Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1	10	0	0	0	0	The recent trends in global temperature (or absence of) since 2000 needs to be explained in this chapter in the context of known natural variability. It is clearly evident in several figures (e.g. 10.1, 10.6, 10.20) that whereas the models can explain every single multi-year events since 1960, they cannot explain the 0 temperature trend since 2000. This chapter needs to hone in on processes during this time period, not just by saying that this is within known natural variability, but by explaining the cause. Perhaps the simplest way to do this would be to isolate the contribution of ENSO to global temperature trend, as was done by Thompson et al Nature 2008. [European Union]	Taken into account. A new box has been developed and cited in Chapter 9, which considers the hiatus in global temperatures over the last 15 years.
10-2	10	0	0	0	0	The recent trends in global temperature (or absence of) since 2000 needs to be explained in this chapter in the context of known natural variability. It is clearly evident in several figures (e.g. 10.1, 10.6, 10.20) that whereas the models can explain every single multi-year events since 1960, they cannot explain the 0 temperature trend since 2000. This chapter needs to hone in on processes during this time period, not just by saying that this is within known natural variability, but by explaining the cause. Perhaps the simplest way to do this would be to isolate the contribution of ENSO to global temperature trend, as was done by Thompson et al Nature 2008. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account. A new box has been developed and cited in Chapter 9 which considers the hiatus in global temperatures over the last 15 years.
10-3	10	0	0	0	0	General comments: Much detection and attribution studies summarized are of GHGs, other human-activity contributions such as aerosol and land cover changes mentioned little. Particularly when talking about the uncentainty, these might be important factors for climate change. Temperature extremes caused by other human-activities such as soil moisture, vegetation change, and urbanization seems ignored (Zhang JY et al., JGR) [Xuemei Shao, China]	Taken into account. The effect of land use/land cover change as well as urbanization are assessed. They can be quite large as local scale, however, it is relatively small when compared with the effect of greenhouse gases, at least in mean temperature (Section 2.4.1.3). So this does not invalidate our conclusions. In the text, have added " Urbanization may have also affected extreme temeprtaures in some regions (e.g. Zhou and Ren 2011)." After discussion on Christidis et al (2012).We havel also added "The effect of land use change and urban heat Island is found to be small in the global average temperature (Section 2.4.1.3). Consequently, this effect on extreme temperature is also expected to be small in the global average."
10-4	10	0	0	0	0	Overall, the assessment on sun's role in climate change is also less convincing and unbalanced. There are many studies investigating the possible influence of solar activity on earth climate, and it is generally hold that the influence could not be overlooked. Not only in Europe but in eastern Asia including mainland China, weak solar activities are usually related to the cold winters and cool climate condition in varied time scales (e.g. Holmes, et al., 2009; Woollings, et al., 2010; Ineson, et al., 2011). Certain association may exist between the solar drive and multi-decadal ocean oscillations which in turn affect the global and regional surface temperature. Considering possibly larger internal variability and the likely influence of solar activities, it is probably improper to use "extremely likely" and "extremely unlikely" for the conclusions drawn in this subsection. [Xuemei Shao, China]	Taken into account. A new box has been included in the chapter which considers specifically the solar influence on climate where this of relevance to detection and attribution of climate change from global to regional scales.
10-5	10	0	0	0	0	It would help reading and interpreting the text, when there would be a list with the terms judging the quality/confidence of the statements (liekely, very likely, etc.) in an order from highest confidence to lowest (evt. with some explanations when which term was used). [Roman Zweifel, Switzerland]	Taken into account. The chapter is using the standard IPCC terminology which is described in Chapter 1 but a footnote has been added at the start of the chapter to explain IPCC confidence language.
10-6	10	0	0			To assume that ALL change is anthropogenic for greenhouse gases is as bad as assuming that all change in climate is anthropogenic. This chapter slips back and forth between stating what was actually done in the D&A and what was a hidden assumption. It seems that only the observed change in GHG is what is used in these studies. No effort is made (nor are there ready publications to justify) that exactly 100% of this is 'anthropogenic'. If the observed GHG change were postulated (by expert judgment?) to be 100%+-10% anthropogenic (a reasonable choice) and this uncertainty were truly propagated through the Chapter 10 analyses, then we would be in great shape, and justified perhaps in making this statement. Although the expert judgment would need some backup from chapters 5 & 6. Given pre-industrial variations, we can expect	Taken into account.The ghg variation in the LIA is a few PPM, see chapter 6 while the overall increase is much larger. A reference is made to chapter 6 in rthe revised chapter to support a statement that natural fluctuations in ghgs are very small relative to the anthropogenic increase.

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						natural cycles to continue. These need to be quantified. [Michael Prather, United States of America]	
10-7	10	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Values have been cross checked.
10-8	10	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
10-9	10	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. ES has been revised to conform to the recommended style.
10-10	10	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
10-11	10	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
10-12	10	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
10-13	10	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
10-14	10	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
10-15	10	0				Some consistency needs to be applied across Ch 2, 9,10,11,12,14 to the index names used for the extremes indices. For instance, annual maximum 5-day rainfall is referred to as R5dmax in Ch 12, RX5day in Ch 9, and R5d in Ch 14, and the warmest 10% of nights as TN90 in Ch10 and TN90p in Ch 2. This should be coordinated amongst all relevant chapters. [Lisa Alexander, Australia]	Taken into account. Ch10 now use index names agreed by all Chapters.
10-16	10	0				As first noted in my comment on P4 L8, the entire chapter has trouble adhering with strict correctness to the IPCC's definitions of detection and attribution. "detection of anthropogenic influence" there, "detection of human influence" at P4 L10, and dozens of similar usages, assign a cause to (that is, attribute) something that is supposed not to have been attributed to anything yet. The trouble is that, unlike "to detect", in English you cannot use the verb "to attribute", or its equivalent nouns, without an adverbial ("to X") phrase. "detection of anthropogenic influence" is a much more natural usage than "attributability to anthropogenic influence of detected ...". But if the Detection and Attribution chapter finds itself unable to use the two verbs and their nouns "correctly", perhaps the IPCC as a whole needs to reconsider the two definitions. The alternative is to	Taken into account. Revisions have been made to ensure the chapter adheres to the Hegerl et al GPGP.

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						go through the text altering all the non-compliant phrases; in some places, a near-synonym of "detection" such as "identification" or "finding" might be useful. See also P6 L4-5 and P8 L22-28. [J. Graham Cogley, Canada]	
10-17	10	0				AR5 WGI Ch10 and WGII Ch18 have no authors in common. This allows for potentially awkward inconsistencies which could make for extra work at a late stage. A possible example is the extended definition of attribution at P8 L43. [J. Graham Cogley, Canada]	Rejected. The definitions in the GPGP were agreed a t a WG1/WGII meeting and are being adopted by both WGs in the AR5.
10-18	10	0				I have reviewed those parts of chapter 10 to do with extremes and I think the autors generally did a very good job. Only the confidence levels assigned to trends in and attribution of heat waves / warm spells is set too low [Dim Coumou, Germany]	Noted.
10-19	10	0				I was reviewing only the Intro and the Methodological Sections. [Tibor Farago, Hungary]	Noted.
10-20	10	0				Misprints etc. introduced by * [Tibor Farago, Hungary]	Noted.
10-21	10	0				General feelling after reading: Nice work, and a little worry that much detection and attribution studies summarized are of GHGs, other human-activity contributions such as aerosol and land cover changes mentioned little. Particularly when talking about the uncentainty, these might be important factors for climate change. [Daoyi Gong, China]	Noted. However, there is quite extensive discussion of aerosol and land use changes in the chapter with attribution of other anthropogenic effects being a key consideration.
10-22	10	0				This chapter uses a number of different expressions for attributing warming to anthropogenic causes. P.3, line 6 'resulting primarily from anthropogenic increases…', p.3, line 21 'more than half' for ocean warming, p.3, line 34, 'caused most of (at least 50%)' for average global temperatures. The term 'most of (at least 50%)' is very difficult for a policy maker to interpret and appears contradictory and weak. Consistent clear language is needed. With the current combination - 'primarily' appears as the easiest to interpret. [Government of Australia]	Noted. It is most important to be consistent in assessment across the chapter and to be quantitative (ie to be explict what amount of warming is being attributed). Revisions have been made in chapter to help ensure this.
10-23	10	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	A footnote(common to all chapters) has been added at the start of the chapter explaining the use of confidence and likelihood language.
10-24	10	0				We notice that when speaking of warming, the temperature increase is written in Kelvin some of the time and not Celsius. That's inconsistent throughout the chapter. Also, the first time Kelvin is mentioned in the report is on page 10-43 (I searched the word), but K is used for temperature change throughout chapter 10 and perhaps elsewhere. [Government of United States of America]	Taken into account. Usage is unified around degrees Celsius.
10-25	10	0				This chapter is well written and short. Section 10.6.2 on extremes is especially clear. The discussions on the last 10 to 15 years and temperature levelling, for example in 10.3.1.1.1 and 10.3.1.1.3 are, however, weak and inconclusive [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. In the revised report the discussion of the lsat 10 to 15 years has been addressed by including a new box in chapter 9 that discusses the last 10 to 15 years.
10-26	10	0				My impression from the FOD is confirmed that this chapter is sound, and existing literature carefully assessed and adequately reflected. Congratulation to the authors. It is also a very data/information rich chapter. In the following I describe some general thoughts, comments or suggestions on this chapter: (1) volume of text: the material compiled is fascinating and I enjoyed reading but for an Assessment Report I think it is too long. The main messages of the chapter would benefit if text was shortened and streamlined, a reader may get lost in the breadth of the chapter. I recognize, however, that virtually all material is of relevance but some of it could possibly be published in some other form (?). The complexity (2) complexity: I feel the complexity of the text is at the upper limit, at least at some points and for an AR. Including some figures, such as for instance Figure 10.19 which is not easily understandable and also has too much information. I think the D&A language of physical climate science has become very fine-tuned with a lot of nuances that are highly relevant scientifically but not most likely will not be captured by a larger community, within science but certainly not outside (media, policy level). I recognize it is a reflection of the literature, so the issue also lies in the way studies are published. Being a LA of WGII chapter 18 on D&A, we experience a similar problem that a larger science community often does not properly understand the issues and methods of D&A which partly has to do that in WGII it is still a young field of research. (3) target audience: both previous points relate to the target issues of this chapter (and the report). I think it is worthwile to reconsider in which respects the current text fits the target	Noted. Some shortening has taken place while noting that sufficient length is required to properly assess a wide ranging scientific literature. In revision care has been taken to improve clarity of language while also noting that a certain technical level of language is required in the chapter body, eg to be consistent wit the GPGP on detection and attribution. However we do have two FAQs that address two of the main questions concerning detection and attribution and which are directed at a more general audience. We have also sought to simplify the figures where possible and we havemade clearer the synthesising figure 10.21 which brings together information from across the chapter. Cross referencing to the WGII SOD has been carried out.

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						audience and where it is beyond, and in what percentage. (4) links to WG II chapter 18 on detection and attribution: at some points it would be helpful to make reference to the corresponding WGII chapter, e.g. where the logic of the subjects proceeds into impacts. We are doing the same in the WGII which in general is desirable in order that the reader recognizes the consistency of the whole report (and by the way we greatly appreciate the review comments we received from WGI). [Christian Huggel, Switzerland]	
10-27	10	0				Climate simulations taking account of only solar output changes are inadequate to explain the effect of solar changes on the climate system. For instance, Nir J. Shaviv (J. Geophys. Res., 113, A11101 (2008), doi:10.1029/2007JA012989, “Using the oceans as a calorimeter to quantify the solar radiative forcing”) showed that although the influence of solar activity changes is clear, it is impossible to attribute the influence to solar output changes only, and hence, some (unkonwn) amplification mechanism is needed. [Kiminori Itoh, Japan]	Noted. The chapter includes a new Box 10.2 which assesses the sun's influence on climate.
10-28	10	0				Possible climate changes associated with the cosmic ray are discussed in the report, but the influence of the solar activity change does not limit to the cosmic ray. In fact, the following paper points out the existence of possible amplification mechanisms for the solar change effect on the climate: Nir J. Shaviv (2008); Using the oceans as a calorimeter to quantify the solar radiative forcing, J. Geophys. Res., 113, A11101, doi:10.1029/2007JA012989. [Kiminori Itoh, Japan]	Noted. The chapter includes a new Box 10.2 which assesses the sun's influence on climate.
10-29	10	0				Chapter reads well and figures are clear. Considerable improvement compared to the FOD. [Albert Klein Tank, Netherlands]	Noted. Thanks.
10-30	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. [Fredrik Ljungqvist, Sweden]	Noted. Chapter 5 and 10 have collaborated extensively on that section, also for the final draft to account for this comment. The reconstruction Christiansen and Ljungqvist is now cited.
10-31	10	0				There is a new paper which looks at emergence of signal in observations "Emerging local warming signals in observational data" by Irina Mahlstein, Gabi Hegerl, and Susan Solomon that is accepted at GRL. It would be nice to when it would get mentioned. [Irina Mahlstein, Switzerland]	Accepted. Text revised.
10-32	10	0				This chapter, which is one of the most important of the WG1 contribution, is in excellent shape and provides a clear and coherent storyline. In particular the new Section 10.6.2 on attribution of single events is very important and makes for fascinating reading. My comments are almost exclusively directed at improving the clarity of the text. [Jochem Marotzke, Germany]	Noted. Thanks.
10-33	10	0				Despite my generally very positive reaction, I think that the "scene-setting" could be improved substantially. The Executive Summary, Introduction (10.1) and Methodology (10.2) sometimes are neither very precise nor very concise. But both are very important for this chapter, because it is likely again to draw focused attention. [Jochem Marotzke, Germany]	Taken into account. The ES is substantially revised, to be more precise and concise by structuring it around key statements with supporting evidence and tracing it back to the body of the chapter. Section 10.1 has been shortened and sharpened. Secion 10.2 has been revised to make it more precise including considerable development of the Box 10.1 figure.
10-34	10	0				Appendix A to the IPCC Procedures says "In preparing the first draft, and at subsequent stages of revision after review, Lead Authors should clearly identify disparate views for which there is significant scientific or technical support, together with the relevant arguments." (4.3.3) and "It is important that Reports describe different (possibly controversial) scientific, technical, and socio-economic views on a subject, particularly if they are relevant to the policy debate." (4.3.5). This chapter fails to discuss views other the assumption on which the IPCC was established, i.e. that CO2 is to blame. The omission should be corrected because if it remains this report will likely be regarded as the view of lobbyists, which will detract from some quite reasonable science. Any response about a lack of space will not be tolerated because failure to design the chapter to cater for this is contrary to the Procedures document. [John McLean, Australia]	Rejected. The chapter does assess scientific literature that considers both natural and anthropogenic factors as possible explanations for observed climate changes. It does not start from the a priori assumption that aCO2 is to blame. The chapter identifiies where there is strong evidence for the role of natural factors, for example the effect of volcanoes on ocean heat content, and considers effects such as the AMO in detail.
10-35	10	0				(Chapter as a whole) Unless the papers cited in this chapter can be shown to accurately incorporate all climate forces they should be disregarded, in fact competent reviewers of these papers should have either	Rejected. The chapter considers known climate forcings and takes into account model biases in its

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						rejected them or demanded comprehensive statements as to what the climate models simulated and how thoroughly. The IPCC does everyone a disservice by citing the output of papers based upon flawed models. (The repeated failure of predictions based on climate models is further evidence that the models are flawed and have no credibility.) [John McLean, Australia]	assessment.
10-36	10	0				(ignore this comment - it was a line accidentally left blank in the comments spreadsheet and without something here the processing of my comments may have terminated her) [John McLean, Australia]	Noted.
10-37	10	0				I don't agree with the authors' interpretation of the charge for chapter 10. It appears they have taken the stance of focusing almost exclusively on the formal detection/attribution literature. That is too narrow an interpretation in my view. I would think that the governments would want to see, in ch. 10, an assessment of all the factors that may have contributed to the global and regional patterns of climate change that occurred since about 1900. [Gerald Meehl, United States of America]	Taken into account. A new Box 10.2 on solar forcing on climate has been added that draws on a variety of sources. Note however the detection and attribution methodology provides a clear standpoint for assessing the causes of observed changes and highlighting those aspects where a human fingerprint has been clearly detected in the observed record. Nevertheless we have made an effort to use a broad range of literature - we use the D+A literature as key supporting methods to determine if changes could be explained by variability and can be attributed but the chapter is not only about D+A results in a narrow sense. There are many places where we make indirect assessments on related variables (eg sea level via heat content; or use results connecting variables or linking dynamically (eg in the sea ice section) to explain what has been detected physically, and use information on what is connected dynamically to explain changes. Nevertheless, a detailed discussion of the many possible causes of every possiblepiece of regional variability would lead to a very long chapter that would not meet the main charge of this chapter, namely to identify those aspects of change where a human or natural fingerprint has clearly emerged from the noise of internal variability.In summary, consistent with previous assessments, the primary focus of this chapter is on attribution of factors external to the climate system.
10-38	10	0				For example, I get asked all the time if global warming is just internal climate variability, or if global warming is all the sun. It would seem that ch. 10 would be compelled to assess the literature that addresses these contributions to the climate fluctuations we have seen over the past 100 years. What they call noise in a D and A sense is in fact what quite a few people think is driving climate change. [Gerald Meehl, United States of America]	Noted. But the chapter does assess whether changes can be attributed entirely to internal variability or to the sun, as well as looking at other possible contributors.
10-39	10	0				Thus, my answer for "isn't it all the sun" is to say that globally there isn't much contribution (which is all they cover at this point in ch 10), but that regionally the sun does produce measurable signals. To me this is an important distinction. As ch 10 is written now, it is dismissive of solar forcing because there is no global signal. While that may be true, and that's all you need to know for D and A, it is worth noting there are regional signals from solar. [Gerald Meehl, United States of America]	Taken into account. The chapter has been revised to include a box discussing detection and attribution of solar forcing on climate from global to regoinal scales.
10-40	10	0				Same story for the IPO/PDO. Indeed, it's decadal timescale noise for D and A, but once again it seems to me that governments would want to know that while there is not a significant global signal, there are indeed quite significant regional signals of this decadal timescale variability that influenced observations of 20th century climate, and will continue to influence regional climate change in the future. That then provides a lead-in to ch. 11. [Gerald Meehl, United States of America]	Noted. But the main charge of this chapter is to assess where regional changes are detected as being outside the range of internal variability and where a substantial fraction of the observed changes can be attributed to natural and anthropogenic forcings. To do this does requre an assessment of internal variability

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							and the extent to which it could explain regional changes but to cite every piece of literature providing a description of the possible contributions of modes of variability to regional climate changes would make for a very long chapter without benefit to the central charge of our chapter.
10-41	10	0				The chapter has been strenghtened by addition of a section on remaining uncertainties in the summary. The addition of summary statements at the end of section is also helpful, but would be more useful if in the longer section it could lis briefly the key factors leading to the chosen level of likelihood/confidence [John Mitchell, United Kingdom]	Taken into account. The key factors are listed in the Table 10.1 from the sod which has been revised to improve clarity and to include more clearly these key factors leading to an assessment including the remaining uncertainties.
10-42	10	0				Overall the chapter is clearly written and focused. I have however a few general concerns: some of the statements could (should?) be more a bit more conservative than currently written (see examples below) . Another one is that there is still some kind of confusing mix between the terms detection and attribution all along the chapter due to the evolving definition of attribution (including now detection of a given specific forcing). A last one is the "remaining challenges" section which deals almost exclusively with the multivariate detection problem. I would very strongly suggest to include another paragraph on optimal fingerprinting methodological issues as mentioned for instance in Jones et al. 12, Ribes et al. 2012 and Ribes and Terray 2012: dimension reduction, covariance matrix estimation, residual consistency test and hypothesis of correct forcing response patterns (when including spatial dimensions) as well as additivity assumptions. [Laurent Terray, France]	Taken into account. Chapter has been revised to ensure compliace with IPCC GPGP terminology. There is discussion of the Ribes methodology in Section 10.2 and the chapter now makes use of the latest results from Ribes et al, now that the papers have been accepted.
10-43	10	0				The Chapter is replete with filler 'fluff' lines at the start of each section. E.g. Section 10.3 starts 'This section assesses causes of change in the atmosphere and at the surface over land and ocean.'. First, a single sentence paragraph is terrible grammar. Second, even for the hard of thinking this is implicit in the title of the section. All these little filler pieces do is serve to break the flow and annoy the reader. They should be deleted throughout to improve readability. [Peter Thorne, United States of America]	Taken into account. We have sought to provide a more helpful explanation at the start of sections. Note that according to the structure designated for chapters we are obliged to have some text sitting between section titles and subsection titles.
10-44	10	0				There are differences in the datasets used in this Chapter and the observations chapters which should be reconciled. Case in point this chapter frequently for surface temperatures alludes to and uses the JMA surface temperature analysis. Yet to Chapter 2's knowledge there is no peer reviewed article basis describing this product. One of two things need to happen here: 1. Chapter 10 drops use of such non-peer reviewed products (and also explicitly cites papers on first reference to a data product - not currently done everywhere) or 2. there exists a peer reviewed paper and so Chapter 2 then includes the JMA analysis. This is solely a case in point but it extends more generally. Chapter 10 needs to ensure it is using the same observational products as the preceding observations chapters. Where it is not a decision needs to be made by consultation with the relevant observational chapters which removes this discrepancy either through inclusion in the observed chapters or exclusion from Chapter 10. Otherwise this constitutes a glaring inter-chapter discrepancy which will be picked up upon and used to discredit one or other of the chapters and perhaps the report as a whole by vested interests. [Peter Thorne, United States of America]	Accepted. Chapter is revised to contain references only to cited material and in figures to only use peer reviewed datasets. As a result we have dropped JMA from figures. We still include peer reviewed studies that analyse the JMA datasets but assessment is not affected by inclusion or exclusion of JMA dataset.
10-45	10	0				This chapter as a whole seems to have been written under a broader remit than what I would interpret to be its remit given its title. There are whole paragraphs and even sections that either mainly or entirely discuss observations, model processes or model projections. This is section-variable - some sections stray little from central charge whereas others contain little or no detection and attribution material and therefore should be shrunk substantively. There are chapters dedicated to these non-d&a related issues and it seems both dangerous and unnecessary to discuss these issues in this chapter. This chapter should concentrate upon issues of detection and attribution and avoid issues that are more within scope of other segments of the report. This will remove bloat and make for a more readable and focussed chapter whilst also avoiding the potential for inter-chapter conflicts of interpretation. Doing this will make the final draft preparation easier and not harder. [Peter Thorne, United States of America]	Taken into account. The chapter has been revised to ensure consistency of treatment across aspects of the climate system dealt with in the various sections of the chapter. Note that concentrating on issues of detection and attribution will mean that the chapter is unable to deal with the wider remit requested by some reviewers - eg see comment 10-40.
10-46	10	0				It feels very odd not to have a section that summarizes in a holistic sense what the sum totality of the discussion in Section 10.8 is for the issues discussed in 10.8 as a whole. It would be nice to have a section	Rejected. We provide this information both in the Synthesis Table and at appropriate places within the

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						10.8.5 that bought together the principle conclusions from the section as a whole that could then be brought through to the chapter summary. The same issue applies elsewhere in the chapter potentially. TSU guidance was that each N.n should have a closing summary section. This should serve to increase readability of the chapter as a whole and impact of each main section. At the end of each of 10.2 thru 10.8 there should consistently be a short summary taht brings out teh key findings and conffidence that can then be pulled through to the Chapter summary. [Peter Thorne, United States of America]	chapter where we sum up the evidence for a particular aspect so it would be duplication to include this information again at the end of each section.
10-47	10	0				Many of the figures are very information dense and therefore of questionable intelligibility to a non-expert reader. Wherever possible a simplified version should be pursued. If necessary the originals can be retained in the appendix, but several of the figures even as a nominal expert in both the science anda passable expert in teh sub-discipline I found very hard to read and interpret. This starts to ring alarm bells about how accessible these might be to the target chapter audience and is why I think a cold hard analysis of the figures and paring out of unnecessary detail would be advisable. There are important findings in here but in many cases they are being buried from simple interpretation by the level of detail. A couple of examples: Does Figure 10.15 really need to be 13 panels or can this information be simplified and distilled into a couple of panels? Figure 10.14 seems to be trying to say several things at once and is a PhD in its own right just to understand it. Figures where the results are from regression - perhaps shading to denote detection region, a line across at 1 to denote when 'consistent' etc.? A little more help in making these figures more accessible in general to non-experts would be incredibly valuable to making this chapter much more valuable as a resource to experts and non-experts alike. [Peter Thorne, United States of America]	Taken into account. Revisions have been made to most of the figures to improve them by making them clearer.
10-48	10	0				Overall, I felt this chapter did a good job in capturing the state of detection and attribution science. A lot of attention has been paid to uncertainty which makes for a consistent treatment of the conclusions.There remains a degree of uneveness between sections which if improved would greatly improve readability. [Peter Thorne, United States of America]	Taken into account. Revisions have been made to ensure a greater degree of consistency between sections, with the main results being summarised in the Synthesis Table.
10-49	10	1	2			The title of the Chapter is misleading to some extent, because „detection" is already discussed throughout the previous chapters; this chapter focuses on the attribution (that is vital e.g. for forming the mitigation actions). It is properly stated e.g. on page 8, lines 33-34. [Tibor Farago, Hungary]	Rejected. This is the only chapter that assesses detection of changes with the meaning of detection as set out in the IPCC report, namely changes outside the range of internal variability.
10-50	10	1	17	1	47	Numbers on line 17 and lines 46 an 47 are difficult to reconcile - 0.6 likely greater than total observed warming with a range of 0.6 to 1.4 - couldn't readily understand this. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. This conclusion has been restated including specifying attributable ranges to make it much clearer how the observed range is made up of the attributable components.
10-51	10	1		5		As a whole, the Executive Summary captures the flavour of the underlying text, reflecting the high points in the main text well. However, the writing is awkward in many places and requires thorough editing and proofing. Some language could be more clearly phrased (i.e. 'better understanding' p.3), there are many typographical errors, and comma's have been used excessively in many sentences resulting in fragmentation that makes sentences difficult to read (i.e. "While internal variability of the climate system, with its ability to move heat around the climate system, is important at hemispheric scales, it is very unlikely that reconstructed temperatures since 1400 can be explained by natural internal variability alone." p.5 line 10-12). [Government of Australia]	Taken into account. The ES has been extensively revised both to bring it into line with a common structure for ES across the report and also to improve clarity and remove typographical errors.
10-52	10	1		90		no comments [Anthony Lupo, United States of America]	Noted.
10-53	10	1		200		16. This paragraph refers to the entire Chapter 10. Chapter 10 reviews some of the published information on the topic "Detection and Attribution of Climate Change: from Global to Regional". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of	Rejected. An assessment is made of the literature and both natural and anthropogenic causes of climate change are considered.

