extra information we can provide in the chapter.
10-1116	10	127				Figure 10.13 (A) top and middle panels should be on same scale - currently top has y axis zoomed in compared to middle panel. [Gareth S Jones, UK]	Figure redrawn to be clearer (CHECK)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1117	10	128		128		Adding multimodel mean and inter-model uncertainty would be helpful to inteprete signal and noise comparison with observations. Updating using CMIP5 would be also recommended. [Seung-Ki Min, Australia]	Figure has been extensively revised and will include CMIP5 models.
10-1118	10	128				Figure 10.14: Could the model name in bottom left hand corner be corrected to "HadGEM1"? [Gareth S Jones, UK]	Yes. Figure revised.
10-1119	10	128				Figure 10.14: The magenta dashed lines are difficult to distinguish from the blue lines. [Gareth S Jones, UK]	Figure has been extensively revised.
10-1120	10	128				light colored lines are too thin or a darker color should be used [Larry Thomason, United States of America]	Figure has been extensively revised.
10-1121	10	129	4	129	13	I don't think this is the right place for this but is it possible (in the glossary?) to somewhere carefully explain the differences between a minimum maximum and maximum minimum etc.? I personally find it tricky to remember what they represent. [Gareth S Jones, UK]	Rejected. This is not something for the glossary. Caption has been clarified though.
10-1122	10	129				Important figure but the individual plots are much too small; maybe ditch the map and increase the individual plot size by at least a factor of 2. [Larry Thomason, United States of America]	Figure has been revised with bigger labels.
10-1123	10	129				Fig 10.15: We suggest to consider to adapt the format such that the map is enlarged (full landscape page), and each plot positioned appropriately over top of the relevant region on the map. [Thomas Stocker/ WGI TSU, Switzerland]	Figure has been revised. (CHECK)
10-1124	10	130	1	130	1	As the Figure 10.16 is never recalled in the text, there is not a direct explanation of it. I notice that the observed lines (OBS) possess a more evident trend than the models, being in all cases the lowest at the beginning of the period and the highest at the end. The models show a more flat behavior for _ANT and almost horizontal lines for _ALL. Could you please explain better this figure? [Claudio Cassardo, Italy]	Figure has been deleted.
10-1125	10	130				Figure 10.16. Given the problems with the Min & al (2011) results raised in my comments on ll 43-44 of 10-28, I think this figure should be entirely removed [William Ingram, UK]	Accepted. Figure removed.
10-1126	10	130				Figure 10.16 It is not at all clear what the extreme indices RX1D and RX5D are? [Gareth S Jones, UK]	Figure has been deleted.
10-1127	10	131	8	131	8	"colours" - only green is shown to represent the models. [Gareth S Jones, UK]	Caption revised to clarify
10-1128	10	131	11	131	15	Is there a reference for panel c? [Gareth S Jones, UK]	Yes. Otto et al reference added.
10-1129	10	131	15	131	15	"the horizontal red arrow": this arrow is hardly visible. [Claudio Cassardo, Italy]	Figure revised to be clearer.
10-1130	10	131				labelling could be a little bigger [Larry Thomason, United States of America]	Figure revised to be clearer.
10-1131	10	131				Fig 10.17: Caption states 'Colors correspond to…'; please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Error in caption corrected.
10-1132	10	132	4	133	6	A "supplement" is mentioned twice in the caption. Is this actually available to reviewers and the final readers of this chapter? [Gareth S Jones, UK]	There is a supplement which will be available to all reviewers of the sod.
10-1133	10	132	9	132	11	Is the regression actually done on annual mean data or some other meaning? [Gareth S Jones, UK]	Figure revised to be clearer.(CHECK)
10-1134	10	132				Fig.10.18 This caption is hard-going! If the model mean fingerprint is scaled to fit each reconstruction in turn shouldn't there be 3 grey curves in top panel and 1 or 2 in bottom panel. Sorry if I'm being stupid. [Joanna Haigh, UK]	Caption revised to clarify
10-1135	10	134				Figure 10.19 Same comment as above about selected time interval. [Richard Keen, USA]	Figure deleted.
10-1136	10	134				Figure 10.19 What is WA?,SSJ? CT?, SWE/P (soil water equivalent?) [John Mitchell, United Kingdom of Great Britain & Northern Ireland]	Figure deleted.
10-1137	10	135	10	135	10	Should it be "AOGCMs"? And last word "tom" -> "top"? [Gareth S Jones, UK]	Typos corrected.
10-1138	10	135				Figure 10.20. Figure 10.20 is the most important figure in the whole chapter, dealing as it does with transient and equilibrium climate sensitivity, the levels of which are critical to how much warming an increase in	Figure has been updated and revised. (CHECK)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						greenhouse gas concentrations should cause (and therefore how much of a threat AGW is, bearing in mind that a warming of 1-2 C might very well on balance be beneficial). At present, Figure 10.20 is so small that the individual graph lines are almost indistinguishable, and black lines are indistinguishable from blue lines even with a magnifying glass. I suggest dividing Fig. 10.20 into separate TCR and ECS sections (the latter taking up a whole page) and making the body of each graph take up the full page width (for TCR) and two thirds of the page width (ECS), with the five coloured squares in the ECS section made smaller and closer spaced so that they fit into 1/3 of the page width. Also, suggest adding keys to the (much larger) ECS graphs to identify which graph line is derived from which study, as with the TCR section, rather than stating this below the figure. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-1139	10	136	4	136	4	"satellite" [Gareth S Jones, UK]	Typo corrected
10-1140	10	136	8	136	8	"millennium" [Gareth S Jones, UK]	Typo corrected
10-1141	10	136	8	136	8	Any reference for the "(blue)" for the "millennium/volcanism" plot? [Gareth S Jones, UK]	Caption revised to clarify
10-1142	10	137	5	137	5	I couldn't find in the list of references "National Research Council (2011)" [Gareth S Jones, UK]	Reference inserted.
10-1143	10	137	6	137	6	Correct reference for HadCRUT3 is Brohan 2006. [Gareth S Jones, UK]	Corrected. Thanks.
10-1144	10	137				FAQ10.1, Fig 1: Top left panel - Dataset should probably be "HadCRUT3" [Gareth S Jones, UK]	Corrected. Thanks.
10-1145	10	137				FAQ 10.1, Figure 1 Same comment as above about selected time interval. [Richard Keen, USA]	Taken into account - figure (and caption) to be thoroughly revised for the SOD (revision in progress)