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						such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is exclusively done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	
10-54	10	1				Throughout: O'gorman --> O'Gorman [Richard Allan, United Kingdom]	Accepted.
10-55	10	1				The entire chapter is far too long by order 50%. Some sections are just to long, others seem out of place and/or redundant with material in other chapters(ref in the text). Finally, the slavish idea that each and every paper published on the subject since the last report must be referenced leads to mass confusion of facts. no one except the reviewers are ever going to read this thing. so a major editing job seems required to shorten and make the main results appear in snappy textual summaries. Finally, substantial amounts of text are dedicated to D&A of global mean signals. That metric might have worked in 1995 but we have to be further along than that today. Omit work on global mean temperature and just refer to the earlier report [tim barnett, United States of America]	Rejected. The body of the chapter needs to be sufficiently comprehensive - including a sufficient citation of the scientific literature - to support the main chapter conclusions coming through into the ES. For this it is important to thoroughly assess recent literature on global mean changes since there is now more observational data, a better understanding of the uncertainties in many observational data sources and a new generation of models. There has also been a period when global mean temperatures have not been increasing as rapidly as the longer term warming. Therefore it is important to provide a new assessment of global mean temperature changes.
10-56	10	1				A systemic problem is a lack of addressing natural forcing due to the focus on anthropogenic forcing. I.e. which is the cause and which the consequence. E.g. three major factors to address: 1) The IPCC 0.2 C/decade is about 2 sigma hotter than the 32 year temperature trend, suggesting major missing physics or systemic bias. 2) The 1.56% decrease in clouds over 39 years -(Eastman & Warren 2012);, 3) Watts et al. 2012 draft finding 0.145 deg C/decade higher warming in the poor meteorological sites vs the raw data at the best ones; 4) Scafetta (2011) finding that alternative calibration of the ACRIM gap could show solar causing 15%, 50% or 60% of the observed warming. If all three of these stood together, then this would result in natural causes dominating anthroprogenic causes. Finally, the next glaciation is nominally due in 1500 years. Will there be sufficient warming to overcome this? Especially in light of the poor performance of current models? [David L. Hagen, United States of America]	Rejected. Contrary to the impression given by this comment, observational uncertainties, solar forcing, and comparisons between models and observations are all part of the overall assessment made in this chapter drawing also on conclusions made in chapters 2,3,4 (observations), chapter 8 (forcings) and chapter 9 (evaluation of climate models)
10-57	10	1				A very good chapter with a lot of new and interesting discoverise to report. Most of my comments relate to details. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. Thanks.
10-58	10	1				Possible new reference for Chapter 10 or to pass to WGII: Mahlstein, I., G. Hegerl, and S. Solomon (2012), Emerging local warming signals in observational data, Geophys. Res. Lett., 39, L21711, doi:10.1029/2012GL053952. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. This reference has been included.
10-59	10	1				A striking difference between this chapter and the WGII D&A chapter is that this one has been written by climate scientists and apparently for climate scientists, while the WGII D&A chapter has been written by scientific researchers for, I think, people with some scientific background, perhaps beyond secondary school. This chapter contains a lot of jargon and technical detail that will seem irrelevant to or confuse a non-climate-scientist reader. I'm not sure which approach is best, but I figured that I should point out that difference is noticeable to at least one physical climate scientist. [Dáithí Stone, United States of America]	Noted. A certain technical level of language is needed to provide the chapter assessment. However we have endeavoured to improve the clarity of the chapter, icluding by ensuring consistent use of terminology which will help readability. Note also that there are two FAQs which are written in a more accessible style.
10-60	10	1				The authors have clearly performed a diligent job of surveying the literature. [Dáithí Stone, United States of America]	Noted. Thanks.
10-61	10	1				To me the SOD of this chapter has taken a huge step from the FOD in terms of contextualisation is space and in time. So I can take that rant on the FOD back now! Some of the sorting of information differs from what I would have volunteered (e.g. extremes being considered methodological and having their own section rather than being integrated in the physics sections), but this is probably more a matter of style. [Dáithí Stone, United States of America]	Noted. Thanks.

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10-62	10	3	1	0	0	Section Executive Summary: There is no specific mention in the Executive Summary of the improved capacity (e.g. due to higher resolution, more accurate imposed climate forcings factors, ESMs..) of models since AR4 (e.g. CMIP5) with all external forcings to reproduce observed changes in the physical climate system (as stated in Sect 10.9.2). [European Union]	Accepted. Revision is made to refer explicitly to the assessment made in chapter 9 on improvement of models.
10-63	10	3	1	0	0	Section Executive Summary: We have very high confidence that surface ocean acidification is anthropogenically driven (e.g. Table 10.1) - suggest that this should therefore be included in the Executive Summary (see also Point 23). [European Union]	Accepted. A statement on ocean acidification has been added to the ES.
10-64	10	3	1	3	6	It would be essential at the very beginning of the Exec Summary (perhaps after line 6) and the very beginning of the Intro (perhaps after line 8 more precisely continuing the prev. sentence): to provide the minimum explanations on how the “detection and attribution” are meant for the climate change studies generally, what those mean and what the difference between these two. I know that this terminology was treated formerly (2010) and described later in this Chapter, but as this is an very essential issue, its minimum should be offered even for those readers, „outsiders” who are not aware how it was used in former Assessment Reports. More concretely, e.g. the text pieces from Hegerl et al on page line 22- can be used for a well understandable narrative on these terms and their importance. [Tibor Farago, Hungary]	Rejected. The concepts of detection and attribution are clearly laid out in the chapter body and do not need to be duplicated in the ES. However the ES has undergone substantial revision to make it clearer including the avoidance of structures like detection of anthropogenic influence which should serve to make the ES much more understandable.
10-65	10	3	1	5	54	Please bring the executive summary in line with the summaries of e.g. Chapters 7 and 8. [Government of NORWAY]	Accepted. Done.
10-66	10	3	1	5	54	The Executive Summary sometimes makes it difficult to extract the main message. This arises becauses in part the ES is written essay-style, which gives it a bit of an introductory or tutorial feel (e.g., p. 3, l. 39-41). Moreover, the ES contains some repetitions. I think it would be helpful to apply a revision strategy that aims at making each paragraph (bullet point?) being "liftable" and able to be read independently. For example, there is no summary assessment of attribution on regional scales. [Jochem Marotzke, Germany]	Accepted. The ES has been substantially revised to bring it into line with the IPCC WGI guidelines for the ES along the lines of this comment.
10-67	10	3	1	5	55	In this ES there are likelihood words not italicised. This also occurs in the Chapter itself. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Revision s made to ensure likelihood statements are italcised.
10-68	10	3	1			Section Executive Summary: There is no specific mention in the Executive Summary of the improved capacity (e.g. due to higher resolution, more accurate imposed climate forcings factors, ESMs..) of models since AR4 (e.g. CMIP5) with all external forcings to reproduce observed changes in the physical climate system (as stated in Sect 10.9.2). [Oliver David Andrews, United Kingdom]	Accepted. Revision is made to refer explicitly to the assessment made in chapter 9 on improvement of models.
10-69	10	3	1			Section Executive Summary: Some rather detailed detection and attribution nomenclature is used in the Executive Summary which may be difficult for non-specialists to understand. For example the terms "fingerprint" and "external forcing" are used in key statements and are not formally introduced until Section 10.2.1. More self explanatory terminology (e.g. "response pattern" instead of "fingerprint") or very brief definitions could make this section more readable for generalists. [Oliver David Andrews, United Kingdom]	Taken into account. The ES has been substantially revised including along the lines suggested in this comment regarding fingerprint. However forcing is a standard term that is defined in the IPCC glossary and so we feel can be left in the ES.
10-70	10	3	1			Section Executive Summary: We have very high confidence that surface ocean acidification is anthropogenically driven (e.g. Table 10.1) - suggest that this should therefore be included in the Executive Summary (see also Point 23). [Oliver David Andrews, United Kingdom]	Accepted. A statement on ocean acidification has been added to the ES.
10-71	10	3	1			Executive Summary: There are no section numbers at the ends of paragraphs (or anywhere else), and therefore no "traceable accounts". [J. Graham Cogley, Canada]	Accepted. The ES has been revised to include tracable sections.
10-72	10	3	1			Executive Summary: Calibrated language (very likely, etc.) is italicized rather erratically. This is true of the chapter as a whole. [J. Graham Cogley, Canada]	Accepted. ES revised to italicize consistently.
10-73	10	3	3	3	13	Both thes paragraphs are. untrue. There have been no comparisons between the projecions of climate models and future climate parameters. Indeed, the comparisons of scenarios with future behaviour, which are given in Figures 1.4, 1.5. 1.6, 1.7 and show that predictability is poor for temperature, N2O and Sea Level, and completely wrong for methane. Presumably, this paragraph is referring to consistency with past climate behaviour, which is no guide to the future and does not justify confidence iin any of the model projections. This is particularly true for temperature, for the rather unrelable "Global "Mean Surface Temperature Anomaly" has hardly changed for the last ten years, which shows that for this period, the world is not currently warming,	Rejected. Evidence for the assessment is based on comaprison between models and observations in order to identity to what extent expected patterns of anthropogenic and natural changes have emerged in observed changes.

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						whatever the claimed increases in anthrpogenic factors. This whole Chapter appears to believe that a proper VALDATION of model outcomes. which involves a comprehensive comparison with future climate behaviour, can be replaced by a system of DETECTION and ATTRIBUTION based entirely on the biased opinions of those who have been paid to produce them, and which is subject to a conflict of interest. These personal opinions are not scientific evidence [Vincent Gray, New Zealand]	
10-74	10	3	3	39	52	In this paragraph and elsewhere in this chapter it would be good to distinguish better between: anthropogenic forcing, natural forcing and external forcing. Are external and anthropogenic forcing synonymous? If so use only the latter term. [European Union]	Takein into account. The ES has been revised to be clearer about external forcings by explicitly referring to anthropogenic and natural forcings.
10-75	10	3	4	3	4	The phrase "changes" requires definition. For example, linear trends? If so, over what time period? [Martin Hoerling, United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-76	10	3	4	3	6	"The consistency of observed and modelled changes across the climate system, including regional temperatures, the water cycle, global energy budget, cryosphere and oceans, points to a large-scale warming resulting primarily from anthropogenic increases in greenhouse gas concentrations." How does 'primarily' in this statement fit with (p.3, 33-35) "We conclude that it is extremely likely that human activities have caused most of (at least 50%) the observed increase in global average temperatures since the 1950s and that it is virtually certain that this warming is not due to internal variability alone." [Government of Australia]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-77	10	3	4	3	6	there is also evidence for the C cycle (e.g., increased C storage in plants due to lengthening of the vegetation period) [European Union]	Noted. However the ES has been substantially revised as part of which this statement no longer appears.
10-78	10	3	4	3	6	This statement is false and should be removed. According to HadCRUT4 data the temperature trend has been flat since January 1987 but models predicted warming, ergo your statement is incorrect. [John McLean, Australia]	Rejected.There is discussion in Chapter 9 in a new box of the last 10 to 15 years. In any case the ES has been substantially revised as part of which this statement no longer appears.
10-79	10	3	5	3	5	Global or local water cycle, or both (especially given Line 11 of the same page)? All other items in that list have their spatial extent indicated, so why not the water cycle? [Government of United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-80	10	3	6			Please state the period to which the large-scale warming that is claimed to result primarily from anthropogenic increases in greenhouse gas concentrations relates. Large-scale warming is only uninterrupted since the mid-1970s. Also, primarily is an undefined term, capable of a wide range of inerpretations. I suggest replacing "resulting primarily from" by "over half of which results from". [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-81	10	3	10	3	12	This statement is false. According to HadCRUT4 data the temperature trend has been flat since January 1987 ergo anthropogenic forces could NOT have warmed the climate. Further, the absence of warming over the last 15 years despite the CO2 increase proves that CO2 either has negligible effect or is very easily overwhelmed by other climate forces. [John McLean, Australia]	Rejected.There is discussion in Chapter 9 in a new box of the last 10 to 15 years. In any case the ES has been substantially revised as part of which this statement no longer appears.
10-82	10	3	10	5		This is the most integrated and accessible Executive Summary I have seen in the entire report. Please maintain the approach. [Jochen Harnisch, Germany]	Rejected. The ES has been substantially revised to accord to a consistent format across the report.
10-83	10	3	10		11	"anthropogenic forcings have warmed the climate". This is ambiguous at best. "have increased global mean surface temperature?"; Have increased the downwelling radiant energy flux"? It's an important sentence. Might as well make it say what is intended. [Stephen E Schwartz, United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-84	10	3	11	3	11	also here mention C cycle [European Union]	Rejected. The ES has been substantially revised as part of which this statement no longer appears.However there is discussion of the climate cycle in the climate system properties section of the ES.

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10-85	10	3	11			"climate change has affected climate regionally"; sounds almost tautological. Better something like "climate change has occurred on regional scales as well as global. " But still when you think about it that doesnt make much sense either. If climate change were uniform globally, it would still be affecting regions, but the same in all regions. I think the intent is that climate change is not uniform globally, but is manifested by regional variation. [Stephen E Schwartz, United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-86	10	3	12	3	13	The authors should consider extending the sentence to reflect that low confidence remains in attributing some aspects of the changes, as per the sentence of lines 28-30. [Government of United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-87	10	3	12	3	13	Be explicit as to whether the phrase "climate change" refers to anthropogenic climate change. [Martin Hoerling, United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-88	10	3	12	3	13	If climate change has affected climate globally there must be regions that are affected (maybe climate change has not yet been detected for these regions but they must exist). [Albert Klein Tank, Netherlands]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-89	10	3	12	3	13	Doesn't global climate change by definition affect climate regionally? [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-90	10	3	12			*„The evidence is stronger that climate change has affected climate regionally as well as globally" – tautology, it would be better e.g.: The evidence is stronger that climate change has occurred (or has been occurring) regionally as well as globally. [Tibor Farago, Hungary]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-91	10	3	12			Can climate change affect climate? [Dáithí Stone, United States of America]	Taken into account. The ES has been substantially revised as part of which this statement no longer appears.
10-92	10	3	15	3	16	Important to emphasise in the Executive Summary (as in Sect 10.3.1.1.3, Page 15, Line 44) that alongside improved CMIP5 models the data record of observed temperature change has been extended to 2010 (with AR4 stopping at 1999) and includes spatial information, which has allowed for further constraints to be placed on the magnitude of GHG attributable warming. [Oliver David Andrews, United Kingdom]	Taken into account. The additional observational information is referred to in the revised ES.
10-93	10	3	15	3	16	Important to emphasise in the Executive Summary (as in Sect 10.3.1.1.3, Page 15, Line 44) that alongside improved CMIP5 models the data record of observed temperature change has been extended to 2010 (with AR4 stopping at 1999) and includes spatial information, which has allowed for further constraints to be placed on the magnitude of GHG attributable warming. [European Union]	Taken into account. The additional observational information is referred to in the revised ES.
10-94	10	3	15	3	16	Sentence part "and fingerprints…climate models" is dangling. [Jochem Marotzke, Germany]	Taken into account. The ES has been substantially revised.
10-95	10	3	15	3	16	This statement is false and should be removed. According to HadCRUT4 data the temperature trend has been flat since January 1987. With no warming across of 15 year period when atmospheric CO2 increased any claims to a fingerprint of manmade warming driven by CO2 emissions are not based on reality. [John McLean, Australia]	Rejected.There is discussion in Chapter 9 in a new box of the last 10 to 15 years. In any case the ES has been substantially revised as part of which this statement no longer appears.
10-96	10	3	15	3	52	The paras "Progress since AR4" and "Evidence for Warming" contain several duplications. Copy/paste should be avoided, please consider restructuring in order to improve text. [Government of Germany]	Taken into account. The ES has been substantially revised including to avoid such repetitions.
10-97	10	3	16	3	16	Human influence' in the observational uncertainty itself? We don't think that is what is meant, but it could be read that way. Please consider revising. [Government of United States of America]	Taken into account. The ES has been substantially revised including revising this sentence to address this concern.
10-98	10	3	16	3	18	"An assessment of the very likely range of the greenhouse gas contribution to observed warming of about 0.6K since 1951 is now possible (0.6–1.4 K)." An explanation of why the best guess figure is the lower end of the range would be useful. [Government of Australia]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components.

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10-99	10	3	16	3	18	The formulation “An assessment of the very likely range of the greenhouse gas contribution to observed warming of about 0.6K since 1951 is now possible (0.6–1.4 K).” with Line 37-39 of Page 15 is not consistent with that in the main report and difficult to understand. The main report says, “Over the 1951–2010 period, greenhouse-gas-attributable warming at 0.6–1.4 K is significantly larger than the observed warming of approximately 0.6 K, and is compensated by an aerosol-induced cooling of between 0 and –0.8 K (Figure 10.4b) (Jones et al., 2012). ” This also happens to the formulations in Line 45-47 of Page 3: “The greenhouse gas contribution to the observed warming of approximately 0.6 K over 1951–2010 was very likely greater than the total observed warming with a range between 0.6 and 1.4 K.” It is recommended to use sentences at Line 28-30 of Page 10, SPM. “The greenhouse gas contribution to the warming from 1951–2010 is in the range between 0.6 and 1.4℃. This is very likely greater than the total observed warming of approximately 0.6℃ over the same period. ” [Government of China]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components.
10-100	10	3	16	3	18	We fear this statement might not make sense to a general reader. As written, it implies that the GHG contribution to warming could exceed what was actually observed (i.e. how can one contribute up to 1.4K, if only 0.6K was observed). We assume it is a cooling contribution from other climate drivers that counteracts the GHG contribution, so bringing the nett warming to around 0.6K. Perhaps this should be mentioned. [Government of United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components.
10-101	10	3	16	3	18	I don't see that a 'very likely' range can be put on estimates of the GHG contribution to observed warming derived from AOGCM simulations, since if there is a material omitted forcing, feedback or modelling problem in one AOGCM it may well affect all AOGCMs. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. The assessment describes how the likelihood ranges on attributable temperature trernds are obtained, which includes taking account of observations in the optimal detection methdology.
10-102	10	3	16	3	18	"An assessment of the very likely range" seems both awkward and colloquial. What matters is the contribution of GHG to observed warming (with uncertainties). Furthermore, the same assessment is repeated further down in the ES (p. 3 l. 44-47) and, more importantly still, given much more clearly in the main text (p. 15, l. 36-39). Here on l. 16-18 of ES, it is not clear whether it's the GHG contribution or the observed warming that is 0.6 K since 1951. [Jochem Marotzke, Germany]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-103	10	3	16	3	18	This statement is false and should be removed. According to HadCRUT4 data the temperature trend has been flat since January 1987, proving that CO2driven warming is negligible or easily overwhelmed by other forces. Given that models that assumed CO2 is a major force predicted warming, the assumption that CO2-driven warming can account for 0.6C since 1950 is not sustainable. [John McLean, Australia]	Rejected. The assessment describes how the likelihood ranges on attributable temperature trernds are obtained, which includes taking account of observations in the optimal detection methdology.
10-104	10	3	16	3	18	There are many ways to interpret this sentence. My first reading was that the median is 0.6 and the uncertainty is 0.6-1.4, except that seems ridiculously skewed. My second reading had the uncertainty of 0.6-1.4 having a width of 0.6, except that it is 0.8. My third reading has the 0.6 applying to the observed warming, while the 0.6-1.4 applies to the greenhouse gas contribution, but that requires an odd parsing. [Dáithí Stone, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-105	10	3	16	3	18	I would suggest to be a little more cautious here: the estimate of GHG contribution uncertainty range given here could possibly be underestimated. If I understand correctly (after reading the paper), the numbers here are likely directly taken from the Jones et al. 2012 JGR paper where the authors select one truncation value (k=24) and base their GHG contribution estimate on results from two kinds (simple and weighted) of multimodel averages. As the selection of k is to a certain extent arbitrary, it would seem reasonable to account for the choice of different k values within the GHG contribution uncertainty range. Given Fig. 17 of Jones et al., this would clearly increase the uncertainty range. In any case, the rationale (including the full set of hypothesis behind it) underlying the estimation of the GHG contribution uncertainty range has to be much more explicit in the appropriate section (see below). Finally, just reading the summary from Jones et al. and other relevant works (Gillett et al 2012., Ribes and Terray 2012), I would suggest to mitigate a bit the expression "the very likely range of GHG contribution" to "the likely range of GHG contribution". [Laurent Terray, France]	Taken into accout. This aspect of the assessment has been reassessed bringing in the new results from the Ribes et al papers which are now included in Fig 10.4 and taken account of in the assessment, as illustrated in the new Figure 10.5. As a result there are new attributable ranges for the GHG contribution.
10-106	10	3	16		17	The GHG contribution to the observed warming being probably more than 100% because of anthropogenic cooling & other factors is often not clear to non-D&A people even in the field: clarify here [William Ingram, United Kingdom]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced

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							components
10-107	10	3	17	3	18	Clarify: "contribution (0.6--1.4 K) to observed warming of about 0.6 K". [J. Graham Cogley, Canada]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-108	10	3	17	3	18	Please make it clear how the observed warming of 0.6K stated here is related to the temperature increases documented in the SPM page 3, line 21-25. E.g. how large is the difference if we talk about GHG contribution to warming, compared to TOTAL warming. Is it possible to use the same reference year(s) as in the SPM? [Government of NORWAY]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-109	10	3	17	3	18	Please revise to clarify whether the 0.6K warming is the GHG contribution, and also what the meaning of the 0.6-1.4K range is. [Martin Hoerling, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-110	10	3	17	3	18	Without background information from the section it is confusing how the contribution from GHG can be greater than the signal itself. [Albert Klein Tank, Netherlands]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-111	10	3	17	3	18	Sentence is misleading: Change to: An assessment of the very likely range of the greenhouse gas contribution of about 0.6K to the total observed warming of 0.6–1.4K since 1951 is now possible [Roman Zweifel, Switzerland]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-112	10	3	17			*the bracketed range is unclear: “about 0.6K since 1951 is now possible (0.6–1.4K)” [Tibor Farago, Hungary]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-113	10	3	17			"about 0.6 K since 1951 ... (0.6 – 1.4 K)". Dont understand. Is the 0.6 – 1.4 K supposed to be a range? or is it a typo 0.6 ± 0.14)? Whatever it is it should be fixed. [Stephen E Schwartz, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-114	10	3	18	3	19	The statement that warming since the mid-20th century is outside the range of internal climate variability is contradicted by recent studies, e.g. see Figure 2 in J. Esper et al., 2012, Orbital forcing of tree-ring data, Nature Climate Change, 8 July 2012, doi:10.1038/NCLIMATE1589. See also Lu et al., 2012, An ikaite record of late Holocene climate at the Antarctic Peninsula, Earth and Planetary Science Letters, Vols. 325–326, 1 April 2012, Pages 108–115. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Addressed. Statement revised to clarify that it discusses variability on hemispheric means in records with forced signals removed.
10-115	10	3	18	3	19	This statement is false and should be removed. McLean et al (2009) show the ENSO to be the likely dominant of global average temperature and that the SOI and temperature show a clear strong correlation, albeit a correlation shrouded in the "noise" caused by short-term forces. The temperature variation since the mid-20th century is therefore consistent with natural forces. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to extend the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) [John McLean, Australia]	Rejected. The cited study examines tropospheric tempearture not surface tempearture. Also the conclusionsgiven in the comment regarding the influence of ENSO on the trend appear to not be supported based on Foster et al. (2010).The study shows an influence of ENSO on interannual tropsopheric temperature variations which is also seen in earlier studies so this study is not cited in the chapter.
10-116	10	3	18	3	19	This sentence is too vague. It is not clear how far back in time the "better understood pre-instrumental data" reach. (Last 1000 years? last million years??). Moreover, the expression "far outside the range" is unclear. How far is "far"? I suggest specification of the time period considered and also a specification of how "far" outside the range of internal variability the mid-20th century warming is; by using a formulation in accordance with the otherwise well defined IPCC-used terms that express uncertainities. [Anders Moberg, Sweden]	Taken into account. In the substantial revision of the ES this statement no longer appears.

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10-117	10	3	18	3	19	This sounds like such a change as never happened before. Is that true of the glacial-interglacial transitions? Of the climate change at the time of the K-T asteroid impact? [Dáithí Stone, United States of America]	Taken into account. In the substantial revision of the ES this statement no longer appears.
10-118	10	3	18		19	“data” & then “records” illucid: use same word [William Ingram, United Kingdom]	Taken into account. In the substantial revision of the ES this statement no longer appears.
10-119	10	3	19	3	22	The anthropogenic fingerprint of ocean warming is something we are far more certain of than this statement suggests. Based on the recent formal detection and attribution work of Gleckler et al. (2012) and Pierce et al. (2012) can't something stronger be said here about the contribution of anthropogenic forcing to observed changes in ocean temperature? For example Pierce et al. (2012) find natural external forcings to yield a null detection result, discounting an 'all external forcings' explanation for observed temperature changes. The 'extremely certain' claim made regarding the anthropogenic contribution to observed 0-700m ocean temperature changes (Section 10.4.1 Page 32, Lines 8-12) could be referred to. [Oliver David Andrews, United Kingdom]	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing to ocen warming.
10-120	10	3	19	3	22	“There is improved understanding of ocean changes including better understanding of ocean temperature variability, which supports it being very likely that more the half of the observed ocean warming since the 1970s is caused by external forcing.” Does external forcing mean anthropogenic forcing or only natural external forcers? The term external forcing is used in a number of places (p.3, line 50, line 56) and needs to be clearly defined. [Government of Australia]	taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing not external forcing. The revised ES is clearer about the use of external forcing term.
10-121	10	3	19	3	22	The anthropogenic fingerprint of ocean warming is something we are far more certain of than this statement suggests. Based on the recent formal detection and attribution work of Gleckler et al. (2012) and Pierce et al. (2012) can't something stronger be said here about the contribution of anthropogenic forcing to observed changes in ocean temperature? For example Pierce et al. (2012) find natural external forcings to yield a null detection result, discounting an 'all external forcings' explanation for observed temperature changes. The 'extremely certain' claim made regarding the anthropogenic contribution to observed 0-700m ocean temperature changes (Section 10.4.1 Page 32, Lines 8-12) could be referred to. [European Union]	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing to ocean warming.
10-122	10	3	20	3	20	ocean temperature variability-- I think "changes in ocean heat content" is more informative [John Mitchell, United Kingdom]	Taken into account. A change along these lines has been implemented in the much revised ES.
10-123	10	3	21	3	21	"more than half". [J. Graham Cogley, Canada]	Not clear what the comment is trying to imply here.
10-124	10	3	21	3	21	the -> than [Laurent Terray, France]	Taken into account in revised ES.
10-125	10	3	21			Is this an italicised very likely? [Dáithí Stone, United States of America]	Taken into account. Italicisation is unified.
10-126	10	3	22	3	22	What is meant by 'external' here: anthropogenic and/or natural? [Government of United States of America]	Taken into account. ES revised to assessment of anthropogenic influence only for ocean warming
10-127	10	3	22			Could simplify “change. The salinity” to “, which” [William Ingram, United Kingdom]	Noted. ES has been substantially revised.
10-128	10	3	23	3	23	We suggest deleting "large scale" and inserting "global" before "hydrological". [Government of United States of America]	Accepted. Global is the term used when referring to parcipitation changes in the revised ES
10-129	10	3	23			*we usually avoid “predicted” but use e.g. “projected” [Tibor Farago, Hungary]	Taken into accout. The ES has been substantially revised and this sentence no longer appears.
10-130	10	3	23			“the” after “with”? [William Ingram, United Kingdom]	Taken into account. The ES has been substantially revised and this sentence no longer appears.
10-131	10	3	24	3	24	Are the 'temperature extremes' global and/or regional? [Government of United States of America]	Taken into account. The ES has been substantially revised and it is made clear in the revised ES that this statement is referring to global changes.
10-132	10	3	24	3	26	"Since the mid 2th Century" is hardly a large period in human or climate history, and you ignors the fact that the"warming: has ceased in the last 10 years.and there is no evidence that any warming is caused by human activity [Vincent Gray, New Zealand]	Rejected. The assessment takes account both of the pre-instremental period and of the last 10 years with a new box in Chapter 9

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10-133	10	3	25	3	25	very likely in italics [European Union]	Taken into account. ES has been revised with consistent italicisation when referring to likelihood statements.
10-134	10	3	25	3	25	Are the 'temperature extremes' global and/or regional? [Government of United States of America]	Taken into account. The ES has been substantially revised and it is made clear in the revised ES that this statement is referring to global changes.
10-135	10	3	25	3	26	State specifically the nature of temperature extremes alluded to (e.g., are they of daily values, record hi/lows, heat wave events, etc) [Martin Hoerling, United States of America]	Taken into account. The ES has been substantially revised and it is made clear in the revised ES what this statement is referring to.
10-136	10	3	28	3	29	Confidence in attribution of changes in Antarctic mass balance: this is not accurate as far as observed mass balance is concerned. Confidence may well be low for projections. Although the range of recent observational estimates is rather wide, confidence in the negative sign of recent Antarctic mass balance can be assessed as high. Make this consistent with WG1 Ch04 P4 L25-32. Note should also be taken of Shepherd, A., and 46 others, 2012, A reconciled estimate of ice-sheet mass balance, Science, 338, 1183-1189. [J. Graham Cogley, Canada]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-137	10	3	28	3	30	The sentence misses a key point and should be acordingly revised. For some of the metrics listed (e.g. droughts, tropical cyclones) detection is not possible, let alone attribution. This is because of the very low ratio of estimated GHG signal to the noise of natural variability. Please clarify and revise. [Martin Hoerling, United States of America]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-138	10	3	28	3	30	Many recent studies have been published evidencing a larger urban warming in regional scale surface temperature changes over the past half a century, and these need to be more thoroughly assessed in Section 2.2.1.2. I will send a comment on this to the Chapter2. These recent findings are relevant to the sub-continental scale detection and attribution assessment. [Guoyu Ren, China]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-139	10	3	29	3	29	Uncertainty will always 'remain'. The question is whether it's becomes acceptably small. Consider writing '…due to large observational…' instead. [Government of United States of America]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-140	10	3	29			"ice" or "ice-sheet" could be inserted before "mass" [Adrian Simmons, United Kingdom]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-141	10	3	30	3	33	Rephrase sentence since could be interpreted as each individual variable is not detectable which is clearly not the case for some. [Government of Australia]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-142	10	3	30	3	33	This sentence is fragmented and difficult to read. Suggest re-ordering sentence parts. [Government of Australia]	Taken into account. The ES has been revised substantially and this statement no longer appears.
10-143	10	3	30	3	33	Append to this sentence "and consistent with naturally-driven warming" because these are the types of changes we could reasonably expect with ANY cause of warming. [John McLean, Australia]	Rejected. The ES has been revised substantially and this statement no longer appears.
10-144	10	3	30			The term "free atmopshere temperatures" should be explained in the glossary. [Klaus Radunsky, Austria]	Taken into account. "Free atmosphere" does not appear in revised ES.
10-145	10	3	31	3	31	Unclear what "taken together" means in this context. Do some variables show this and some do not, or is this about multiple attribution? [Albert Klein Tank, Netherlands]	Taken into account. What is mean is made more explicit in the revised ES.
10-146	10	3	31	3	32	Awkward: ", when taken together, show, not just" [Jochem Marotzke, Germany]	Taken into account. Statement has been revised to aid clarity.
10-147	10	3	33	3	33	"extremely likely" - it would be helpful somewhere in the chapter to give an indication of the rationale behind how the likelihood level was quantified/assessed [John Mitchell, United Kingdom]	Taken into account. This statement does not appear in the revised ES.
10-148	10	3	33	3	33	"extremely likely" - this is not a recognised IPCC expression for treating uncertainty. Change to either "very likely" or virtually certain". This phrase appears several times throughout the chapter. [James Renwick, New Zealand]	Taken into account. This statement does not appear in the revised ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-149	10	3	33	3	33	Although the old AR4 term "extremely likely" is included as an acceptable term in the new uncertainty guidance document it would be preferable if one of the 7 primary likelihood terms could be used, i.e., in this case, either 'very likely' or 'virtually certain'. This is also relevant on lines 44, and 56/57. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This statement does not appear in the revised ES. However the ES does continue to use the term "extremely likely" for other statements. For detection and attribution assessments, the "extremely likely" formulation is very useful since it marks an intermediate point between "very likely" and "virtually certain".
10-150	10	3	33	3	35	This is a very important statement but it seems quite unbalanced - 'extremely likely' implies very high confidence although can only a minimum of 50% of the warming be confidently attributed to anthropogenic forcing? This statement needs to be more fully justified and explained in Section 10.3.1.1.3. [Oliver David Andrews, United Kingdom]	Taken into account. This statement does not appear in the revised ES.
10-151	10	3	33	3	35	The original texts say, “We conclude that it is extremely likely that human activities have caused most of (at least 50%) the observed increase in global average temperatures since the 1950s and that it is virtually certain that this warming is not due to internal variability alone.”, where “extremely likely” refers to 95% possibility, while AR4 states that most of the observed increase in global average temperatures since the mid-20th century is very likely (namely, over 90% of possibility) due to the observed increase in anthropogenic greenhouse gas concentrations. Actually, in the above two statements extracted from AR5 and AR4 respectively, “extremely likely” and “very likely” are not describing exactly the same subject. In our view, in order to avoid misleading decision- or policy-makers, the report should explain the implications of this important conclusion and its difference with AR4 in terms of confidence levels in greater details. Otherwise, policy-makers may mistakenly believe that the AR5 conclusion on climate change attribution is simply an increase of confidence level to 95% (extremely likely) from 90% (very likely) in AR4. In addition, the present expression may mislead policymakers into thinking that it is the human activities conducted after 1950 that resulted in the most (more than 50%) observed average global surface temperature increase since the 1950s. it is recommended to add “since industrial revolution (1750)”after “human activities” in this sentence. Also see Line 8-9 of Page 10 SPM, Line 14-16 of Page 23 TS, Line 43-45 of Page 3 in Chapter 10, Line 44-47 of Page 62 of Chapter 10 and Table 10.1 of Chapter 10, where the same problem is identified. [Government of China]	Taken into account. This statement does not appear in the revised ES.
10-152	10	3	33	3	35	Another conclusion is that this increase in global temperatures has had regional affects, as described in all prior paragraphs of this Executive Summary? [Government of United States of America]	Taken into account. This statement does not appear in the revised ES.
10-153	10	3	33	3	35	The sentence, beginning "We conclude..." lacks transparency and clarity as to what the change in assessment since AR4 actually is. Revision is needed. In particular, the Executive Summary of Ch9 in the 2007 IPCC report states "Greenhouse gas forcing has very likely caused most of the observed global warming over the last 50 years. " In this AR5 draft, it is written that "it is extremely likely that human activities have caused most (at least 50%) of the observed increase in global average temepratures since the 1950s". In comparing these, are the phrases "greenhouse gas forcing" and "human activities" meant to be interchangable? Is there a material differtence in assessing a change "over the last 50 yrs" in AR4 versus a change "since the 1950s" in this AR5 draft? One might reasonably wonder whether a re-analysis of the exact same period assessed in AR4, but having available additional observations and new model simulations, would alone justify the change in confidence. Does it? A reader might further wonder why the stated confidence has risen, given this draft's subsequent statement (line 50-51) that global mean temperatures have not changed significantly since 1998? The authors must reconcile their increased confidence in the cause for the observed increase in global average temperatures with the fact that the observations have failed to reveal further warming in global mean temperatures since TAR? Is the traceability clear on this matter? The authors must also clarify how the second portion of this sentence, which states "it is virtually certain that this warming is not due to internal variability alone", has been revised since AR4. In that report, it is written in the Ch9 Executive Summary that "It is extremely unlikely that the global pattern of warming during the past half century can be explained without external forcing". Is the reader, for instance, to understand that "internal variability" is synonymous in meaning to "without external forcing"? Is there a distinction being made by the authors between "the pattern of global warming" as expressed in AR4, and "the increase in global average temepratures" as expressed in this	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES.

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						draft sentence? As now proposed in this draft paragraph concerning the new professed raised confidence on the attribution regarding global averaged temperatures, the authors will want to make sure that their appraisal is solidly founded on supporting evidence, and that the change in confidence is not a matter of subjectivit or open to strong criticism to that effect, as the latter would undermine credibility of both the AR4 and AR5 assessments. Finally, the last statement on line 35 , which first appears to be a strong attribution (i.e., virtually certain) loses its punch with the ending word "alone". I encourage the authors to consider whether the scientific evidence for the intensity of internal variability on 50-yr trends in global average temperature might allow the following statment: "It is virtually certain that the observed increase in global average temperature since the 1950s is not due to internal variability" [Martin Hoerling, United States of America]	
10-154	10	3	33	3	35	This statement cannot be sustained in light of the absence of warming over the last 15 years, a period when atmospheric CO2 increased. (And don't dismiss 15 years as being of no significance when the period of general warming was across just 20 years, from 1977 to 1996.) Remove the statement. [John McLean, Australia]	Rejected. Assessement includes an assessment of the global warming over the last 15 years including a box in chapter 9.
10-155	10	3	33	3	46	At least 50% as an explanation of "most of" seems odd. Please explain how you get to the 50%, given that you also state that already the GHG contribution is greater than the observed warming. [Government of Germany]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES.
10-156	10	3	34	3	34	"most (at least 50%) of". Make the same correction at P3 L45, P18 L13, P62 L46. [J. Graham Cogley, Canada]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES.
10-157	10	3	34	3	34	"at least 50%" should "more than 50%" if "most" is retained here and elsewhere. [Government of United States of America]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES. This includes a more than formulation as suggested.
10-158	10	3	34	3	34	changing "most of (at least 50%)" to "more than half of" [Zong-Ci Zhao, China]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES. This includes a more than formulation as suggested.
10-159	10	3	34			*“most of (at least 50%)” – these two terms cannot be matched, i.e. „most of means” >50%, i.e.: caused at least 50% or more [Tibor Farago, Hungary]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES. This includes a more than formulation.
10-160	10	3	34			*for clarity: „increase in global average near surface temperatures” [Tibor Farago, Hungary]	Taken into account. The ES has been substantially revised under headings with the final synthesis statement based on the combination of evidence at the end in order to improve clarity and traceability of the ES.
10-161	10	3	34			Here, and in a couple of other places, the words "most of (at least 50%)" are used. To me "most of" implies much more then 50%. "the majority of (at least 50%)" would perhaps be better. Or "more then half". [Adrian Simmons, United Kingdom]	Taken into account. The ES has been revised to use the more than formulion and to avoid the use of most.

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10-162	10	3	35	3	35	"not due to internal variability alone": consider strengthening, or rather increasing the specificity of, this by saying something like "due to emissions of greenhouse gas and black carbon offset by emissions of tropospheric aerosols". [J. Graham Cogley, Canada]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-163	10	3	37	3	37	Since AR4 a range of different techniques have been applied to detection and attribution of global surface temperature changes which go beyond "traditional" regression-based optimal fingerprinting. Examples from the literature are summarised nicely in Sect. 10.3.1.1.3 Page 16 (from line 46) but it would be good to highlight in the Executive Summary that these novel approaches (e.g. Drost and Karoly, 2012) support and strengthen the existing D&A evidence for anthropogenic warming. [Oliver David Andrews, United Kingdom]	Accepted. The ES has been revised to include a reference to multiple studies using different methods.
10-164	10	3	37	3	37	Since AR4 a range of different techniques have been applied to detection and attribution of global surface temperature changes which go beyond "traditional" regression-based optimal fingerprinting. Examples from the literature are summarised nicely in Sect. 10.3.1.1.3 Page 16 (from line 46) but it would be good to highlight in the Executive Summary that these novel approaches (e.g. Drost and Karoly, 2012) support and strengthen the existing D&A evidence for anthropogenic warming. [European Union]	Accepted. The ES has been revised to include a reference to multiple studies using different methods.
10-165	10	3	37			Section "Evidence for Warming": This title does not follow the other ttiles in the Executive Summary. I suggest "Evidence from Temperature Changes" Also, the discussion of cooling stratospheric temperatures is not consistent with the title. [Chris Forest, United States of America]	Taken into account. The ES has been substantially revised with new headings according to variables.
10-166	10	3	39	3	39	Over what time period is this fingerprint (given that you subsequently refer to decadal variability)? [Government of United States of America]	Taken into account. This sentence has been deleted from the ES.
10-167	10	3	39	3	43	Sentence is too long and difficult to read. Consider re-writing as two sentences, one listing anthropogenic fingerprints and one explaining expected patterns. [Government of Australia]	Taken into account. This sentence has been deleted from the ES.
10-168	10	3	39	3	43	Sentence has a tutorial feel, rather than ES-style. [Jochem Marotzke, Germany]	Taken into account. This sentence has been deleted from the ES.
10-169	10	3	39	3	52	Anecdotal evidence is not reliable. Measurements of temperature show that there is no current warning, Ther is also no evidence that any warming is "anthropogenic" even if it has a subjectively assessed "fingerprint" [Vincent Gray, New Zealand]	Rejected. Assessment is not based on anecdotal evidence. Note however that the initial tutorial sentences have been deleted.
10-170	10	3	40	3	41	The spreading of ocean warming from surface to depth is used as one of the three major fingerprints of greenhouse gas warming. Yet chapter 3 (Fig 3.3 and associated text) shows no warming for the 2000-3000 m depth rage. It is important that the discrepancy is resolved between the two chapters so this key fingerprint is fully supported by the report. It looks like Chapter 3 is reporting a time period that is simply too short. [European Union]	Taken into account. This sentence has been deleted from the ES.
10-171	10	3	40	3	41	The spreading of ocean warming from surface to depth is used as one of the three major fingerprints of greenhouse gas warming. Yet chapter 3 (Fig 3.3 and associated text) shows no warming for the 2000-3000 m depth rage. It is important that the discrepancy is resolved between the two chapters so this key fingerprint is fully supported by the report. It looks like Chapter 3 is reporting a time period that is simply too short. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This sentence has been deleted from the ES.
10-172	10	3	41	3	41	Why "expected"? [Albert Klein Tank, Netherlands]	Taken into account. This sentence has been deleted from the ES.
10-173	10	3	42			*for more clarity: and the expected response of these indicators to changes [Tibor Farago, Hungary]	Taken into account. This sentence has been deleted from the ES.
10-174	10	3	43	3	45	The sentence starting "Quanitification …" is redundant with the statement in lines 33-35 above it. [Chris Forest, United States of America]	Taken into account. The ES has been substantially revised including to avoid duplication.
10-175	10	3	43	3	45	I don't think that attribution analyses can show that it is "extremely likely" that human activities have caused >50% of the increase in global mean temperature since the 1950s. Such analyses involve a substantial element of circularity, due to the fact that, in addition to an element of explicit tuning, only AOGCMs that show warming consistent with the historical record will be retained. See Mauritzen et al., 2012, Tuning the climate of	Rejected. Justification for this assessment is provided in the chapter including an explanation of why this is not a circular argument.

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						a global model, Jnl Advances in Modeling Earth Systems, doi:10.1029/2012MS000154. And Stone and Allen, 2005, Attribution of global surface warming without dynamical models, GRl, doi:10.1029/2005GL023682, 2005, wrote, as justification for the near-complete circularity of the attribution method in that paper: "The circularity of this methodology is a general problem with attribution studies because the scientific community is a long way from producing realistic climate models based solely on first principles with no tuning of the output." [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-176	10	3	43	3	45	This statement is unsustainable given that there's been no warming for the last 15 years despite CO2 concentration being higher than during 1977-1996 when general warming did occur. Remove the statement. [John McLean, Australia]	Rejected.There is discussion in Chapter 9 in a new box of the last 10 to 15 years and the assessment takes account of the global temperature record over this time.
10-177	10	3	43	3	47	This repetition of material from the two preceding paragraphs is unprofitable. [J. Graham Cogley, Canada]	Taken into account. The ES has been substantially revised including to remove repetition.
10-178	10	3	43	3	50	Some overlap with lines 16-22 ans 33-35. So, the text may be somewhat abbreviated. [Christian-D. Schoenwiese, Germany]	Taken into account. The ES has been substantially revised including to remove repetition.
10-179	10	3	43			Will ES readers know what a forcing is? [Dáithí Stone, United States of America]	Rejected. Radiative forcing is a term in the glossary.
10-180	10	3	44	3	45	The sentences read “it is extremely likely that human activities have caused most of (at least 50%) the observed increase in global average temperatures since the 1950s.” This statement contradicts the conclusion of recent important paper of Large and Yeager (2012): Large W. G., S. G. Yeager, 2012: On the Observed Trends and Changes in Global Sea Surface Temperature and Air–Sea Heat Fluxes (1984–2006). J. Climate, 25, 6123–6135. doi: http://dx.doi.org/10.1175/JCLI-D-11-00148.1. They have shown that the increase in SST after 1970 is likely natural from the measurement of heat fluxes. [Kiminori Itoh, Japan]	Rejected. Chapter 3 assesses the climate response from surface fluxes and concludes " 'In an alternative approach, Large and Yeager (2009) modified NCEP1 reanalysis state variables prior to flux calculation using various adjustment techniques, to produce the Coordinated Ocean-ice Reference Experiments (CORE) turbulent fluxes for 1948–2007 (Griffies et al., 2009). However, as the adjustments employed to produce the CORE fluxes were based on limited periods (e.g., 2000–2004 for wind speed) it is not clear to what extent CORE can be reliably used for studies of interdecadal variability over the 60 year period that it spans. In a subsequent analysis, Large and Yeager (2012) examined surface flux changes using CORE over the shorter 23 year period, 1984-2006, and concluded that natural variability, rather than long-term climate change, dominates heat flux changes over this period.' Surface heat fluxes remain relatively poorly know compared with surface temperature assessed in Chapter 2, and thus the work from Large and Yeager 2012 is not contradictory to the conclusion in this Executive summary about human influence on gloabl average temperatures.
10-181	10	3	45	3	45	"surface" should be inserted before "temperatures". [Government of United States of America]	Accepted. Surface has been inserted.
10-182	10	3	45	3	45	changing "most of (at least 50%)" to "more than half of" [Zong-Ci Zhao, China]	Taken into account. A "more than" formulation has been adopted.
10-183	10	3	45	3	47	*again, the wording is not very clear: “The greenhouse gas contribution to the observed warming of approximately 0.6 K over 1951–2010 was very likely greater than the total observed warming with a range between 0.6 and 1.4 K.” Obviously, it is on the purely ghg-induced (hypothetical) larger warming vs. the factual one where there are other factors, as well (e.g. aerosols). [Tibor Farago, Hungary]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-184	10	3	45	3	47	"The greenhouse gas contribution to the observed warming of approximately 0.6 K over 1951-2010 was very likely greater than the total observed warming with a range between 0.6 and 1.4 K." This sentence is a little bit ambigous, e.g., is the 'range between 0.6 and 1.4 K' refering to the total observed warming or to the	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced

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						greenhouse gas contribution to the observed warming? [Government of United States of America]	components
10-185	10	3	45	3	47	Repeats lines 16-18. [Jonathan Gregory, United Kingdom]	Take into account. Repetitions deleted in the much revised ES.
10-186	10	3	45	3	47	The way this sentence is formulated makes it unclear to what change the 0.6 and 0.6-1.4K refer to. The text on page 15, lines 36-39 reads better. [Albert Klein Tank, Netherlands]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-187	10	3	45	3	47	Repeats l. 16-18 above. It is not clear whether it's the GHG contribution or the observed warming that is 0.6 K since 1951. Main text is much clearer on this. [Jochem Marotzke, Germany]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-188	10	3	45	3	47	Given that no warming has occurred for the last 15 years despite the elevated concentration of atmospheric CO2 one would have to logically conclude that the claimed 0.6K is very highly unlikely and that climate forces are poorly understood. Remove the statement. [John McLean, Australia]	Rejected. Assessment takes account of temperature changes over last 15 years.
10-189	10	3	45	3	47	McLean et al (2009) shows the primary driver to be the ENSO (see its Fig 7(a),(b) and(c) which are of monthly data rather than derivatives) and that no greenhouse gas forcing is required to explain the temperature variations since 1950. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?). Your statement needs correction. [John McLean, Australia]	Rejected. Assessment of combined evidence supports ES statements.
10-190	10	3	45	3	47	It seems that the evaluated range of 0.6 – 1.4 K is mainly supported by one publication (Jones et al, 2012; an other estimate is included in this range). This range is thus very dependant on the method that has been used to produce it. The finding appears to be of “limited evidence” if we apply to it the uncertainty language of the IPCC guidance note and the statement should be associated to a “low confidence”. The qualification “very likely” applied to the following statement should thus also be re-evaluated. [Serge PLANTON, France]	Take into account. The new assessment in the revised chapter draws on 3 optimal detection papers (Ribes et al, Jones e al, Gillett et al) supported by other modelling studies including Wigley and Santer and Huber and Knutti.
10-191	10	3	45	3	47	This sentence should be agreed with page 15 line 36-39. [Zong-Ci Zhao, China]	Taken into account. Statement revised in the revised ES.
10-192	10	3	45	3	55	A recent study (Kishtawal et al. 2012) that used the observations during satellite era (1986-2010) concluded that tropical cyclone intensification rates are increasing in all the global basins. [Government of India]	Noted. This chapter is a chapter on attribution, ie causes not on the observed trends.
10-193	10	3	45			*“most of (at least 50%)” – as above .. [Tibor Farago, Hungary]	Taken into account. ES has been revised to use more than formulation
10-194	10	3	45			Same comment as above. Text is rather repetitive of what is in the preceding paragraph, though it is easy to see how this has happened, as we are in a different section. [Adrian Simmons, United Kingdom]	Taken into account. ES has been revised to avoid duplication.
10-195	10	3	46	3	46	For clarity, state "observed global mean warming" rather than " observed warming". [Martin Hoerling, United States of America]	Accepted. Global mean warming is used.
10-196	10	3	46	3	47	The sentence needs a rewrite in order to more clearly articulate the meaning of various cited numbers. As now written, it is not clear if the observed warming has been 0.6 K, or whether the 0.6 K refers to the greenhouse gas forced wamring. It is also unclear whether the range of 0.6 K to 1.4 K refers to a measure of uncertainty in the obseved warming, or in the estimated GHG forced warming. [Martin Hoerling, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-197	10	3	46	3	47	This sentence has multiple interpretations just like lines 16-18. [Dáithí Stone, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components

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10-198	10	3	47	3	47	Clarify in this sentence that at least one of the factors mentioned must have contributed a net cooling. Otherwise the reader has to puzzle out by him- or herself why greenhouse gas having contributed up to 1.4 K to a net warming of only 0.6 K. [J. Graham Cogley, Canada]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-199	10	3	47	3	49	Clarify that some forcings are negative and have offset the greenhouse gas contribution to warming. [Government of Australia]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-200	10	3	47	3	50	So is it these 'other forcings' that -- despite contributing only 'to the year to year and decade to decade variability' -- counteract the longer term 'greater than the total observed warming' due to greenhouse gases, and thus produce the net observed warming of 0.6K? This is not clear from the text. [Government of United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-201	10	3	47	3	50	The sentence beginning "Other forcings…." does not follow logically from the prior content of this paragraph, and requires revision. The confusion stems from the fact that the prior sentences were strictly concerned with globally averaged conditions, whereas one gets the impression that this subsequent sentence is referred to variability in the climate system more generally (e.g. regionally rather than just globally). If that is not the case or the intent, then a remedy which statse that these other factors also contribute to variability in "globally averaged surface temperatures" rather than the open-ended reference to "the climate system" could suffice. [Martin Hoerling, United States of America]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components and the ambiguity concerning forcings and the climate system removed.
10-202	10	3	47		49	Reads as if these are all random changes: the greatest is likely to have been consistent anthropogenic aerosol effects [William Ingram, United Kingdom]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components.
10-203	10	3	49	3	49	replace "decade to decade" by "decadal to multidecadal" (see Deser et al. 2012 NCC). [Laurent Terray, France]	Taken into account. The ES has been revised substantially to make it clearer how the observed warming is made up of the attributable forced components
10-204	10	3	50	3	50	Please explain the term "external forcing" the first time it appears. E.g. that this is both natural and anthropogenic forcing, and includes everything that causes changes in the climate except the internal climate variability. [Government of NORWAY]	Accepted. This has been implemented in the revised ES.
10-205	10	3	50	3	50	What is meant by 'external' here: anthropogenic and/or natural? [Government of United States of America]	Accepted. External forcing is explained when it is first used.
10-206	10	3	50	3	50	This statement is nonsense. Insolation is an external forcing. Unless you can demonstrate that greenhouse gases caused ALL warming over a certain period it logically follows that external forcing must cause some warming, assuming that warming occurs. Remove the statement. [John McLean, Australia]	Taken into account. This statement has been deleted from the revised ES as does not provide a major conclusion of the chapter.
10-207	10	3	50	3	52	Suggest to include a measure of confidence in this explanation for the slowdown in observed global warming since 1998. For example a "high confidence" statement is made in Section 10.3.1.1.4 Page 19 Line 46. [Oliver David Andrews, United Kingdom]	Taken into account. The ES draws on the revised Box 9.2 for a statement on the recent hiatus with a confidence level.
10-208	10	3	50	3	52	“While the trend in global mean temperature since 1998 is not significantly different from zero, it is also consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models.” This sentence would be more powerful if turned around “The trend in global mean temperature since 1998 is consistent with natural variability superimposed on the long-term anthropogenic warming trends projected by climate models. While the trend since 1998 is not significantly different from zero, it is not statically robust to determine long-term trends from short periods of data.” (A further statement could be made on an appropriate period from which to draw conclusions on trends. Mention could also be made of the fact that 2005 and 2010 were the warmest years on record while 2011 was the warmest la Niña year on record). [Government of Australia]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2

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10-209	10	3	50	3	52	The message of this sentence is lost by starting with a negative statement. Suggest replacing sentence with 'Global mean temperatures since 1998 are consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models.' [Government of Australia]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-210	10	3	50	3	52	In the original texts, “While the trend in global mean temperature since 1998 is not significantly different from zero, it is also consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models”, both this sentence and main report do not clearly explain why CMIP5 model cannot simulate that the global warming is not significantly different from zero after 1998. It is recommended to modify the above text, based on Figure 10.5, and related formulations on Page 18-19 in order to further explain the flat increase of temperature after 1998. [Government of China]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-211	10	3	50	3	52	Suggest to include a measure of confidence in this explanation for the slowdown in observed global warming since 1998. For example a "high confidence" statement is made in Section 10.3.1.1.4 Page 19 Line 46. [European Union]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-212	10	3	50	3	52	Why does IPCC consider a trend since 1998, a very strong El-Nino year und thus an outstanding warm year in the temperature time series? At least it should be mentioned that 1998 is a very warm year due to internal climate variability and that the considered time period is considerably shorter than the typical time period applied in the definition of climate. [Government of Germany]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-213	10	3	50	3	52	Why is this finding -- relating to a relatively short period in observational record -- important to report here? And what is the significance of 1998 (large El Nino?). Please clarify. [Government of United States of America]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-214	10	3	50	3	52	A revision is required to repair the disruption in the temporal flow of the paragraph. Wheras all of the prior text in the parapgraph pertains to 1951-2010, the writing jumps back to an assessment of early 20th century conditions, and then springs forward to an assessment of conditions since 1998. This creates confusion and detracts from the main point of the paragraph concerning trends since 1951. One option is to consider beginning a new paragraph with the current sentence "It is very likely that early....", perhaps augmented with suitable segue material. [Martin Hoerling, United States of America]	Taken into account. The ES has been substantially revised around headings of main findings and therefore does not jump around in this way.
10-215	10	3	50	3	52	What does "significant" mean here? Statistically significant? If so, give details. Be more specific. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-216	10	3	50	3	52	Ther following wording is suggested to add clarity: While the trend in global mean temperature since 1998 is not significantly different from zero, this trend is still consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models. [Klaus Radunsky, Austria]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-217	10	3	50	3	52	The reasons since 1998, it should be mentioned clearly. [Zong-Ci Zhao, China]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-218	10	3	51	3	51	By "natural" do you mean "internally generated" or do you attribute it to natural forcings? [Jonathan Gregory, United Kingdom]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-219	10	3	51	3	52	Revision is needed that would state clearly what a lack of warming since 1998 has to do with the main section's titled theme "Evidence for warming". One gets the impression that apologetics is at play here, and perhaps an effort by the authors to anticipate a criticism against the evidence for warming based on recent events. In either case, this brief allusion to a sub-period having little global warming creates more confusion than clarity. One might ask why the authors fail to highlight the period of rapid warming during the 15-year period preceeding 1998, for instance. The danger of cherry picking is evident. For the executive summary in particular, it may be best to simply focus on a particular and suitably long period for discussing the evidence for warming. It goes without saying here that trends over a decade or so may not constitute strong scientific evidence for nor against GHG warming. [Martin Hoerling, United States of America]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-220	10	3	51	3	52	replace "natural variability" by "natural internal variability" or just "internal variability" to avoid confusion with variability due to natural external forcings. [Laurent Terray, France]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-221	10	3	51	3	53	We think it is important to address better that the global mean temperature between 1998 and 2011(?) has not	Taken into account. The ES provides the attribution

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						increased. We propose to rephrase the sentence. "The global mean temperature has not changed significantly between 1998 and YYYY. " The next sentence about the explanation for this (natural variability) should be explained better. What is the natural variability that counteracts the increase in GHG and warming? [Government of NORWAY]	headline statement from the new Box 9.2
10-222	10	3	51			Not significantly different from zero? So what? The December-July trend might be statistically significant, but not the 12-month trend. Why pick on 1998? Does this hold for 1999, 2007, 1995, etc. too? Otherwise you seem to be creating a hole from which you then have to wriggle yourself out. [Dáithí Stone, United States of America]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-223	10	3	52	3	52	"simulated"; reserve "projected" for the future. [J. Graham Cogley, Canada]	Taken into account. The ES provides the attribution headline statement from the new Box 9.2
10-224	10	3	54	3	54	Change "through increased" to ", as shown by increases in". [J. Graham Cogley, Canada]	Taken into account. This statement does not appear in the revised ES.
10-225	10	3	54	3	55	No credible evidence to support this claim. Delete it. [John McLean, Australia]	Rejected. There is credible evidence to support this claim which is provided in chapter 3. However because this evidence is assessed in Chapter 3 this statement does not appear in the revised ES of chapter 10 for reasons of brevity and avoiding overlap with chapter 3.
10-226	10	3	54	4	4	Merely biased opinions, not science [Vincent Gray, New Zealand]	Rejected. The conclusions are based on the assessment and are tracable back to the relevant sections of the chapter. In the revised ES this is made explicit by providing the information to subsection level as to where the assessment comes from.
10-227	10	3	54		55	“increased subsurface temperatures” → “subsurface warming” [William Ingram, United Kingdom]	Taken into account. Because this evidence is assessed in Chapter 3 this statement does not appear in the revised ES of chapter 10 for reasons of brevity and avoiding overlap with chapter 3.
10-228	10	3	54			"radiative imbalance is currently taken up by the oceans" is awkward and does not make physical sense. Please fix. [Chris Forest, United States of America]	Taken into account. Because this evidence is assessed in Chapter 3 this statement does not appear in the revised ES of chapter 10 for reasons of brevity and avoiding overlap with chapter 3.
10-229	10	3	54			More than 90%. This is very precise, is there an associated likelihood/confidence assessment? [Dáithí Stone, United States of America]	Taken into account. However because this evidence is assessed in Chapter 3 this statement does not appear in the revised ES of chapter 10 for reasons of brevity and avoiding overlap with chapter 3.
10-230	10	3	55	3	56	See Point 5. [Oliver David Andrews, United Kingdom]	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing.
10-231	10	3	55	3	56	See comment 5 related to page 3 - lines 19 to 22 [European Union]	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing.
10-232	10	3	55	3	56	Revise the sentence beginning "It is very likely…" to be symmetric in its terminolgy with the sentence on line 33 that begins.."We conclude that it is extremely likely…". For instance, is "external forcing" as used here the same meaning as "human activities" as used in the earlier sentence? If so, please choose one, and dont interchange, unless you are doing so for a reason. On a separate matter, why is the confidence lower for the attribution of ocean warming lower than for the attirbution of global average temperature warming? One might wonder, given the prior sentence (line 54) that asserts over 90% of the earth's radiative balance is taken up	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing to ocean warming. Also the revised ES does not have a likelihood statement on whether human activities have caused more than half of the warming. Finally the phrase external forcing is defined where it is first

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						by the oceans. This will need to be clarified as a key matter of tracability, and it will be especially important to explain the greater confidence in attribution of warming in that part of the earth system where less than 10% of the radiative imbalance is taken up versus that part where over 90% is taken up. [Martin Hoerling, United States of America]	used.
10-233	10	3	56	3	56	What is meant by 'external' here: anthropogenic and/or natural? For example, note how 'anthropogenic' forcings have been stated explicitly in the rest of the paragraph, as opposed to merely 'external'. [Government of United States of America]	Taken into account. The revised ES includes a statement on the contribution of anthropogenic forcing not external forcings.
10-234	10	3	56	3	57	This claim is unsustainable. Downwelling radiation from CO2 penetrates only a few microns at the ocean surface and rapidly disappears in evaporation and convection. Not only is there no method by which anthropogenic greenhouse gas emissions can cause dep ocean warming, but also chapter 3 failed to describe any physical process by which heat could sink. Remove the statement. [John McLean, Australia]	Rejected. The assessment of chapter 3 shows robust evidence for ocean warming and sea level rise from observations and section 10.4 shows robust evidence for this warming being anthropogenic.
10-235	10	3	56	3	57	It seems awkward having “extremely likely” attached to the sea level statement while only “very likely” is attached to the ocean warming statement. I know they are not necessarily inconsistent, but it stands out. [Dáithí Stone, United States of America]	Taken into account. The ES has been revised to state that "this anthropogenic ocean warming has contributed to global sea level rise", ie a statement of fact.
10-236	10	3	57	3	57	Please consider to use a different word then "steric", as it is difficult to understand. [Government of NORWAY]	Accepted. The word "steric" is not used in the revised ES.
10-237	10	3	57			Will ES readers know what steric means? [Dáithí Stone, United States of America]	Accepted. The word "steric" is not used in the revised ES.
10-238	10	3		5		There isn't any traceable account given to the chapter? Is WGI not doing this? [Dáithí Stone, United States of America]	Accepted. The revised ES provides tracable subsection numbers back to the chatper.
10-239	10	3		5		One third of the chapter's title is “from global to regional”, probably a higher fraction of the chapter's (non-methodological) content is regional, but only about one twelfth of the ES deals with regional assessment. Why the discrepancy? [Dáithí Stone, United States of America]	Taken into account. The problem here is with the headings rather than the content of the ES which does reflect the global to regional nature of the assessment in the body of the chapter. The headings have been revised to better reflect the nature of much of the assessment being from global to regional.
10-240	10	4	2	4	4	Remove these claims because they are unsustainable given the absence of warming for the last 15 or 16 years, a period during which greenhouse gas concentrations increased. [John McLean, Australia]	Rejected. The assessment takes account of the global warming trends over the last 15 years which are assessed in a new box in chapter 9.
10-241	10	4	2			*dominated by the emissions of greenhouse gases [Tibor Farago, Hungary]	Taken into account. Statement has been revised to "dominated by forcing due to greenhouse gases"
10-242	10	4	6	4	6	This section on the Hydrological Cycle would benefit from a statement on evaporation trends and attribution. [Government of Australia]	Rejected. Based on the assessment given in chapter 2 there is insufficient observational evidence of global evaporation trends over the last 5 decades to base an attribution assessment on.
10-243	10	4	6	4	6	Here the term "hydrological cycle" is used while there in several other parts of the report the term "water cycle" is used. Is there a special reason? Consider to use one of the terms for consistency. [Government of NORWAY]	Accepted. Changed to water cycle. Not however that both terms are defined in the glossary where they are regarded as synonymous.
10-244	10	4	6	4	21	This summary on hydr. cycle apparently is not fully in line with the relevant part of Ch. 2 (see e.g. the exec summary on page 4 and later the details) where in some cases even the statements of the AR4 are reconsidered .. Thus it would be important to have some better consistency with the relevant part of Ch 2. at least because the “attribution” related conclusions are especially critical for the policymaker readers .. [Tibor Farago, Hungary]	Taken into account. There has been coordination between chapters 2 and 10 to ensure the assessment is consistent across the chapter. Note that this relates mainly to details in the chapter body.
10-245	10	4	8	4	8	Change "detection of anthropogenic influence on" to "attributability to human influence of detected changes in". [J. Graham Cogley, Canada]	Taken into account. The ES has been substantially revised including this statement.

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10-246	10	4	8	4	10	Make more concise: "Consistent new evidence from both atmosphere and ocean points to anthropogenic influence on the water cycle since 1950." [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The ES has been substantially revised including to make it more concise.
10-247	10	4	8	4	12	Begin a new sentence after "cycle", and mention the multiple datasets sooner. "The consistency of the evidence from both atmosphere and ocean in multiple datasets points …". [J. Graham Cogley, Canada]	Taken into account. The ES has been substantially revised to improve clarity.
10-248	10	4	8	4	21	Surely the reported changes would also be consistent with naturally-driven variations in temperature, so add this to the paragraph. [John McLean, Australia]	Rejected. The ES correclty summarises the overall assesssment - where likelihood/confidence language is used to convery the strength of evdence.
10-249	10	4	8	4	54	There is no evidence that these changes are caused by humans [Vincent Gray, New Zealand]	Rejected. The ES correclty summarises the overall assesssment - where likelihood/confidence language is used to convery the strength of evdence.
10-250	10	4	10	4	11	Revise to read "…zonal patterns of precipitation over land". [Martin Hoerling, United States of America]	Accepted. Change implemented.
10-251	10	4	11	4	13	Seems to be partially repeated at the end of this paragraph. [Jochem Marotzke, Germany]	Taken into account. ES has been substantially revised to improve clarity and to avoid repetition.
10-252	10	4	13	4	13	Delete "observed and". [J. Graham Cogley, Canada]	Taken into account. ES has been substantially revised including relating to this point.
10-253	10	4	13	4	13	I suggest replacing "consistent with" by "most economically explained by". [J. Graham Cogley, Canada]	Taken into account. ES has been substantially revised to improve clarity.
10-254	10	4	13	4	13	Delete "observed and" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. ES has been substantially revised including relating to this point.
10-255	10	4	14	3	14	Please explain what you mean by "intensified global water cycle" [Government of NORWAY]	Taken into account. The word is not used in the revised ES.
10-256	10	4	15	4	15	"moisture content" seems to be an informal synonym of "humidity", which would be better here. Moisture certainly includes the liquid phase, and probably in most readers' minds the solid phase as well. The observations referred to are presumably of vapour. [J. Graham Cogley, Canada]	Accepted. Mositure content changed to humidity.
10-257	10	4	15	4	15	Revise to read "…observed increase in global average atmospheric moisture content…". Also, indicate over what period these increases have been observed. [Martin Hoerling, United States of America]	Rejected. The Santer et al papers consider patterns of atmospheric moisture content not just global average atmospheric moisture content. As Santer et al concludes "the dissimilarity of the water vapor fingerprint and the leading noise patterns does not. This dissimilarity is the main explanation for the robustness of our D&A results." Therefore the attribution to anthropogenic influence is based on the spatial pattern of the changes in these results.
10-258	10	4	15	4	16	What does "global scale changes on precipitation patterns over land" mean? Please clarify. [Martin Hoerling, United States of America]	Taken into account. Changed to "global land precipitation changes"
10-259	10	4	16	4	16	clarify 'reductions in low latitudes', Zhang et al shows increases in the equatorial tropics. [Government of Australia]	Taken into account. Refefence to changes in low latitudes deleted from ES.
10-260	10	4	17	4	17	", and", not "and, ". [J. Graham Cogley, Canada]	Taken into account. ES substantially revised.
10-261	10	4	17	4	17	By "natural" do you mean "internally generated" or do you attribute it to natural forcings? [Jonathan Gregory, United Kingdom]	Accepted. We mean internally generated and have changed statement accordingly.
10-262	10	4	17	4	17	omit 2 " , ": … and the large on observed precititation… ?; " - " instead? [Helga Nitsche, Germany]	Taken into account. ES statement has been revised.
10-263	10	4	19	4	21	Is this statement conditional? It sounds like the likely statement “observed changes in ocean... salinity” is conditional on the “changes in the hydrological cycle” being anthropogenic. [Dáithí Stone, United States of	Taken into account. The salinity statement has been revised to a straight forward statement about

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						America]	anthropogenic influence on salinity.
10-264	10	4	20	4	20	Clatify what is meant by "…are due in part to…". Previously, the summary was careful to indicate that "most" is intended to mean at least 50%. Is "in part" also meant to mean more than 50%? Or perhaps do you mean more than 5%? Without clarification, this qualifier leads to confusion, and the sentence should be rewritten. [Martin Hoerling, United States of America]	Taken into account. Statement has been revised to "made a substnatial contribution" to.
10-265	10	4	23			"since 1950" [Peter Guttorp, United States of America]	Taken into account. Statement has been revised to state that it is since 1979.
10-266	10	4	25	4	25	"since 1950". [J. Graham Cogley, Canada]	Taken into account. Statement has been revised to state that it is since 1979.
10-267	10	4	25	4	25	Insert 'since' between '(high confidence)' and '1950' [Government of Australia]	Taken into account. Statement has been revised to state that it is since 1979.
10-268	10	4	25	4	25	Revise to write "reduction in Arctic sea ice cover" rather than "Arctic sea ice retreat". Reconsider the current statement regarding Arctic sea ice extent since 1950. A graph of the monhtly sea ice extent anomaly (e.g., see NSIDC web site) reveals a decline commencing in about 1990. The draft sentence gives the impression of a decline happening much earlier. Also, and perhaps most importantly, I recommend a reconsideration of the stated confidence. The statement "It is likely that anthropogengic forcings have contributed to ...." comes across as a weak statement, given the statement made in AR4 and also given the subsequent observations of reduction in sea ice extent in the years following AR4. Section 9.5.5.1 of AR4 Ch9 states "the decline in arctic sea ice extent and its thinning appears to be largely, but not wholly, due to greenhouse forcing". That statement is a stronger one than the current draft report. The authors will need to explain, elsewhere in Ch10, why the assessed confidence has declined since AR4. [Martin Hoerling, United States of America]	Taken into account. Statement has been revised to a "very likely" statement and since 1979.
10-269	10	4	25	4	25	typo: "since 1950" [Albert Klein Tank, Netherlands]	Taken into account. Statement has been revised to state that it is since 1979.
10-270	10	4	25	4	25	Should read "since 1950" [Jochem Marotzke, Germany]	Taken into account. Statement has been revised to state that it is since 1979.
10-271	10	4	25	4	25	missing word: 'since' 1950 ? [Helga Nitsche, Germany]	Taken into account. Statement has been revised to state that it is since 1979.
10-272	10	4	25	4	25	According to Section 10.5.1.1. and Table 10.1 it is very likely - not just likely - that anthropogenic forcings have contributed to Arctic sea ice retreat. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Statement has been revised to a "very likely" statement.
10-273	10	4	25	4	25	"...ice retreat (high confidence) since 1950…" [James Renwick, New Zealand]	Taken into account. Statement has been revised to state that it is since 1979.
10-274	10	4	25	4	25	Add "since" before "1950". [Christian-D. Schoenwiese, Germany]	Taken into account. Statement has been revised to state that it is since 1979.
10-275	10	4	25	4	25	since' should be added prior to 1950 here. [Peter Thorne, United States of America]	Taken into account. Statement has been revised to state that it is since 1979.
10-276	10	4	25	4	25	This statement the Arctic sea ice retreat is considerably weaker than the corresponding statement given on page 38, line 3. Please ensure consistency. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Statement has been revised to a "very likely" statement.
10-277	10	4	25	4	26	The following wording is suggested to add clarity: It is likely that anthropogenic forcings have contributed to Arctic sea ice retreat (high confidence) since 1950 and to the increased surface melt of the Greenland ice sheet since 2000. [Klaus Radunsky, Austria]	Taken into account. Statements revised in ES.
10-278	10	4	25	4	26	Does the likely assessment apply to both the sea ice and Greenland together, each separately, or just one? [Dáithí Stone, United States of America]	Taken into account. ES now has separate statements on Arctic sea ice and on Greenland.
10-279	10	4	25	4	26	The Greenland statement is counterintuitive considering that in the previous page you mentioned that the planet has not warmed since 1998. [Dáithí Stone, United States of America]	Rejected. There has been considerable warming at high Northern latitudes since 1990

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10-280	10	4	25	4	27	Suggest to break this into two sentences since we have higher certainity as to the anthropogenic contribution to observed decreases in Arctic sea ice ('very likely' Sect. 10.5.1.1 Page 38 Lines 1-4) than we do for changes in Greenland surface melt ('likely' although no formal detection, Sect 10.5.2.1). This distinction would improve clarity as to our understanding of observed changes in the cryosphere for policymakers. [Oliver David Andrews, United Kingdom]	Taken into account. ES now has separate statements on Arctic sea ice and on Greenland.
10-281	10	4	25	4	27	Suggest to break this into two sentences since we have higher certainty as to the anthropogenic contribution to observed decreases in Arctic sea ice ('very likely' Sect. 10.5.1.1 Page 38 Lines 1-4) than we do for changes in Greenland surface melt ('likely' although no formal detection, Sect 10.5.2.1). This distinction would improve clarity as to our understanding of observed changes in the cryosphere for policymakers. [European Union]	Taken into account. ES now has separate statements on Arctic sea ice and on Greenland.
10-282	10	4	25	4	29	1st, 2nd and 4th sentences/statements: it would be useful to indicate (for the non-climate expert readers) that in this case the anth. forcings basically have acted through the surface level warming. [Tibor Farago, Hungary]	Taken into account. ES has been revised to include more explanatory statements.
10-283	10	4	25	4	31	You have no empirical evidence to support your claims that anthropogenic forcing is to blame, so remove them all. This report is supposed to be based on empirical science, not speculation. [John McLean, Australia]	Rejected. The evidence is set out in the report as delineated in the subsections listed.
10-284	10	4	25	4	31	There are just so many confidence statements in here that I got very confused. But I think there is at least one case where the language is contradictory. It would be useful to simplify the characterisation of this issue in terms of confidence to the extent possible. [Peter Thorne, United States of America]	Taken into account. ES has been revised to have separate statements under distinct headings to aid clarity.
10-285	10	4	25			*since 1950 .. [Tibor Farago, Hungary]	Taken into account. Statement has been revised to state that it is since 1979.
10-286	10	4	25			Why is this conservative statement made about attribution for sea ice: It is likely that anthropogenic forcings have contributed to Arctic sea ice retreat (high confidence) 1950" compared with the statement on Ch. 10 p. 38 (line 3-4): "it is very likely that anthropogenic forcing is a major contributor to the observed decreases in Arctic sea ice." [Thomas Knutson, United States of America]	Accepted. Statement has been revised to a "very likely" statement.
10-287	10	4	25			“1950” or “since 1950”? [Dáithí Stone, United States of America]	Taken into account. Statement has been revised to state that it is since 1979.
10-288	10	4	25			Why the sudden confidence statement on a likely statement? This is the first case. [Dáithí Stone, United States of America]	Taken into account. High confidence removed from likelihood statement.
10-289	10	4	26	4	27	One of the main issues with this sentence is that we are not able to estimate accurately the magnitude of the internal variability of the Antarctic sea ice extent. The observation time series are probably too short and the models do not adequately reproduce the variability compared to observations over the last 30 years (see for instance Zunz et al. 2012, cited in Chapter 9). This is a clear illustration to me of the limitations underlined page 5, lines 36-38. So, to my point of view, we still have low confidence on the hypothesis that the small net increase in Antarctic sea ice extent since 1979 is consistent with natural variability. Additionally, it is said page 38, lines 33-34 that we have low confidence in the scientific understanding of the observed increase, so a low confidence on the related issue of the compatibility of this trend with internal variability seems logical for me. [Hugues Goosse, Belgium]	Accepted. ES statement has been revised.
10-290	10	4	26	4	31	It seems more logical to have the final sentence on Antarctic Ice Sheet mass balance moved up to come on line 27, after the sentence on Antarctic Sea Ice. Strange now to have sentences on snow cover, permafrost, and glaciers, and then going back to Ice sheets again. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. The ES has been reorganised to have a logical flow with first sea ice (Arctic and Antarctic), then ice sheets and glaciers, then snow cover.
10-291	10	4	27	4	27	Why mentioning the positive trend since 1990 and not since 1979 as discussed in Chapter 4 and page 10-38. [Hugues Goosse, Belgium]	Accepted. ES Statement has been revised to refer to since 1979.
10-292	10	4	27	4	29	It is unclear if these two findings relate to global or regional observations. If they are global-scale findings 'globally' should be inserted to make findings clearer. [Government of Australia]	Taken into account. ES has been revised to make clear what is being referred to, ie Northern hemisphere snow cover. Permafrost statement has been removed.

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10-293	10	4	28	4	29	A likely statement on a significance statement? [Dáithí Stone, United States of America]	Taken into account. The statement has been revised to avoid using a "significant" formulation.
10-294	10	4	29	4	29	Change "diminished significantly" to the less vague "lost significant mass". [J. Graham Cogley, Canada]	Taken into account. The statement has been revised to avoid using a "significant" formulation.
10-295	10	4	29	4	30	A low confidence statement on a significance statement? [Dáithí Stone, United States of America]	Taken into account. The statement has been revised to avoid using a "significant" formulation.
10-296	10	4	29	4	31	Not clear, something is missing in this phrase [Moira Evelina Doyle, Argentina]	Taken into account. The statement has been revised to avoid using a "significant" formulation.
10-297	10	4	29	4	31	Due to a low level of scientific understanding there is low confidence that anthropogenic forcing is a significant factor of the observed loss of the mass of the Antarctic ice sheet since 1990. [Klaus Radunsky, Austria]	Taken into account. The statement has been revised to avoid using a "significant" formulation.
10-298	10	4	29			“factor” to “factor behind”? [Dáithí Stone, United States of America]	Taken into account. ES statement has been revised.
10-299	10	4	30	4	30	typo: "factor in" [Albert Klein Tank, Netherlands]	Taken into account. ES statement has been revised.
10-300	10	4	30	4	30	missing word:..factor 'of' observed.. [Helga Nitsche, Germany]	Taken into account. ES statement has been revised.
10-301	10	4	30	4	30	Add "in" before "observed". [Christian-D. Schoenwiese, Germany]	Taken into account. ES statement has been revised.
10-302	10	4	30	4	31	Change "factor" to "contributor to" and delete the superfluous "balance". [J. Graham Cogley, Canada]	Taken into account. ES statement has been revised.
10-303	10	4	30			*factor of the observed [Tibor Farago, Hungary]	Taken into account. ES statement has been revised.
10-304	10	4	35	4	36	The statement regarding temperature extremes lacks precision. Revision is required that states what manner of temperature extremes are being assessed (daily extremes, warm nights, monthly extremes, etc). . It needs to be revised also to indicate over what geographical domain this statement of confidence of attributable change applies to (e.g., the global scale?, continental scale?, regional scale?). In the SREX Summary for Policy Makers for instance, it is clearly stated that "it is likely that anthropogenic influences have led to warming of extreme daily minimum and maximum temepratures at the global scale". The current statement in this draft is too sweeping. Regarding the increase in confidence since SREX, I recognize that section 10.6.1 gives justification based on the results of published studies subsequent to SREX, though it is not clear that those additional studies permit the application of strong confidence language to also cover all geopgraphical areas and all manners of temperature extremes, as this draft statement could lead a reader to believe. [Martin Hoerling, United States of America]	Taken into account. The revised ES specifies type of extreme and spatial scale.
10-305	10	4	35	4	37	Heatwaves are caused by stationary or quasi-stationary pressure cells constantly directing streams of warm air to specific locations. (Ref: IPCC 4AR chapter 3 discussion of the 2003 European heatwave.) Claiming that CO2 emissions caused the pressure cells to halt defies logic, so remove the statement. [John McLean, Australia]	Rejected. Assessment supports statement.
10-306	10	4	37	4	39	Concerning the attribution of precipitation, the statement is neither consistent with SREX, nor consistent with subsequent language in section 10.6.1.2. Revision is thus required. For instance, SREX (section 3.3.2) states "there is medium confidence that anthropogenic influence has contributed to changes in extreme precipitation at the global scale". In this draft, the authors explicitly call out a particular type of precipitation change, i.e. "increases in frequency of heavy preciptation events". The report also says this change occurs "over land areas", rather than at the global scale as in SREX, thus giving an impression of regional applicability. Regarding internal inconsistency, it is stated in section 10.6.1.2 that "there is medium confidence that anthropogenic forcing has contributed to intensification of extreme precipitation at the global scale..." , quite different from the statement in this Executive Summary which addresses increases in frequency of heavy precipitation events. [Martin Hoerling, United States of America]	Taken into account. ES has been revised including the removal of the word "events" and to refer to global scale in order also to be consistent with underlying chapter.
10-307	10	4	37	4	39	You have no evidence to suipport your assertion, so remove it. [John McLean, Australia]	Rejected. Evidence is provided in the chapter as per the referred to subsections.
10-308	10	4	39	4	41	Mohanty et al. (2012) have studied the tropical cyclone activities over the north Indian Ocean using 120 years	Rejected. While this information in these papers is

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						observational (1891–2010) data obtained from IMD. They divide the whole period into two equal slabs of 60 years i.e. from 1891-1950 is first slab and 1951-2010 is second slab. Their study indicates that the increase in intensity of severe cyclones is more in the second slab i.e. during 1951-2010. Kishtawal et al. (2012) have examined the trend of TCs intensity over all basins (1986-2010) (except north Indian ocean) and found a reduction in time by about 9 hours to mature from 64 kt to average peak intensity (~104 kt) during past 25 years, which in turn implies that the rate of intensification has been increased during last three decades. These studies suggest that peak intensity as well as rate of intensification of tropical cyclones have increased during last few decades. Observations confirmed that the increasing trend of surface temperature is more after 1950. Indirectly it can be assumed that there is a positive correlation in between changes in intensity of tropical cyclones and surface temperature, however further study is needed to confirm the influences of human activities on tropical cyclone activities. [Government of India]	interesting, this is observational information and so is not relevant to the subject of this chapter.
10-309	10	4	39	4	41	A sentence re-write is needed. First, what is meant by "insufficient observational evidence and limited evidence"? I suppose this is merely a typographical error. Second, the statement gives an incomplete indication for why the confidence is low. The fact that the list of reasons for the low confidence is shorter than given in SREX, for instance, could be interpreted to mean that some limitiing factors have been overcome. Specifically, unless new results have appeared since SREX, then the reasons for low confidence needs to include the fact that physical understanding of links between tropical cyclone characteristics and climate change is still incomplete. It needs to state also that tropical cyclone variability is large, preventing a detection of change, let alone an attribution to anthropogenic forcing. The language here in the Executive Summary fails to adequately refelct the synthesis stament in section 10.6.1.5, page10-45, lines 50-56. Lacking especially ius a statement that there is low confidence in having detected any long term increases in tropical cyclone activity. [Martin Hoerling, United States of America]	Taken into account. Typographical error corrected. Statement rephrased to reflect evidence provided in chapter.
10-310	10	4	40	4	41	Repetition of level of evidence is confusing - "insufficient observational evidence and limited evidence and low level of agreement". If there is limited non-observational evidence the nature of this limited evidence should be made clear, otherwise 'limited evidence' should be removed. [Government of Australia]	Taken into account. Statement rephrased to improve clarity and reflect evidence provided in chapter.
10-311	10	4	40	4	41	Awkward: "due to insufficient observational evidence and limited evidence and low level of agreement between studies." [Jochem Marotzke, Germany]	Taken into account. Statement rephrased to improve clarity and reflect evidence provided in chapter.
10-312	10	4	41	4	41	"limited evidence" of what? Please specify. This wording sounds vague and repetitive. [Government of United States of America]	Taken into account. Statement rephrased to improve clarity and reflect evidence provided in chapter.
10-313	10	4	41	4	41	Some statement on drought should be provided in this Executive Summary, given broad interest in that phenomenon. Also, there is a statement on drought appearing in the draft SPM, so one should also be given here in the Executive Summary. I recommend adding the statment, from section 10.6.1.3, pg 44, line 41-42 that "There is low confidence in attributing changes in drought over global land areas since the mid-20th century to human influence" [Martin Hoerling, United States of America]	Accepted. A statement on drought has been added to the ES.
10-314	10	4	41	4	41	Please delete "and limited evidence" -> this is already covered when you say there is insufficient observational evidence. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Statement rephrased to improve clarity and reflect evidence provided in chapter.
10-315	10	4	41			*and limited evidence provided by (the studies?) and .. [Tibor Farago, Hungary]	Taken into account. Statement rephrased to improve clarity and reflect evidence provided in chapter.
10-316	10	4	45	4	46	"Further evidence has accumulated for different parts of the world on the detection of climate change and its attribution to anthropogenic influence." [J. Graham Cogley, Canada]	Rejected. Previous formulation is clearer.
10-317	10	4	46	4	47	Define 'substantial'? Detected an anthropogenic influence at a particular significance level? Degree of certainity central to this statement. [Oliver David Andrews, United Kingdom]	Noted. Substantial means that it is significant and that it is a large fraction.
10-318	10	4	46	4	47	I suggest that it would be better to rephrase this sentence in a way which does not have the possible implication that it is unlikely in Antarctica. That is, the contrast concerns the confidence, not the likelihood. Could you convey the meaning that you have sufficient confidence, for every continent except Antarctica, that it is likely? [Jonathan Gregory, United Kingdom]	Accepted. Statement has been rephrased to avoid this implication.
10-319	10	4	46	4	47	Your statement is mere speculation. The absence of warming from 1945 to 1976, which I notice you don't	Rejected. Reasons for assessment is provided in the

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						mention, can be blamed on the dominance of ENSO conditions on the La Nina side of absolutely neutral the scale prior to the Pacific Climate Shift of 1976 (see 4AR), and the post-1977 warming blamed on the dominance of conditions on El Nino side. If there is any human influence at all it is not significant but minor and even negligible. Correct your statement. [John McLean, Australia]	chapter traceable back to the ES.
10-320	10	4	46		49	The 1st sentence reads as if things are different over Antarctica, not just less well known. "Over every continent except Antarctica, where there are large observational uncertainties in estimating temperatures..." or "Anthropogenic influence has ... mid-20th century over every continent except Antarctica, where there are large observational uncertainties in estimating temperatures."? [William Ingram, United Kingdom]	Accepted. Statement has been rephrased to avoid this implication.
10-321	10	4	47			it is NOT "surface temperature" but "near surface temperature", please consider it in ALL the text [Barbara Früh, Germany]	Rejected. This suggest would not aid clarity.
10-322	10	4	48	4	49	Are observational uncertainties the only reason? The warming over Antarctica is also not expected theoretically to warm as quickly as other land regions so could be a signal to noise issue. [Government of Australia]	Rejected. It is the large observational uncertainties that are the main reason precluding an assessment.
10-323	10	4	49	4	50	Warming of the Arctic is consistent with the increased vigour of the Hadley Circulation associated with ENSO conditions on the El Nino side of the scale. This coupled with the PDO and AMO provide a more plausible explanation for Arctic warming. Add words to this effect. [John McLean, Australia]	Rejected. The ES is consistent with the assessment provided in the body of fhe chapter.
10-324	10	4	49	4	50	What is Arctic land surface? Land within the Arctic Circle [John Mitchell, United Kingdom]	Taken into account. Changed to Arctic warming to reflect that the assessment is of both land and ocean.
10-325	10	4	50	4	51	"Detection and attribution to greenhouse gases are complicated at regional scales by the …". The reduced sample sizes, and therefore degrees of freedom, at regional scales should be mentioned here. [J. Graham Cogley, Canada]	Rejected. This is not a major reason to elevate to the ES.
10-326	10	4	50	4	54	These lines would be better at the start of the "From global to regional " section [John Mitchell, United Kingdom]	Rejected. The structure of the new ES is to start off with a summary statement then continue with the evidence for it (including uncertainties).
10-327	10	4	53	4	53	"warming", not "temperature". [J. Graham Cogley, Canada]	Accepted. Changed to temperature changes
10-328	10	4	53	4	53	It seems like a word "change/increase" is missing in the sentence", human influence has likely contributed to temperature …" [Government of NORWAY]	Accepted. Changed to temperature changes
10-329	10	4	53	4	53	Is the "likely" to denote calibrated language? temperature --> temperature change? [Jochem Marotzke, Germany]	Accepted. Likely italicised.
10-330	10	4	53			*contributed to temperature increase .. [Tibor Farago, Hungary]	Accepted. Changed to temperature changes
10-331	10	4	53			The wording should probably read: .. Has likely contributed to temperature change in many … [Klaus Radunsky, Austria]	Accepted. Changed to temperature changes
10-332	10	4	53			"likely" in italics? [Dáithí Stone, United States of America]	Accepted. Likely italicised.
10-333	10	4	56	4	57	The sentence beginning "Changes in atmospheric…" , provides no information and should be deleted. [Martin Hoerling, United States of America]	Accepted.
10-334	10	4	56	4	57	What is your metric? Aren't circulation changes themselves climate change? [Dáithí Stone, United States of America]	Accepted. Sentence deleted.
10-335	10	4	57	4	57	"than elsewhere" - would be better to say "than the global-average changes". [Government of United States of America]	Taken into account. Sentence deleted.
10-336	10	4	57	5	1	It is even more likely that ENSO conditions can account for such changes, so include a comment to that effect. [John McLean, Australia]	Rejected. The statement is consistent with the evidence provided in the assessment.
10-337	10	4		4		The discussion on the Antarctic on this page would benefit from noting the differences in observations	Taken into account. Statement on Antarctic

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						between west and east Antarctica. See comment regarding page 20 - the discussion on attribution in Antarctica on page 20 is easier to interpret than the executive summary points which read as though there is no anthropogenic influence in Antarctica. [Government of Australia]	temperature rephrased.
10-338	10	5	1	5	5	GHGs are also thought to have played a role in the expansion of the Hadley Cell and poleward shift of the storm tracks. Needs rephrasing so emphasis is not all on ozone. [Government of Australia]	Accepted. This aspect has been included.
10-339	10	5	1	5	5	What is the confidence that a poleward shift in the SH Hadley cell has been detected, let alone that there is attribution? Davis and Rosenlof (2012) show considerable uncertainty in estimates of the trends in SH (DJF) Hadley cell extent, based on various indicators. [Martin Hoerling, United States of America]	Noted. The assessment here is only medium confidence for an effect of stratospheric ozone depletion.
10-340	10	5	1			it is suggested to include a definition of sea level pressure as it is a scientific term. [Klaus Radunsky, Austria]	Rejected. Sea level pressure is a standard and easily understood concept.
10-341	10	5	2	5	2	"poleward shift of the poleward border of the southern Hadley cell". Do not capitalize "austral". [J. Graham Cogley, Canada]	Accepted. Corrected
10-342	10	5	2	5	2	typo: "southern" [Albert Klein Tank, Netherlands]	Accepted.Corrected
10-343	10	5	2	5	2	Typo: southern [Jochem Marotzke, Germany]	Accepted.Corrected
10-344	10	5	2	5	2	misprint: … southern Hadley.. [Helga Nitsche, Germany]	Accepted.Corrected
10-345	10	5	2			“southerN” [William Ingram, United Kingdom]	Accepted.Corrected
10-346	10	5	2			“Austral” is sometimes capitalized, sometimes not – don't, for consistency with “boreal” throughout [William Ingram, United Kingdom]	Accepted.Corrected
10-347	10	5	2			Typo "souther" [Adrian Simmons, United Kingdom]	Accepted.Corrected
10-348	10	5	5			*a southward shift of the storm tracks /I guess:/ in the SH. [Tibor Farago, Hungary]	Accepted.Corrected
10-349	10	5	9	5	14	Remove the first sentence. Begin second sentence at "It is very unlikely…..". In that revised sentence, clarify what manner of reconconstructued temperatures variations are being discussed…e.g., regional, global and concerning time scales, decadal, centennial etc. Explicitly state the "natural forcings" being considered, e.g., solar and volcanic variability. Explicitly state what is meant by "natural internal variability", e.g. the flucutations of the coupled ocean-atmosphere-land-cyrospheric systems that take place in the absence of any external forcing, either natural of anthropogenic. [Martin Hoerling, United States of America]	Rejected. The first sentence has become our highlighted sentence in the revised ES structure.
10-350	10	5	9	5	17	I suggest you change "heat" to "energy". Energy is what you mean. Note also that natural variability does not only redistribute energy in the system", it can also change the total amount of energy in the system to some extent, even in the absence of any natural forcing. [Olivier Boucher, France]	Accepted
10-351	10	5	9	5	17	To be accurate this paragraph should report that there has been no statistically significant warming for the last 15 years. If you omit this highly relevant fact this report will look like the output of a lobbyist group. [John McLean, Australia]	Rejected. The temperature trends over the last 15 years are accounted for in the assessment.
10-352	10	5	10	5	12	Remove your statement because it is mere speculation. [John McLean, Australia]	Rejcted. Statement is based on assessment in chapter.
10-353	10	5	10	5	14	There is no corresponding information in the content of the Chapter. In the Chapter, 1400 is not mentioned and the conclusion is made only based on the results of Northern Hemisphere. [Xuemei Shao, China]	Taken into account. Statement revised to refer to Northern hemisphere temperatures
10-354	10	5	11	5	11	Delete "the climate system". [J. Graham Cogley, Canada]	Rejected. Climate system is what is meant here.
10-355	10	5	12	5	12	Naturally forced or internally generated or both? [Jonathan Gregory, United Kingdom]	Talken into account. Statement changed to internal variability.
10-356	10	5	12	5	12	I suggest omitting "reconstructed" because it's not clear what point it makes. It could be misinterpreted as being in opposition to some other kind of temperature. [Jonathan Gregory, United Kingdom]	Accepted.

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10-357	10	5	12			*it can be misinterpreted (primarily: the timing, i.e. “since 1400”): “unlikely that reconstructed temperatures since 1400 can be explained by natural internal variability alone” [Tibor Farago, Hungary]	Rejected. Unclear what is being proposed.
10-358	10	5	13	5	14	The ability of models with natural-only forcings to capture pre-industrial inter-decadal temperature variability is a strong argument supporting our understanding of the forced and unforced climate system, so this point should be less general (how much of the temperature variability is captured by the models? less qualitative than 'substantial') and be made more fully in Section 10.7.4 [Oliver David Andrews, United Kingdom]	Taken into account. This statement has been deleted to aid clarity given the difficulties of defining substantial in this context.
10-359	10	5	13	5	14	These "simulations:: conveniently omit the two main reasons for the increase in the :Mean Global Surface Temperature Anomaly:which are the changes in ocean oscillations (particularly EMSO) and the biases in weather staion temeperature measurements from uurban development and land use changes. And you actually admit that the temperature is not currently changing, anyway! [Vincent Gray, New Zealand]	Rejected. But the statement has been deleted to aid clarity given the difficulties of defining substantial in this context.
10-360	10	5	13	5	16	However is a conjunction; it joins two parts of the same sentence. To commence a sentence with "however" when it is used as a conjunction is very poor English. [John McLean, Australia]	This is debatable but in any case the sentence has been deleted. Since this sub-section's header addresses time scales of multi-century to millenia, the first statement regarding changes since 1950 appear ill-placed and inappropriate here.
10-361	10	5	13	5	16	Your statement is dishonest unless you can demonstrate that climate models fully and accurately describe all natural climate forces..TAR and 4AR (table 2.11, page 201) showed that this was not the case because many forces were poorly understood. On top of this McLean et al (2009) showed in Figure 7 that the ENSO was a very good indicator of global average lower tropospheric temperatures 7 months later, but 4AR described how ENSO models have poor predictive skill beyond 12 months. The logical conclusion is that little credence can be placed on the output of climate models and that it is false to make claims that assume the modelling of natural climate forces to be accurate. [John McLean, Australia]	Rejected. However the statement has been deleted Since this sub-section's header addresses time scales of multi-century to millenia, the first statement regarding changes since 1950 appear ill-placed and inappropriate here.
10-362	10	5	14	5	16	Since this sub-section's header addresses time scales of multi-century to millenia, the statements regarding changes since 1950 appear ill-placed and inappropriate here. Please remove, therefore, the sentence "However, such simulations…" The subsequent sentence is justifiable in this section since it attempts to place the post-1950 warming trend into a perspective of similar time scale trends occurring in reconstruction records. Please clarify, however, whether the phrase "residual internal variability" denotes an estimate of the natural internal variability. The introduction of this additional technical phrase merely creates confusion, and is best avoided. [Martin Hoerling, United States of America]	Accepted.
10-363	10	5	16	5	17	"range of trends of similar length estimated from reconstructions …". [J. Graham Cogley, Canada]	Taken into account. Sentence has been revised.
10-364	10	5	16	5	17	You have no genuine measurement of the average temperature of the earth's surface or its possible increase. The "Mean Global Surface Temperature Anomaly" is based on multiple varying samples and is unreliable. It is also prone to a variety of upward biases related to :anthropogenic: changes in population and development. It has also been almost constant for the past ten years. It is not currently "warming" [Vincent Gray, New Zealand]	Rejected. Assessment of the ability to measure global mean temperature is given in chapter 2 and shows that there is a robust measure of global warming.
10-365	10	5	16	5	17	Sentence is very awkward, with the two "estimated". [Jochem Marotzke, Germany]	Taken into account. Sentence revised.
10-366	10	5	16	5	17	Warming did not commence until 1977, which is almost 20 years after CO2 was known to be increasing, so discussion trends since 1950 is deceitful. GISP2 ice core records indicate several periods of more rapid warming in the last 10,000 years; your statement is dishonest if you don't put the rate of warming (only from 1977 to 1996) into its proper context. You should mentioning the relative scarcity of La Nina events across the period. [John McLean, Australia]	Rejected. Evidence is provided in the chapter for the assessment.
10-367	10	5	17	5	17	What is "residual internal variability"? [Jonathan Gregory, United Kingdom]	Taken into account. ES statement has been revised.
10-368	10	5	19	5	19	Revise header such that the Implications of "What", for climate system properties and projections is stated. [Martin Hoerling, United States of America]	Accepted. Title edited to Climate System Properties
10-369	10	5	19	5	19	Seems necessary to specify "implications of what?" [Jochem Marotzke, Germany]	Accepted. Title edited to Climate System Properties
10-370	10	5	21	5	22	Consider "…of those basic properties…that have implications" [Jochem Marotzke, Germany]	Accepted. Title edited to Climate System Properties

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10-371	10	5	21	5	32	Please include a definition of a transient climate response and an equilibrium climate response that highlights how the two are different. [Government of Australia]	Rejected. TCR and ECS are standard terms that are defined elsewhere in the report. It would be too cumbersome to repeat that here.
10-372	10	5	21	5	32	Please check the units of TCR, TRCE and ECS. [Government of Germany]	Taken into account. Units have been checked.
10-373	10	5	21	5	32	The ranges for ECS, TCR and TCRE also appear in the Exec Summ of ch12 (and the numbers are not the same for TCRE). I don't think they should appear in both chapters. I would say that they belong better in ch12, from the point of view of a reader. [Jonathan Gregory, United Kingdom]	Rejected. These quantities based on recent climate change are given here but is referred forwards to the synthesis statement given in chapter 12.
10-374	10	5	22	5	25	Revise sentence to read "transient climate response (TCR) of global mean temeprature change at the time of CO2 doubling which is estimated…..". Concerning the probable range of the TCR, it appears that the bpunding statement of 1°C on low end to no more than 3°C in high end is very similar to that reported in AR4. [Martin Hoerling, United States of America]	Rejected. Definition of TCR is left for elsewhere in the report.
10-375	10	5	23	5	23	Seems inconsistent to invoke models to arrive at an "observational constraint". [Jochem Marotzke, Germany]	Taken into account. Observational constraints phrase deleted.
10-376	10	5	23	5	23	I am surprised that I have to remind you that models cannot produce "evidence". The output of models amounts to nothing more than predictions derived from the input parameters and the assumptions embodied in the algorithms. Reword or delete. [John McLean, Australia]	Taken into account. Sentence rewritten.
10-377	10	5	25	5	27	Is this detection and attribution? [Dáithí Stone, United States of America]	Rejected. The assessment of what we can learn from recent climate change for climate system properties in this chapter.
10-378	10	5	25	5	32	Most sentences here need to be qualified with the phrase "Assuming that climate models are correct" [John McLean, Australia]	Rejected. Models are being used to understand the observations.
10-379	10	5	28	5	28	Is the units for the rate of warming per cumulative carbon emissions correct? Please consider to express this ratio differently, or explain the unit. [Government of NORWAY]	Taken into account. Units have been checked.
10-380	10	5	28	5	28	"TRCE" -> here and throughout the chapter should be replaced with transient climate response to cumulative carbon emissions (TCRE), see chapter 12. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. TRCE changed to TCRE.
10-381	10	5	29	5	30	I disagree that estimates of climate sensitivity (ECS) continue to indicate that it is 'very likely' that it exceeds 1.5 C, if they ever did. There are a number of studies suggesting a substantial probability that it is below 1.5 C (that includes studies showing a best estimate somewhat above 1.5 C, of course). Forster and Gregory, 2006: The climate sensitivity and its components diagnosed from Earth Radiation Budget data. Journal of Climate. 39-52. gave a central (highest probability) estimate of 1.6 C. Aldrin et al, 2012, Bayesian estimation of climate sensitivity based on a simple climate model fitted to observations of hemispheric temperatures and global ocean heat content. Environmetrics, 23, 253-271 give an identical central estimate for ECS of 1.6 C (they quote the mean of 2.0 C, but the mean is not an appropriate central estimate for a highky skewed distribution, and is sensitive to the parameterisation used, unlike the mode). Further, 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]	Taken into account. The ECS assessment has been revised.
10-382	10	5	29	5	30	Additionally, most of the observationally-based studies whose PDFs for climate sensitivity were featured in AR4 WG1 (in Figure 9.21) gave PDFs that appear to indicate serious defects in the related studies. See the reviewer's paper, submitted to Climatic Change in July 2012: Lewis, Noninformative prior distributions for observationally-based objective estimates of climate sensitivity PDFs (copies sent to the relevant IPCC lead authors in early August 2012). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account; this concern is discussed in the revised section 10.8
10-383	10	5	29	5	32	This paragraph relates to evidence from observations, not the results of climate model simulations (which may well be inconsistent with observations). As set out in more detail in my comments on Section 10.8.2, a 'likely' range of 2-4.5 C for ECS does not seem compatible with the best estimates from observational evidence cited	Taken into account; 'likely' range from observational constraints has been revised downward.

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						in other chapters of AR5 WG1. Rather, such estimates points to a best estimate for ECS of approximately 1.6 - 1.7 C, not far above 1.5 C. That estimate implies that the bottom of a valid 'likely' range for ECS cannot be as high as 2 C. That best estimate is also far too close to 1.5 C for values below that level to be considered 'unlikely', let alone 'very unlikely'. Based purely on observational evidence, the lower limit of the 'likely' range for ECS should be significantly below 1.5 C. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-384	10	5	29	5	32	Maintaining the same 'likely' range for ECS based on observational evidence as in AR4 is not scientifically credible given the large change from AR4 to AR5 in the best estimate for total aerosol forcing. The best estimate of total aerosol forcing in 2005 given in AR4 WG1, section 2.9.2, was -1.3 W/m^2, with a 5-95% range of -2.2 to -0.50 W/m^2. The AR4 estimate of Cloud albedo effect, called RFaci/AFaci in AR5, was based very largely, if not entirely, on modelling results. The corresponding best estimate in section 2.9.2 of AR4 WG1 for Total Anthropogenic radiative forcing (RF) in 2005 was 1.6 W/m^2, with a 5-95% range of 0.6 to 2.4 W/m^2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account, range revised down for observational constraint
10-385	10	5	29	5	32	The best observational-evidence based estimate of total (ari+aci) aerosol adjusted forcing (AF) given in AR5 WG1(Chapter 7, p.49, line 33) is -0.73 W/m^2, for 2011, with a standard deviation of 0.30 W/m^2 (implying a 5-95% range of -1.33 to -0.13 W/m^2), with the estimate for 2005 almost the same. The best estimate of and 95% range for Total Anthropogenic AF for 2011 is 2.24 (1.35 to 3.1 per Figure 8.17) W/m^2. However, this uses a best estimate of AFari+aci of -0.90 W/m^2 (Chapter 7, p.49, line 35) based on a composite of CMIP5 models, inverse estimates and observations. When adjusted to use the purely observational -0.73 W/m^2 best estimate of AFari+aci, the best estimate of Total Anthropogenic AF for 2011 becomes 2.41W/m^2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, chapter 7 provides forcing estimates.
10-386	10	5	29	5	32	Taking recent radiative imbalance to be 0.5 W/m^2, in line with the Levitus et al. 2012 (Figure 1) 0-2000m global ocean heat uptake trend of 0.45 W/m^2 over the last decade (which graph actually show lower heat uptake since 2005) plus an added allowance of 0.05 W/m^2 for heat uptake in other parts of the climate system, and to be zero in 1750, the implied increase in Total Anthropogenic AF, net of the change in radiative imbalance, from 1750 to 2011 based on purely observational estimates for AFari+aci is now estimated as 2.41 - 0.5 = 1.91 W/m^2. The corresponding estimate for 1750-2005 using the AR4 best estimate of Total Anthropogenic RF was 1.6 - 0.5 = 1.1 W/m^2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, dependence of results on heat uptake estimates and their uncertainty discussed in section 10.8; discussion updated to account for comment.
10-387	10	5	29	5	32	There has been a negligible increase in global mean temperature over the six years since AR4 compared with the six previous years (HadCRUT4: change from 0.45 C over 2000-05 to 0.46 C over 2006-11), and there has been no significant change in the main measures of internal climate variability (AMO, ENSO). There is little difference in the sum of solar and volcanic forcing in the six years since AR4 to that in the previous six years; both are very close to zero, so there is little difference between Total Forcing and Total Anthropogenic Forcing in either period. Therefore, the best estimate for 1750-2011 anthropogenic warming should be little different from that for 1750-2005, notwithstanding that the actual anthropogenic warming should be higher in 2011 due to the increase in anthropogenic forcing between 2005 and 2011. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The influence of the past decade on ECS estimates is now explicitly discussed in section 10.8
10-388	10	5	29	5	32	If the best estimate for ECS of 3 C in AR4 correctly corresponded to its best estimate of Total Anthropogenic RF, the implied 1750 - 2005 best increase in global temperature attributable to Total Anthropogenic RF (taking RF from a doubling of CO2 as 3.71 W/m^2) is 3.0 / 3.71 x 1.1 = 0.89 C. On that basis, the corresponding best estimate of ECS based on the AR5 best estimate of Total Anthropogenic AF using observational-only estimates for AFari+aci should be 3.71 x 0.89 / 1.91 = 1.7 C, not 3 C. (In reality, 1750-2005 anthropogenic warming may have been overestimated, leading to the 3 C observationally based estimate for ECS in AR4 having been too high.) [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, however, the best estimate is not discussed in this ES.
10-389	10	5	29	5	32	It cannot possibly be scientifically valid to maintain that current observational evidence supports the same 'likely' range for ECS as per AR4 when the best observational estimate, or even the best composite estimate, of total aerosol forcing in AR5 is far below the best estimate thereof per AR4. That point is reinforced by there having been no increase in global mean temperature (average over the six years between AR4 and AR5 compared with the previous six years) despite a continuing increase in Anthropogenic forcings, accompanied	Noted, and partly accepted. The aerosol estimates have been revised, as has been the assessment here.

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						by almost no change in other relevant factors. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-390	10	5	29	5	32	It is very important to keep the range for ECS estimated from observations – particularly instrumental observations, which as well as being more accurate also relate to the current climatic conditions – separate from that derived from AOGCM simulations. AOGCMs may, directly or indirectly, use forcings or other inputs that are not consistent with the best current observational evidence. That is a particular concern in relation to aerosol forcing, and also ocean effective vertical diffusivity, either or both of which may be substantially overestimated in AOGCMs, leading to excessive levels of ECS nevertheless producing realistic simulations of past warming. For instance, the NASA GISS global climate models now assume recent (2010) Total Aerosol forcing of -2.42 W/m^2 (http://data.giss.nasa.gov/modelforce/RadF.txt), over three times the best purely observational best estimate per AR5 of -0.73 W/m^2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted, see Chapter 7 for assessment of aerosol forcing.
10-391	10	5	29	5	33	Nic Lewis has shown how IPCC misrepresented the results of Forster and Gregory 2006 in AR4: http://judithcurry.com/2011/07/05/the-ipccs-alteration-of-forster-gregorys-model-independent-climate-sensitivity-results/ showing the difference between the original result and the IPCC result here: http://curryja.files.wordpress.com/2011/07/fig4_influence-of-prior.jpg The central estimate of Forster and Gregory 2006 for ECS is 1.6 C and in AR4 this was the only estimate for ECS that was purely observation based. Aldrin (2012) also come up with a central estimate of 1.6 C. Lindzen and Choi published an even lower estimate. These studies alone show that an ECS of 1.5 C is not that unlikely. The likely and very likely range for ECS have to be lowered. [Marcel Crok, The Netherlands]	Rejected - this is a comment on AR4, not AR5. Studies referenced (Aldrin et al., Lindzen and Choi) are assessed in 10.8.
10-392	10	5	30	5	32	State how the ECS estimate has changed since AR4. Section 9.6.4 of AR4 stated that "constraints from observed climate change support the overall assesment that the ECS is likely to lie between 2°C and 4.5°C, with a most likely value of approximately 3°C". It also states that the ECS is very likely grater than 1.5°C. This prior appraisal by AR4 thus appears to be unchanged from that rendered in this draft of the AR5. If so, then what is the reader to make of the introductory sentence of this section that states "More observational data have allowed a better characterization of basic properties of the climate syatem which have implications of the rate of future warming"? Please revise this section to indicate more clearly how such data have affected estimates of future warming, relative to expectations rendered at the time of AR4. [Martin Hoerling, United States of America]	Rejected, the overall assessment of ECS is given in chapter 12, a better place to discuss change in evidence. The technical text in 10.8, however, now discusses the role of recent data, hence partly accepted.
10-393	10	5	30	5	32	It is noted that this chapter of the AR5 does not address the literature with respect to ECS that has been published since the AR4 (2007). However, chapter 12 provides a much more in-depth discussion on ECS that also addresses the most recent literature. It is therefore suggested to skip this subchapter on ECS in chapter 10 in order to avoid inconsistences and confusion. [Klaus Radunsky, Austria]	Rejected, literature since AR4 is assessed as shown in figure 10.19, where only new studies are labelled.
10-394	10	5	32	5	32	Correct the C-cedilla. [J. Graham Cogley, Canada]	accepted
10-395	10	5	32			Well is it 6 or 7? Otherwise I don't see how you can assign a likely statement. [Dáithí Stone, United States of America]	accepted, revised.
10-396	10	5	34	5	34	Suggest considering whether "Remaining uncertainties" should be changed to "Additional uncertainties" or something similar. [Government of Canada]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-397	10	5	34	5	54	And so what is the prospect on these? Will we need a D&A chapter in the AR6? [Dáithí Stone, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-398	10	5	36	5	36	Change "is subject to" to "depends on". [J. Graham Cogley, Canada]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-399	10	5	36	5	36	We suggest that "detection" be "detection and attribution" here for completeness. [Government of United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.

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10-400	10	5	36	5	38	This sentence does not seem consistent: How can the capacity of models to adequately simulate multi-decadale scale variability when compared with obs be assessed, if the same quantity (multi-decadale scale variability) cannot be estimated from obs? [Government of Germany]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-401	10	5	36	5	41	Please indicate what metrics are being compared with observations, e.g. is this discussion pertaining to global mean temperature, precipitation, etc.? [Martin Hoerling, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-402	10	5	36	5	52	You have failed to mention the inability of models to accurately predict future ENSO conditions. Figure 7 of McLean et al (2009) showed the link between ENSO and average global lower tropospheric temperatures. [John McLean, Australia]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections including understanding of internal variability
10-403	10	5	36	5	54	This paragraph was a little technical and diffcult to read. Suggest reviewing and revising where possible given that it is part of the executive summary. [Government of Canada]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-404	10	5	36	5	54	This section of the ES is rather long, and should be condensed to focus on the key messages relating to uncertainties coming out of the Chapter 10 assessment. Lines 48 - 50 repeat statements one would expect from Chapter 2, and lines 50 - 54 repeat material coming out of Chapter 9. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-405	10	5	36			Remaining uncertainties- should include contributions from possible errors in signal patterns derived from models, and possible degeneracy between aerosol and greenhouse signals in using regression approaches [John Mitchell, United Kingdom]	Taken into account. This statement has been included in the ES. Note this section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-406	10	5	38	5	38	Why is it 'difficult'? Because the record is too short? [Government of United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-407	10	5	39	5	39	A factor of two does not seem to be huge. How does this compare to the uncertainty in the paleo data? [Albert Klein Tank, Netherlands]	Accepted. Has been revised based on the assessment of it being at least 3 in standard deviation.
10-408	10	5	39	5	39	I presume a factor of 2 in amplitude, not variance? [John Mitchell, United Kingdom]	Accepted. Has been revised based on the assessment of it being at least 3 in standard deviation.
10-409	10	5	39	5	40	"detection of recent … warming to be in error, and …". [J. Graham Cogley, Canada]	Taken into account. Statement revisied.
10-410	10	5	39	5	41	However veriability… simulation.' Too long sentence, HARD to understand. [Daoyi Gong, China]	Taken into account. Statement revisied.
10-411	10	5	39	5	41	I do not understand this sentence. [Helga Nitsche, Germany]	Taken into account. Statement revisied.
10-412	10	5	39	5	41	Sentence seems poorly written. Suggest changing to "However variability would have to be underestimated by approximately a factor of two for detection of atmospheric and ocean warming to be lost. Furthermore, over the past six centuries, the distribution of residual variability in 50-year trends estimated from paleo data is statistically indistinguishable from that in climate model control simulations." I am assuming this is what you mean by 'reasonably close'. [James Renwick, New Zealand]	Taken into account. Statement revisied.
10-413	10	5	39	5	41	However variability…simulation.' Too long sentence, HARD to understand. [Xuemei Shao, China]	Taken into account. Statement revisied.
10-414	10	5	40	5	40	"and in any case" seems misplaced in an ES. [Jochem Marotzke, Germany]	Taken into account. Statement revisied.
10-415	10	5	40	5	40	The use of 'and in any case' could be construed, unfairly as being a prima facae and unsupported value judgement on the authors' part. Is this advisable / rteally necessary? [Peter Thorne, United States of America]	Taken into account. Statement revisied.
10-416	10	5	40	5	41	The uncertainty in estimating the amplitude of internal variability from a residual from palaeodata must huge-differencing unceratin model signals from uncertain palaeo-reconsructions- I feel this is a very weak line of evidence for a summary [John Mitchell, United Kingdom]	Rejected. This is the assessment made in the chapter.

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10-417	10	5	40	5	41	The current wording lacks clarity. Therefore the following wording is suggested: .. and ocean warming to be lost. However, the skill of models to simulate well the multidecadal climate variability is demonstrated by the fact that in climate model control simulations models simulate well the residual variability in 50-year trends over the past six centuries as estimated from paleo data. [Klaus Radunsky, Austria]	Taken into account. The statement has been revised.
10-418	10	5	41	5	42	The prior sentences in this section concerned detection, and the remaining uncertainties in that. Persumably, those were pertaining to global mean conditions. This next sentence introduces the challenges on regional scales, but rather than continuing with the theme of detection, it addresses instead attribution. Please revise so as not to give the unintended impression that detection and attribution are interchangable. Please also revise to clarify whether the uncertainties concerning detection at regional scales are greater than (or less than) those at the global scale. [Martin Hoerling, United States of America]	Taken into account. ES has been revised to be clearer on this point.
10-419	10	5	41			What are “control simulations”? It cannot be constant-forcing simulations because you discounted those above. Non-climate-modellers might consider “control” to be naturally-forced, while climate modellers would consider it to be constant-forcing out of habit. [Dáithí Stone, United States of America]	Taken intoa account. We do not have control in the ES in revised version.
10-420	10	5	43	5	43	"lack of incorporation". [J. Graham Cogley, Canada]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-421	10	5	44	5	45	Internal variability does not "enhance externally forced changes". Internal variability is best understood as being superimposed upon a signal of externally forced change. Please revise. [Martin Hoerling, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections. This sentence has been deleted.
10-422	10	5	45	5	48	Rewrite the sentence that begins: "Observational uncertainties for climate variables….". One might forsee this sentence being taken out of context and used inappropriately to mean that natural varibaility cannot be readily discriminated from externally forced change. The rewrite should be clear to indicate that it is more difficult to discriminate being natural variations and external forced change at the regional relative to the global scale, and also that at the regional scale it is generally more difficult to disctiminate between natural and external forcing for precipitation than for temperature. [Martin Hoerling, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-423	10	5	45	10	45	Please consider adding "land use and land cover changes" to aerosols, as these may become important for regional attribution. [Government of United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-424	10	5	46	5	48	I think this sentence applies to attribution of regional change only, but it is written so generally that it appears to contradict the entire chapter. An example of a non-liftable sentence; taken out of context, it does not say what it was intended to say. [Jochem Marotzke, Germany]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-425	10	5	47	5	48	The following wording is suggested to add clarity: .. Betwen natural internal variability and externally forced changes at regional scales. [Klaus Radunsky, Austria]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-426	10	5	48	5	50	I do not fully understand the conclusion of this sentence. Increased understanding of the observational problems has led to improved bias correction of the observations concerned, whether the corrections are applied directly to the radiosonde data in so-called "homogenization" or determined variationally and applied to the use of satellite radiances in reanalysis. Moreover, despite these differences in bias correction, upper tropospheric temperature trends computed from ERA-40 and the newer ERA-Interim reanalysis are in quite good agreement from 1979 onwards, and in the case of ERA-Interim vary consistently with the variation in upper-tropospheric humidity, which in turn gives a more consistent link between upper tropospheric humidity and surface-temperature variabnbility, as discussed in Chapter 7 in the context of water vapor feedback. I have made numerous comments on Chapter 2, which in my view has not handled this topic well, in part because it did not include reanalysis results in its discussion of upper-air temperature. [Adrian Simmons, United Kingdom]	Noted. But for the assessment we need to use multi-decadal data ie the pre satellite data era, meaning radiosondes. Also this sentence has been deleted in the revised ES.
10-427	10	5	50	5	50	What comparisons are implied by "remain" and "less reliably"? [Jonathan Gregory, United Kingdom]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections and this ambiguity avoided.

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10-428	10	5	50	5	50	"less reliable" if compared to temperature or something else? [Albert Klein Tank, Netherlands]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-429	10	5	50			Less reliably than what? [Dáithí Stone, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-430	10	5	51	5	54	Revise to include a statement about the diffiiculty to detect changes in some extreme events given the short record of observational data. [Martin Hoerling, United States of America]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections which includes issues with observational data.
10-431	10	5	53	5	53	Change "reliably simulate" to "reproduce". [J. Graham Cogley, Canada]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-432	10	5	53	5	53	Why "mean changes". Extremes often are related to anomalies in circulation instead of mean circulation. [Albert Klein Tank, Netherlands]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-433	10	5	54	5	54	What do you mean by "circulation blocking", and how can this happen? [Government of NORWAY]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-434	10	5				Section remaining uncertainties: in line with comment 1, I suggest to include here a couple of sentences on methodological issues. [Laurent Terray, France]	Taken into account. This section has been deleted. Uncertainties are folded into the assessments of individual sections.
10-435	10	6	1	65	2	General comments: The aft-1998 surface temperature stagnation and the more frequent cold winters in northern continents over the past 15 years than the previous decades have to be explained more exclusively in this chapter. These are in a sharp contrast with what were projected by most climatologists a decade ago, and may imply a larger natural decadal to multi-decadal internal climate variability, or a bigger role the sun has played in the earth climate system. If the multi-decadal natural variability or the sun forced change are more important, the current detection and attribution analyses may have been using the simulations and assumptions with too small internal climate variability or natural external forcings, and the confidence with the analysis results may have been overestimated. It would be also unreasonable to assign a similar or even higher confidence to the assessment conclusions in this chapter when the major question puzzling not only scientific community but publics has not been well answered. [Guoyu Ren, China]	Taken into account. There is a separate box on temperature trends over the last 15 years in the revised chapter 9 and the revised assessment takes account of that.
10-436	10	6	1	65	2	General comments: The assessments on attribution of changes in other components than atmospheric variables may have overlaps with the corresponding sections of the WG 2. For example, studies on detection and attribution of discharge (runoff) and glacial changes will be also assessed in subsections related to water resources and Cryosphere in the WG 2. A better coordination with the WG2 authors is needed. [Guoyu Ren, China]	The Reference to WGII in the introduction has been made more explicit as to what WGII considers . Whereas WGI considers attribution of runoff and glacial changes, WGII considers the effects of these changes on ecosystems and other impacts.
10-437	10	6	1	65	2	General comments: Extra publications in other languages than English should be included. The authors of the report from the non-English speaking countries are responsible for reading and summarizing more publications in Chinese, Russian, France, German and Japanese, for example. [Guoyu Ren, China]	Noted. While the assessment is dominated by english speaking journals there are many papers cited from Chinese, Russian, French, German and Japanese scientists.
10-438	10	6	3	6	4	Reword this sentence. "Understanding" is not physical. I suspect you mean "understanding of physical processes", so say that. [John McLean, Australia]	Accepted. Has been reworded to say understanding of physical processes.
10-439	10	6	3	6	18	Your models ignore scientific study of the climate which has found that internal heat exchange is from conduction, convection and latent heat exchange, not, exclusively radiation. Instead of judging the model	Rejected. The evidence grounds for the assessment are laid out in the chapter.

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						results by their ability to predict future climate succesfully you rely entirely on the considered opinions of paid investigators, who apply supposed :statistical figures to their opinions which are no more than guesswork. [Vincent Gray, New Zealand]	
10-440	10	6	3			*assesses the causes of observed changes assessed .. [Tibor Farago, Hungary]	Rejected. It is not clear what point the comment is making given that the comment simply repeats words from the draft, therefore no changes are made.
10-441	10	6	4	10	5	The Hegerl et al. terminology should be self-contained in this volume, especially if it departs from previous IPCC assessments, so that readers do not need to search for this reference for definitions. This could be done by providing definitions of the key terms early in the chapter, or as a brief appendix to the chapter. Some terms are defined later, e.g., p. 10-8, lines 22-28, but should be stated up front. [Government of United States of America]	Taken into account. The text is revised referring to the GPGP and detection and attribution are defined at the start of 10.2.
10-442	10	6	5	6	8	Add the sentence "The ability to evaluate changes due to natural forces is often constrained by the low level of scientific understanding." (refer table 2.11 page 201 IPCC 4AR) [John McLean, Australia]	Rejected. Discussion of levels of confidence in attribution statements is contained throughout the chapter and there is no jusitifcation provided by the reviewer for any special treatment of natural forces versus anthropogenic forcings and therefore this change is rejected.
10-443	10	6	8	6	8	"both human and natural" (presumably the noun being qualified is "drivers", not "change"). [J. Graham Cogley, Canada]	Taken into account.Text edited.
10-444	10	6	8			see above concerning the terms of “detection and attribution” .. [Tibor Farago, Hungary]	Taken into account.Text has been revised.
10-445	10	6	10	6	10	expression 'upper atmosphere': is this right? Or is it 'free atmosphere'?=the atmopshere from the surface to the tropopause [Helga Nitsche, Germany]	Rejected. The chapter considers changes above the tropopause, in the stratosphere, and so this is a suitable word to use here.
10-446	10	6	12	6	14	The components related to ocean properties and the cryosphere could be put in parentheses to help clarify the sentence. [Chris Forest, United States of America]	Accepted.
10-447	10	6	21	6	22	expression 'climate indices': this seems to be not exact: there are indices for (precipitation) change, for temperature extremes, etc. see for instance the list of climate extremes in www.ecad eu [Helga Nitsche, Germany]	Taken into account. "climate variables" is the term used elsewhere in the chapter and avoids confusion with specific usage of the term "indices".
10-448	10	6	23	6	24	We feel this sentence about impacts should come after the next paragraph about regional perspectives – because the vast majority of impacts are regional. [Government of United States of America]	Rejected. It is helpful to state at the start what the chapter does not cover because WGII covers it.
10-449	10	6	24	6	24	"Working Group II, in their chapter 18 in particular." [J. Graham Cogley, Canada]	Accepted. Revision made to include reference to chapter 18
10-450	10	6	26	6	34	It would be useful if this paragraph could point out that the signa-to-noise ratio for the forced response is expected to be lower at regional scales. Also, when stating that models need to be assessed regionally, it should be recognized that this is not sufficient in the sense that regional change is a function of more than just the response to forcing that takes place in the region of interest. For example, it be a that model that, on the face of it, is a bit less skillful in a given region may produce more skillful projections because it does a better job of simulating large scale teleconnected processes. [Francis Zwiers, Canada]	Taken into account. However this text has been deleted to shorten chapter as these issues are discussed in section 10.3.
10-451	10	6	29	6	31	I don't think this is always an "additional challenge" because observational coverage for some regions (US and Europe) is much better than for the globe. [Albert Klein Tank, Netherlands]	Noted. Text revised and does not include phrase additional challenge.
10-452	10	6	29	6	31	Revise sentence because "going back in time" is very unprofessional. I suspect that you mean to say "The availability of observational climate data decreases as we step further into the past" (Hmm, I see your problem!) [John McLean, Australia]	Noted. Text has been simplified.
10-453	10	6	30	6	30	When you say 'greater problem' you mean they are larger. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Text has been simplified.

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10-454	10	6	32			*instead “and local forcings” – “and regional forcings” .. [Tibor Farago, Hungary]	Change rejected to make the distinction between local and non-local but text has been revised here in response to reviewers' comments to aid clairty.
10-455	10	6	33	6	34	The sentence on Extremes appears to be out of place here. [Chris Forest, United States of America]	Noted. Text has been simplified.
10-456	10	6	36	6	36	Delete "progressively". [J. Graham Cogley, Canada]	Noted. Text has been simplified..
10-457	10	6	36	6	42	Humans influence the climate evry time they put up a building or change the lamdscape. There is, however, no evidence that any change in the climate is influenced by emissions of so-called:'Greenhouse Gases' [Vincent Gray, New Zealand]	Rejected. The evidence is contained within the 4 previous IPCC reports.
10-458	10	6	36	6	51	This historical material should be in Ch01. [Jochem Marotzke, Germany]	Rejected. It is important to place the results of AR5 on attribution into the context of previous assessments just as AR4 did in the introduction of the corresponding chapter 9. Note however that the discussion has been shortened.
10-459	10	6	38	6	40	You must be aware that this statement from 2AR was false. It was a very late conclusion was inserted after the review of SOD and was based on an unpublished paper written by several authors of the 2AR "attribution" chapter (refer section 8.2.1 + endnotes, Bolin, "A History of the Science and Politics of Climate Change", Cambridge Press, 2007), what's more the paper was dismissed by the scientific community when it was finally published. [John McLean, Australia]	Rejected. The Santer et al conclusion has been supported by subsequent studies and neither the assessment of the SAR nor the Santer et al study has been dismissed by the scientific community.
10-460	10	6	39	6	39	"Detection" and "Attribution" are mechanisms of organised guesswork They provide only speculation, not evidence of cause and effect, however much "confidence": is expressed in them. [Vincent Gray, New Zealand]	Rejected. The basis for detection and attribution is set out in the chapter where a number of different methodologies are described and from which the results stem, noting that multiple different methodologies provide consistent conclusions.
10-461	10	6	40	6	42	Factually you are correct, that's what the report said. The problem lies with the absence of credible evidence on which that "finding" was made". [John McLean, Australia]	Rejected. The evidence is set out in Chapter 12 of the Third Assessment Report
10-462	10	6	44	6	44	Insert "further" at the end of this line ("conclusions were" further "strengthened"). [Francis Zwiers, Canada]	Change made.
10-463	10	6	45	6	51	Again factually correct, that's what the report says, but that "finding" was based on the output of climate models that (a) could not possibly be correct because of the low levels of scientific understanding shown in table 2.11 (which focussed only on a small subset of all forces) and (b) chapter 8 that described some of the many flaws in climate models. To subsequently treat the output of climate models as accurate lacks professional integrity. [John McLean, Australia]	Rejected. Chapter 9 of AR4 did not assume that models could not have systematic errors.
10-464	10	6	47	6	51	Based entirely on the personal opinions of your paid investigators [Vincent Gray, New Zealand]	Rejected. These conclusions were not based on opinions but on the result of an assessment process.
10-465	10	6	51	6	51	Is there a reason why "wind patterns" are not mentioned in the ES on page 10-3. No longer supporting evidence? [Albert Klein Tank, Netherlands]	Rejected. The effect on wind patterns is via changes in SLP which evidence is provided in the ES which is also the way it was discussed in the AR4 Chapter 9 ES.
10-466	10	6	53	6	53	uncertainties- unresolved issues? [John Mitchell, United Kingdom]	Change accepted.
10-467	10	6	56	6	56	Is is really "hot" nights, not "warm" nights? [Jochem Marotzke, Germany]	Accepted. It is warm nights and this has been corrected.
10-468	10	7	3			The term "anthropogenic fingerprint" has not been defined yet. [Chris Forest, United States of America]	Text edited to remove reference to fingerprint until the term has been defined in section 10.2
10-469	10	7	13			overkill. Put most of the material in an appendix or reduce to 1-2 pages [tim barnett, United States of America]	This comment is assumed to refer to Section 10.2. Section 10.2 is an important section as supported by

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							many reviewers but it has been shortened somewhat to make it more succinct.
10-470	10	7	15	7	29	Please append the following sentence to this paragraph. "We also turn a blind eye to any research that challenges our claims about CO2-driven warming being significant." You may find this statement provocative but the IPCC Procedures document states in sections 4.3.3 and 4.3.5 that this report must include different "(possibly controversial)" views. I see no evidence at all that this chapter mentions those views, which means that my sentence at the start of this comment is correct. [John McLean, Australia]	Reject as this statement is not true.
10-471	10	7	22	7	22	missing word: … and thus are able to…. [Helga Nitsche, Germany]	Text has been revised to make it clearer.
10-472	10	7	26	7	28	Single forcing experiments also conducted for a number of CMIP5 models (Tier 1), important for process based understanding. Could cite Taylor et al., (2011) here. [Oliver David Andrews, United Kingdom]	Reference to Taylor added and revision made to make more explicit.
10-473	10	7	27			Most? [Dáithí Stone, United States of America]	Word deleted.
10-474	10	7	31	13	6	I am not sure how you are in terms of page limits, but while technically fascinating I don't see how the section in its current form supports the chapter. It is quite technical and, with the exception of 10.2.5, doesn't link to the rest of the chapter. [Dáithí Stone, United States of America]	Rejected. The section lays out the basis for detection and attribution including the box 10.1
10-475	10	7	31			Section 10.2. Could this methods section also include an overview of the different approaches used for detection and attribution of extremes? E.g. applying extreme value theory (Zwiers et al., 2011). There has been progress and debate on this since AR4 (and SREX) and attribution of extreme events is a topic of particular interest to policymakers, so further details of techniques could be helpful. [Oliver David Andrews, United Kingdom]	Given length constraints, this has been left to the relevant subsections. Note other RE comments that 10.2 was too long.
10-476	10	7	31			Section 10.2. Could this methods section also include an overview of the different approaches used for detection and attribution of extremes? E.g. applying extreme value theory (Zwiers et al., 2011). There has been progress and debate on this since AR4 and attribution of extreme events is a topic of particular interest to policymakers, so further details of techniques could be helpful. [European Union]	See 10-475
10-477	10	7	31			This methodological section does not have a clear enough through-line. The definition of D&A comes only after a full page (p. 8, l 22-34), which is much too late if a definition is given at all. Also, p. 8 l. 30 includes attribution to antecedent events, whereas p. 7 l. 40 includes only response to external drivers. This "double definition" is confusing and appears inconsistent. [Jochem Marotzke, Germany]	Accepted. We have moved up the definitions
10-478	10	7	31			You describe but don't “evaluate” in this section. [Dáithí Stone, United States of America]	Accepted. Evaluation is left to the results sections
10-479	10	7	36			Section 10.2.1: There is growing acceptance of alternative ways of regarding attribution, as outlined by Trenberth (2012) as “All weather events are affected by climate change because the environment in which they occur is warmer and moister than it used to be.” Trenberth, K. E., 2012: Framing the way to relate climate extremes to climate change. Climatic Change, 115, Issue 2, 283-290, doi: 10.1007/s10584-012-0441-5. The original publication is available at: http://link.springer.com/article/10.1007%2Fs10584-012-0441-5. It is recognized that this is the antithesis for the authors of this chapter who have a vested interest in doing things the old way. But it is actually important to recognize that it does not make sense to even try to do attribution for many things because every weather event is unique and there is no perfect model. For events, weather and chaotic aspects will always dominate and the problem is not well posed. There is always an incomplete description of events and the inability to simulate it accurately means it cannot be taken apart. Yet it cannot not be affected. So the question is not one of attribution but one of saying how large is the affect? The present attribution approach underestimates the effects, always. I believe this alternative view should be discussed and adopted in section 10.2.1. [Kevin Trenberth, United States of America]	Taken into account. Our assessment is that this re-framing proposal is still very much a minority opinion, but we have revisited the language on this.

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10-480	10	7	39	7	39	"involve". [J. Graham Cogley, Canada]	Accepted
10-481	10	7	39	7	44	Again, historical material that should be in Ch01. [Jochem Marotzke, Germany]	Taken into account. Link made to Ch01
10-482	10	7	41	7	41	To clarify what you mean by "not the only" reference can be made to Section 1.2.3 in Chapter 1. [Albert Klein Tank, Netherlands]	Taken into account. Link made to Ch01
10-483	10	7	46	7	48	It is not mentioned here how the observational errors enter into the detection and attribution context. This should be mentioned somewhere in this section. [Chris Forest, United States of America]	Accepted
10-484	10	7	46			The core elements as you describe them here apply to the analysis of changes in the physical climate system but do not cover detection and attribution in general. [Dáithí Stone, United States of America]	Clarified, with reference to the GPGP
10-485	10	7	47	10	47	Please change "climate indicators" to "climate system variables." Otherwise, please define "climate indicators". [Government of United States of America]	Accepted
10-486	10	7	53	7	54	Can you prove that all climate forces are accurately and completely embodied in the climate models used in this report? If the answer is "no", and it's difficult to see how it can be otherwise, then this item 3 is fundamentally flawed and therefore the entire sequence of "attribution" fatally compromised and should be deleted. [John McLean, Australia]	It is not possible to "prove" the absence of some unknown confounding factor, but this issue is common to all attribution situations. Text has been clarified.
10-487	10	7	53			Must it be quantitative? And physical? I believe you are also dealing with chemistry in this chapter. [Dáithí Stone, United States of America]	Noted. Yes, and physical sciences includes chemistry.
10-488	10	7	53			How else than with a model? If it must be quantitative then it is definitely a model, and I would still consider it a model even if it were qualitative. [Dáithí Stone, United States of America]	Accepted. Text clarified.
10-489	10	7	54	7	54	Suggest replacing "these" with "the observed" to avoid using "these" twice, and to emphasize that D&A is about understanding observed changes. [Francis Zwiers, Canada]	Accepted
10-490	10	7	56	7	56	Please delete "in these indicators." [Government of United States of America]	See 10-489
10-491	10	8	2	8	2	We think that "is about testing" is perhaps too colloquial. We recommend rephrasing. Perhaps "involves". [Government of United States of America]	Accepted
10-492	10	8	2	8	2	Sentence sounds very defensive. [Jochem Marotzke, Germany]	Sentence has been deleted
10-493	10	8	2	8	2	correlations are tested too. [Andreas Walter, Germany]	Yes, but not all are physically based
10-494	10	8	4	8	4	… system ?is generally (or basically)? chaotic… [Helga Nitsche, Germany]	Chaotic or random --clarified
10-495	10	8	4	8	5	Perhaps "generating unpredictable variability on all time scales" could be reworded a little, to acknowledge that some components of variability are predictable on some time scales. Otherwise we would not have routine weather prediction and sub-seasonal to seasonal prediction, and decadal prediction (Chapter 11) would have less to offer. [Adrian Simmons, United Kingdom]	Noted. Good point.
10-496	10	8	4	8	7	This sentence exaggerates "chaotic variability". Climate and weather forces operate at a range of time scales from a few seconds to epochs. Chaotic variability can therefore be regarded as the net impact at a given time of a host of variable forces acting across different timescales. Low levels of scientific understanding currently impede our interpretation of short term variation but doesn't mean that if our knowledge improves we won't understand them better. [John McLean, Australia]	Noted. Section has been heavily edited.
10-497	10	8	4	8	12	This discussion is useful - particularly the explanation of why detection and attribution statements can never be at 100% confidence - this would be a useful discussion to include in the executive summary and even the SPM. [Government of Australia]	Accepted
10-498	10	8	4	8	12	Relying on a single citation to work in 1976 may not be not sufficient. Theoretical understanding of nonlinear dynamical systems could be referenced here to explain the expectation of some level of inherent unpredictability. A suggestion is to reference Nature: Climate Change: Perspective, 26 October, 2012 doi:10.1038/NCLIMATE1562, Deser et al. "Communication of the role of natural variability in future North	Noted. Good suggestions, but section has been largely deleted to save space.

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						American climate", a recent paper that provides some perspective on climate predictability related to natural climate variability. Other helpful references would be to Michael Ghil on nonlinear aspects of climate variability and Eric Simonnet, Henk A. Dijkstra, Michael Ghil on Bifurcation analysis of ocean, atmosphere and climate models." [Government of Canada]	
10-499	10	8	4	8	28	I have som difficulities to follow the line of this passage,. [Helga Nitsche, Germany]	Rejected. Others found it useful.
10-500	10	8	5	8	5	adapt writing of HASSELMANN, 1976 to standard style [European Union]	Accepted
10-501	10	8	5	8	5	Why is the reference HASSELMANN, 1976 in capital letters? [Roman Zweifel, Switzerland]	Taken into account. Because he is a very important chap (and we made a typo)
10-502	10	8	5			*Hasselmann, [Tibor Farago, Hungary]	Accepted
10-503	10	8	7	8	7	Please avoiding starting sentences with conjunctions. It is poor grammar and can easily lead to false perceptions because people don't read the reservations expressed in the second sentence until they have finished mentally processing the first sentence. [John McLean, Australia]	Rejected. We think it is clearest to use short sentences.
10-504	10	8	7	8	8	Despite your statement, it is clear that the IPCC is very likely falsely attributing trends because it fails to recognise that a sustained variation in natural forces will cause a trend. A shift in the ENSO state from one of domination of the La Nina side of absolutely neutral (ie. > 0 but not necessarily crossing the arbitrary threshold) to one where El Nino conditions dominated will cause a rising temperature trend (and vice versa cause a cooling trend). Such a sustained shift began in mid 1976, hence the temperature trend since that date can be attributed to the ENSO. Corrections to your text are required. [John McLean, Australia]	Rejected. Again, this is a hypothetical confounding factor, since the observations are equally consistent with stationary ENSO variability superimposed on a trend.
10-505	10	8	8	8	11	Delete "Hence, ". The sentence does not follow from the previous sentence. "of a cold year". [J. Graham Cogley, Canada]	Accepted
10-506	10	8	10			*term warming trend to an external forcing. [Tibor Farago, Hungary]	Accepted
10-507	10	8	11	8	12	Here, and several other places in the chapter, the term "confidence level" is used when it would be more correct to say "significance level". Both terms could be confused with the IPCC calibrated uncertainty language, and thus my suggestion, in this context, would be to avoid either term in this sentence, and simply rephrase it as "Detection and attribution statements are based on statistical inferences, and therefore can never be made with absolute certainty". [Francis Zwiers, Canada]	Accepted
10-508	10	8	12	8	12	Isn't this true for all scientific statements? [Jochem Marotzke, Germany]	Noted. Some are unequivocal -- not D&A
10-509	10	8	12			Is this IPCC confidence? [Dáithí Stone, United States of America]	See 10-507
10-510	10	8	14	8	15	These are listed in the wrong order. There is no doubt that natural forces operate on climate and sometimes they act strongly. There is significant doubt about the influence of anthorpogenic forces, the claim being based on the output of climate models that are known to be flawed. [John McLean, Australia]	Noted. There is no statement here about their relative importance, which will depend on the problem.
10-511	10	8	15	8	16	Wind should be mentioned. Wind distributes heat and the distribution of heat (cf. its concentration in one place) means a reduced rate of radiative heat loss. [John McLean, Australia]	Rejected: wind is not an external forcing.
10-512	10	8	15	8	16	Suggest inserting "The responses to" at the beginning of the sentence, and deleting "therefore" on line 16. Clarification is needed because the sentence makes a comparison between two types of naturally induced climate variability, one of which is the RESULT of natural extrenal forcing, rather than the forcing itself. [Francis Zwiers, Canada]	Accepted
10-513	10	8	16	8	16	"therefore" does not fit - previous sentence is about anthropogenic vs. natural forcing [Jochem Marotzke, Germany]	Accepted
10-514	10	8	16	8	16	, are therefore distinct… why 'therefore'? [Helga Nitsche, Germany]	See previous
10-515	10	8	17	8	17	Contradicts page 9.47 (lines 3-5) [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	Taken into account bia coordination with Ch09
10-516	10	8	17	8	18	You seem to think that a distinct line divides external and internal variability. Winds and ENSO are two	Rejected: ENSO is an aspect of internally-generated

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						examples of forces that are falsely considered to be internal variability. As noted above, winds can impact radiative emission into deep space, which is clear not internal. The ENSO doesn't just vary the distribution of warm air but has an impact on cloud cover, and a change in cloud cover means a change in insolation (from an external force) reaching the Earth's surface. [John McLean, Australia]	climate variability.
10-517	10	8	18	8	18	..statistics of this noise - or ?its? noise? [Helga Nitsche, Germany]	See 10-518
10-518	10	8	18	8	18	For clarity, change to "...ability to capture the statistics of this variability (often referred to as “noise”)." [James Renwick, New Zealand]	Noted. Good suggestions
10-519	10	8	18	8	20	This sentence attempts to argue that an inability to forecast the short term evolution of the climate system (treating it as an initial value problem) does not preclude the ability to estimate the expected response to external forcing of the climate system (a boundary values problem), but it does not do so very convincingly. I think a few more lines to make the argument more completely, perhaps linking to Chapter 11, would be useful. As it is, these few lines remind the reader of a reason for doubt, but they do not really seem to deal with the concern convincingly. [Francis Zwiers, Canada]	Taken into account. Clarified.
10-520	10	8	25	8	26	"An identified change is detected in observations if its likelihood of occurrence by chance due to internal variability alone is determined to be small." This sentence makes no sense and if it is trying to say what I think it is, it's a false statement. A sustained shift in ENSO conditions will cause a temperature trend. The reasoning is simple, a sustained period of El Nino-driven warming following a period of sustained La Nina-driven cooling will cause a period of higher temperatures and hence a trend. This occurred in the last 20 years of the 20th century. [John McLean, Australia]	See 10-516
10-521	10	8	33	8	34	You should also include other solar forces and the substantial body of research that claims a link between climate the shifting solar barycentre and the influences of the other major planets. (It's true that the planets exert only small forces, but in the absence of larger forces small forces may be all that's required.) Don't forget luni-solar influences either. [John McLean, Australia]	rejected. Attribution requires physically-based understanding.
10-522	10	8	36	8	36	Avoid circularity by saying "a change that may be a response to an external driver … if the contribution of the driver can be detected after allowing ...". [J. Graham Cogley, Canada]	Accepted
10-523	10	8	36	8	37	The sentence appears to announce that here, a different approach will be taken to previous assessments. Moreover, the choice of "detected" at the end of the line appears unfortunate, given the crucial distinction between detection and attribution. [Jochem Marotzke, Germany]	Taken into account. We have clarified that the GPGP represented a small generalisation of previous practice to cover a wider range of cases.
10-524	10	8	36	8	38	A key requirement here is "allowing for uncertainty in potential confounding factors" - perhaps that should be expanded a bit more. It would also be useful to cite discussion in Mitchell et al (2001 - TAR, Ch 12) and Hegerl et al (2007 - AR4, Ch 9). [Francis Zwiers, Canada]	Accepted, within length constraints
10-525	10	8	38	8	40	Unclear to what extent relaxing the criterium and changing the D/A method has contributed to strengthening of D/A evidence as noted on page 3, line 3 [Albert Klein Tank, Netherlands]	Accepted. It has not -- we have clarified
10-526	10	8	38	8	45	The language used here ("new flexibility", "new guidance allows") makes it sound like some oversight body has relaxed the rules to allow people doing D&A to push the limits of inference, which is not the case; it is clear that one would need to be very high confidence in process understanding to make an additional inference about an associated variable on the basis of a given attribution result. The words "flexibility" and "allows" both miscommunicate that requirement. I suggest using a phrase such as "new guidance recognizes that it may be possible, in some instances to attribute a change in an associated variabile before it can be detected, provided that there is a strong body of knowledge that links change in that variable to the change that has been attributed", or something to that effect. [Francis Zwiers, Canada]	Noted. Good suggestions, paragraph has been extensively rewritten.
10-527	10	8	42	8	44	Since "risk" is rigorously defined and used elsewhere, should it be used loosely here? What is meant by "risk" in this attribution sense differs from "risk" as defined in other communities, e.g., impacts and adaptation. [Government of United States of America]	Accepted. Replacted with probability of occurrence

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10-528	10	8	42			*large-scale temperature changes (?) [Tibor Farago, Hungary]	Accepted
10-529	10	8	43	8	43	The Detection and Attribution chapter 18 of WGII has recently adopted a definition that makes attribution permissible only after detection, which, depending on how "closely associated variable" is understood, could be inconsistent with what is said here. [J. Graham Cogley, Canada]	Rejected. We are adhering to the cross-working-group GPGP: this seems to be an issue for WGII
10-530	10	8	49	8	54	This sentence ignores the question of whether climate models are accurate. IPCC 4AR showed that they cannot be (table 2.11) and in fact are not (chapter 8). Attribution studies based on flawed climate models have no credibility whatsoever, neither do predictions of future temperature. Until you can prove that climate models are 100% complete and 100% accurate any claims based on the output of climate models should be viewed with scepticism. Revise the sentence accordingly. [John McLean, Australia]	Rejected. Models do not have to be perfect to provide a useful basis for inference.
10-531	10	8	49			*instead „observable quantities": observable variables [Tibor Farago, Hungary]	Accepted
10-532	10	8	50	8	50	We feel that "test" or "evaluate" would be better than "frame" here. [Government of United States of America]	Accepted
10-533	10	8	53	8	53	Change "they do not need" to "it is not possible for them". [J. Graham Cogley, Canada]	Accepted
10-534	10	8	54	8	54	What is meant by a "physically coherent" model, and what is "an incoherent model" (other than perhaps the human sort)? This is a terminology that we don't believe will be familiar to most readers. Does it mean physically consistent or inconsistent? How does this apply to a simple set of statistical assumptions, which could easily be inconsistent with known physics? [Government of United States of America]	Physically consistent. We meant a model that does not, for example, violate energy conservation (as a purely empirical statistical model might)
10-535	10	8	54	8	54	meaningless, however statistically significant.: instead of 'however' 'though possibly'? [Helga Nitsche, Germany]	Sentence has been deleted
10-536	10	8	54	8	54	Define what is meant by "coherent"? [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	See 10-534
10-537	10	8	54	8	54	Make a statement about was was done with results from such models: Results based on an incoherent model are meaningless, however statistically significant [Roman Zweifel, Switzerland]	Accepted
10-538	10	8				an author's name should not be capitalized. [Government of France]	Noted. Taken into account.
10-539	10	9	1	9	3	At the start of this sentence insert the word "If climate models simulated all natural climate forces with 100% accuracy,". This qualifier is vital to your statement and needs to be made explicit. [John McLean, Australia]	See 10-530
10-540	10	9	3	9	3	Fortuitous' - ist this the proper word for this effect? [Helga Nitsche, Germany]	Taken into account. Word deleted.
10-541	10	9	4	9	4	..attribution studies are designed to ensure…: or: 'must be' designed? [Helga Nitsche, Germany]	Accepted
10-542	10	9	7	9	8	Suggest replacing "This cancellation of errors did not" with "This possibility does not". As written, you seem accept that there were errors, while on line 5, you note only that there was a "possible cancellation of errors". Is there another way to describe this uncertainty other than just calling it a "cancellation of errors", particularly given that climate sensitivity is the result of a combination of parameter and structural choices in building a model more complex than an EBM or an EMIC, and that the sulphate aersol forcing that is similarly the result of a collection of such choices. [Francis Zwiers, Canada]	Taken into account through revisions, although the point that models always involve cancellation of errors is valid
10-543	10	9	7	9	11	Sentence very awkward. Would it still be correct to write "...surface warming, because these conclusions were based on estimating from observations the responses to greenhouse and sulphate forcing separately, rather than assuming the model-simulating responses were correct (Hegerl et al. 2011)." Note that it would be "surface warming" or "surface temperature rise". [Jochem Marotzke, Germany]	Taken into account. Wording clarified.
10-544	10	9	8			*instead „temperature warming": temperature increase [Tibor Farago, Hungary]	Taken into account in revision.
10-545	10	9	13	9	16	Somewhere in the chapter the near degeneracy of sulphate and greenhouse gas signals should be discussed eg how it is accounted for, and its contribution ot uncertainty [John Mitchell, United Kingdom]	Accepted: a paragraph on degeneracy has been inserted
10-546	10	9	13	9	20	This 8-line sentence is too long and very unclear. [John McLean, Australia]	Taken into account in revision.
10-547	10	9	13	9	26	again I find it difficult to follow the line of this passage [Helga Nitsche, Germany]	Taken into account in revision.

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10-548	10	9	17	9	17	The scaling factor should be unity for a perfect simulation. Is 1 significantly larger than zero? Surely zero here should be unity? Scaling down will come if the scaling factor is 1.5-2 or higher, and scaling up if it is down at 0.5. Also the error of the scaling factor shouldn't include zero. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Unclear comment: We have tried to clarify this text anyway.
10-549	10	9	20	9	22	It is nonsense to suggest that flaws in models don't matter. In truth they mean that no reliance can be placed on the output of those models. [John McLean, Australia]	See 10-530
10-550	10	9	21	9	21	It seems a qualifier should be provided on "it does not matter", as for some purposes it may well; perhaps "for present purposes"? [Government of United States of America]	Noted. Good suggestion.
10-551	10	9	22		26	This sentence acknowledges the potential for an inaccurate "fingerprint" to distort results and states that this is addressed by using multiple models. If some physical process is omitted from all the models this will not help. Specifically, if heating by solar UV is not included in the model runs producing the solar fingerprint then it is less likely that any signal due to UV variations will be identified in the observational data. There should be a statement somewhere in the chapter acknowledging this problem OR stating that the models which produced the fingerprint pattern had good resolution (spatial and spectral) in the stratosphere. [Joanna Haigh, United Kingdom]	Noted. Good suggestion. We now also make the point that large discrepancies in magnitude might also flag issues with missing processes.
10-552	10	9	31	9	32	Even for large-scale temperature change, this assumption is debatable. For example, it fails for the AMO and several CMIP5 models (see Terray 2012, Evidence for multiple drivers of North Atlantic multi-decadal climate variability. Geophys. Res. Lett, 39, L197-12, doi:10.1029/2012 GL053046. [Laurent Terray, France]	Taken into account. Clarified with reference to the AMO literature
10-553	10	9	34	9	35	We suggest "have not yet found wide application" rather than "not yet found to be needed" to improve this sentence. [Government of United States of America]	Accepted
10-554	10	9	34	9	35	"found to be needed" or simply not considered in the available literature? [Albert Klein Tank, Netherlands]	See previous
10-555	10	9	34	9	35	Delete "such as neural networks, but these have not yet been found to be needsed for attrribution studies". - In fact, all methods, usual climate models and statistics, have their advantages and shortcomings. So, all these methods are needed (similar to regression, econometrics, Granger statistics etc, all mentioned in this report). Note that neural networks are sucessfully used in physics and many other fiields of science, why not in climaatology? Based on radiative forcing data and in comparison with usual climate models they may contribute to non-linear and multiple attribution studies quantifying the signals under consideration (including, of course, a variety of tests). [Christian-D. Schoenwiese, Germany]	See previous
10-556	10	9	34	9	35	Isn't this circular? [Dáithí Stone, United States of America]	Accepted. Wording has been clarified.
10-557	10	9	34	9	35	I would say this differently, e.g., "In principle, additivity is not required to undertake detection and attribution studies, but to date, nonadditive approaches have not been required." I would avoid invoking a specific method, such as neural networks (which are just classes of non-linear regression models, with lots of free parameters), without some demonstration that it has been successfully applied. [Francis Zwiers, Canada]	Accepted
10-558	10	9	34			*„Additivity is ..” – this sentence is needless here. [Tibor Farago, Hungary]	Rejected. We think it is important to emphasise that additivity is not an absolute requirement, but a significant simplification
10-559	10	9	35	9	35	needed' or ? used ? [Helga Nitsche, Germany]	Taken into account.Text clarified
10-560	10	9	37			*in these detection and attribution assessments. [Tibor Farago, Hungary]	Taken into account. Text clarified
10-561	10	9	40	9	40	Delete "used". [J. Graham Cogley, Canada]	Accepted
10-562	10	9	42	10	47	Box 10.1, the graphics and the captions present all the information necessary to interpret the observed annual global mean temperature from 1861 to 2010. The one thing that should be included is a reference to the data depicted, i.e. does the observed annual global mean temperature for 1861 to 2010 have a reference source, if so, it should be included. [Government of United States of America]	Accepted
10-563	10	9	42	10	47	Box 10.1 is nice from a statistical point of view, for those familiar with the underlying techniques. For the lay	Taken into account. The point is to explain where the

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						reader, I think it would be quite confusing. Panel (a) is nice, but panels (b) and (c) are trickier to understand. Instead of (b) I suggest you just state that the best combination of natural and anthropogenic forcings that fit the observed record are to take 76% of the natural forcing and add it to the anthropogenic forcing. Any other simplifications or clarity improvements would be welcome. [James Renwick, New Zealand]	76% comes from:have worked on clarifying the box
10-564	10	9	42	10	47	I know what you are trying to do here, but I think the main message that is coming across is: “This is very complicated. And we've even simplified things in this example. So trust us, we're scientists.” Is this what you want? [Dáithí Stone, United States of America]	Taken into account. Have worked on clarifying the box
10-565	10	9	44	10	47	Box 10.1 The box is not clear. We feel that this topic is critically important. We recommend that panel b be deleted. We also recommend that the basic regression model be presented in the text. [Government of United States of America]	Rejected. We don't see that replacing panel b with an equation will help: the point of the box is to explain D&A to people unfamiliar with equations. We have included an intervening panel to help the transition from panel a to the new panel c.
10-566	10	9	46	10	35	1a shows dots changing colors, but never explains the changes (it's apparent that this reflects an increasing anthropogenic contribution, but should be stated). The text refers to a thick black line at the base of the box in Fig 1b, but is obscured by the figure itself. [Government of United States of America]	Taken into account. Good point: the colours are proportional to observed temperatures. Box has been revised.
10-567	10	9	48	9	48	Replace "temperatures" with "temperature anomalies". [Francis Zwiers, Canada]	Accepted
10-568	10	9	54	9	55	Add the words "assuming that these models are accurate" to this sentence. [John McLean, Australia]	Rejected. No, this assumption is not made -- they are just plotted against each other
10-569	10	9	54	10	7	Unfortunately, I don't find the presentation in panel (b) particularly intuitive (I find that 3-d scatter plots tend to work only when the plume is animated (rotated on the screen), making it easier to perceive depth). Also, the figure seems to show the OLS fit rather than a TLS fit (shouldn't the lines that connect the plane to the observations be perpendicular to the plane (after "optimization"))? [Francis Zwiers, Canada]	Taken int account. The lines are just there to help indicate the height of the dots. We have worked on the visualisation, and have produced an animated version in the Electronic Supplementary Material.
10-570	10	10	10	10	10	Please, also cite Esper et al. (2012) here. Full reference: Esper, J., D.C. Frank, M. Timonen, E. Zorita, R.J.S. Wilson, J. Luterbacher, S. Holzkämper, N. Fischer, S. Wagner, D. Nievergelt, A. Verstege, and U. Büntgen (2012) Orbital forcing of tree-ring data. Nat. Clim. Change, doi:10.1038/NCLIMATE1589. [Fredrik Ljungqvist, Sweden]	Taken into account. Have mentioned this as an alternate source of estimated variability
10-571	10	10	12	10	12	In the data this long-term cooling continues to about 1900 CE. Actually, the 19th century is among the coldest centuries in the Arctic according to proxy data. [Fredrik Ljungqvist, Sweden]	Rejected. Comment is unclear
10-572	10	10	18	10	18	Insert "significance" after "5%" (i.e., "5% significance level"). [Francis Zwiers, Canada]	Accepted
10-573	10	10	18	10	19	Delete "and encloses the (1,1) point". This is not required for detection, which is the subject of the sentence. [Francis Zwiers, Canada]	Taken into account. Have clarified.
10-574	10	10	20	10	20	I suggest replacing "significantly" with "substantially", so that there is no confusion as to whether the change in scatter is statistically significant. [Francis Zwiers, Canada]	Accepted
10-575	10	10	21	10	23	It is very important here to consider the fact that most temperature reconstructions underestimate the amplitude of the trend and low-frequency variability of past temperature changes. It ought to be acknowledged and discussed. 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 Ljungqvist, Sweden]	Rejected. We are using model-simulated variability, not proxy reconstructions: and spectral analysis provides no clear evidence of a systematic underestimate of low-frequency variability in current models.
10-576	10	10	23	10	31	The likelihood for the attribution of Arctic ice retreat to human activities appears much weaker in the executive summary compared to the corresponding text in the chapter. This may be because the full 1950-2010 period is covered by the statement. If so, please focus the statement on the facts and time period that are best known (possibly the trend since 1990) as this is the information that is most useful to build climate policy. [European Union]	Rejected. This is a heuristic example: and many statements address attributable trends over 1950-2010

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10-577	10	10	23	10	31	The likelihood for the attribution of Arctic ice retreat to human activities appears much weaker in the executive summary compared to the corresponding text in the chapter. This may be because the full 1950-2010 period is covered by the statement. If so, please focus the statement on the facts and time period that are best known (possibly the trend since 1990) as this is the information that is most useful to build climate policy. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	See previous
10-578	10	10	31	10	31	Insert "that" before "there are only two ….". [Francis Zwiers, Canada]	Accepted
10-579	10	10	32	10	32	Replace "anthropogenic or natural" with "anthropogenic and natural". [Francis Zwiers, Canada]	Accepted
10-580	10	10	38	10	38	Please, change “many” to “most”. [Fredrik Ljungqvist, Sweden]	Rejected. Ljungqvists comments appear to be referring to a different line-numbering
10-581	10	10	38	10	38	I don't think "schematic" really describes the figure well. What about "Example of a simplified detection and attribution study"? The figure describes a real result (I assume) that the reader could try to reproduce, so it is more than just a schematic (which suggests a flow chart to me ...). [Francis Zwiers, Canada]	Taken into account. Replaced with "simple example"
10-582	10	10	38	10	45	Well done for using HadCRUT4 in the text and some of the plots. You talk about taking obs uncertainty into account, but you could in this Figure show the obs uncertainty from the 100 realizations for HadCRUT4. This could also be shown in Figs 10.1, 10.3, 10.5 and 10.6. Probably difficult to do for all, but an attempt on some of them would be useful. I realize you meant using obs uncertainty within formal D&A procedures, but showing it on some plots would also be useful. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	We have used HadCRUT4. Observational uncertainty is discussed in 10.3 and results using HadCRUT4 observatioal uncertainty (Gillett et al) are assessed.
10-583	10	10	41	10	42	The sentence is unclear here. Is not the 11th century around the turn of the millennium? If I understand it correctly, what is meant is that temperature reconstructions show their maximum medieval warming in the 10th and 11th centuries whereas models show their maximum medieval warming in the 12th and 13th centuries. [Fredrik Ljungqvist, Sweden]	Rejected. Ljungqvists comments appear to be referring to a different line-numbering
10-584	10	10	44	10	44	Insert "over the period" before "1951-2010". Insert "is" after the dates. Insert "are" after "responses". [Francis Zwiers, Canada]	Accepted
10-585	10	10	44	10	45	I suggest using a slightly more sophisticated low-pass filter than the simple box-car (moving average) filter to prevent leakage of higher-frequency variability, e.g., a Hanning taper or some such filter. [Francis Zwiers, Canada]	Noted. But it doesn't make any difference and might come across as more technical.
10-586	10	10	48	10	49	It is too strong to say that the climate models simulate the climatic periods of the last millennium “generally well”: the models do not capture the timing and amplitude of the Medieval Climate Anomaly that well and have an overall tendency to underestimate centennial scale variability. [Fredrik Ljungqvist, Sweden]	Rejected. Ljungqvists comments appear to be referring to a different line-numbering
10-587	10	10	49	10	49	These sophitictad methods of studying time series all depend on the assumption that all individual items have been obtained under identical circunstances, This assunption is hardly ever true for climate observations, particularly for long lapses of time. This means that all of the studies are probably unreliable. [Vincent Gray, New Zealand]	Taken into account. Caveats on these studies are mentioned as qualifiers, but note that it does not mean these studies are necessarily unreliable
10-588	10	10	51	10	52	..attempts…. have attempted… - better: have been made [Helga Nitsche, Germany]	Accept
10-589	10	10	56	10	56	Do not use such vague and subjective expressions as "generally consistent with". Quantify that amount of consistency with R-squared values or correlation coefficients. [John McLean, Australia]	Rejected. Statement refers to the general consistency of conclusions across multiple studies: an R-squared statistic would be inappropriate here.
10-590	10	11	1	11	1	The mention of "econometrics literature" is irrelevant and a waste of words. [John McLean, Australia]	Rejected. It is important to stress the cross-disciplinary nature of these studies
10-591	10	11	1	11	5	Regarding establishing casual links to/between external drivers, you could consider a recent review/critical examination of major econometrics literature sources in terms of their mathematical treatment of causal concepts (see: B. Chen and J. Pearl, "Regression and Causation: A Critical Examination of Econometrics Textbooks", Technical Report, R-395, October 2012, Computer Science, Univ. California-Los Angeles, and: J. Pearl, "Causality", Cambridge University Press, 2000). [Government of Canada]	Noted. Various additional econometric references have been added.

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10-592	10	11	7	11	12	I think this comes from the last IPCC report, but it was wrong then and it is even more wrong now. Granger causality is the basis for one early (1997) paper in Nature but since then, statistical analyses try to make a direct connection between temperature and radiative forcing. I suggest that this paragraph be placed with the following material, which summarizes statistical approaches to detection and attribution. [Robert Kaufmann, United States of America]	Taken into account. Very useful suggestions. We have had to edit the proposed text to satisfy length constraints.
10-593	10	11	7	11	12	Many of the time-series methods that are used to detect and attribute climate change can be cast in the overall framework of cointegration and error correction. This methodology is used because the blind application of standard statistical techniques, such as ordinary least squares, is invalidated by the presence of stochastic trends in the time series for radiaive forcing and temperature (e.g. Bloomfield and Nychka, 1992; Woodward and Gray, 1993; 1995; Stern and Kaufmann, 2000). Stochastic trends in the time series may generate spurious regressions, in which standard diagnostic statistics, such as correlation coefficients and t statistics, are likely to indicate that there is a significant relation between variables when none exists (Yule, 1929; Granger and Newbold, 1974; Hendry and Juselius, 2000). The potential for spurious regression results lead the first IPCC report to caution "rigorous statistical tools do not exist to show whether relationships between statistically non-stationary data of this kind are truly statistically significant (Folland et al 1992, p. 163)." [Robert Kaufmann, United States of America]	Accepted, within length constraints
10-594	10	11	7	11	12	The statistical difficulties associated with analyzing relations among nonstationary time series are overcome when Engle and Granger (1987) develop the idea of cointegration and Johansen and Juselius (1990) and Stock and Watson (1993) develop methods to estimate cointergating relations. Cointegration is based on the idea that if two or more non-stationary time series have a functionally dependent relation, the stochastic trends present in some of the series will be present in the other. This shared trend implies that there will be at least one or more linear combinations of the series that is stationary so that there is no stochastic trend in the regression residual. [Robert Kaufmann, United States of America]	Accepted, within length constraints
10-595	10	11	7	11	12	If the stochastic trend(s) in radiative forcing imparts a stochastic trend to temperature, the long-run cointegrating relation is given by: (1) in which Temp is temperature, F is radiative forcing, and is a stationary error term. The stationary nature of is critical—it implies that the stochastic trends in temperature and radiative forcing are eliminated via the linear combination given by and (i.e. temperature and raditive forcing cointegrate). From this perspective, stochastic trends in radaitive forcing are viewed as the 'fingerprints' of human activity that are 'matched' to temperature with a finding of cointegration. [Robert Kaufmann, United States of America]	Taken into account. We have to rely on the cited literature for this level of detail
10-596	10	11	7	11	12	The dynamics of the long-run cointegrating relation is examined using an error correction model: (2) in which is the error correction coefficient, which quantifies the rate at which temperature adjusts to the disequilibrium in the long-run relation between radiative forcing and temperature that is given by equation (1). [Robert Kaufmann, United States of America]	See previous
10-597	10	11	7	11	12	Beyond avoiding spurious regressions, the cointegration/error correction approach is consistent with the physical and economic processes thought to drive climate change. The stochastic trends in radaitive forcing originate in the economic processes that drive anthropogenic emissions of greenhouse gases and the chemical and physical processes that determine their atmospheric residence times (Kaufmann 2006a; Kafmann et al., 2010; Kaufmann et al., in review). Temperature does not have a stochastic trend per se, but this trend is imparted by radiative forcing. [Robert Kaufmann, United States of America]	Noted. Section has been heavily edited.
10-598	10	11	7	11	12	Furthermore, the cointegration/error correction approach generates an equation that is the Euler discrete-time approximation to a simple zero-dimensional energy balance model (Kaufmann et al., subm). As such, statistical coefficients estimated from equations (1) and (2) can be compared directly to several parameters in process-based climate models. The value of from equation (1) represents the transient climate response (Kaufmann et al., 2006b) and statistical models estimate values from the observational temperature record that range between 1.7oC to 2.5oC (Kaufmann and Stock, 2006a, Mills, 2009; Kaufmann et al, 2011). Statistical estimates for from equation (2) can be converted to the e-folding rate (Kaufmann et al., subm).	See previous

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						[Robert Kaufmann, United States of America]	
10-599	10	11	7	11	12	Despite these advantages, the cointegration/error correction approach is not the only statistical methodology used for attribution. Other analysts argue that the time series for global surface temperature can be described as a trend-stationary process, with or without a one-time structural change (Gay et al., 2009; Estrada et al., 2010). According to this perspective, the data generating process for temperature (and radiative forcing) is given by: (3) in which t measures the passage of time, Tb is the date of a one-time structural change at which the slope of the time trend is altered by , and is a stationary error term. According to this model, temperature and/or radiative forcing changes on average by the same mean quantity (γ) year after year, until there is a structural change, after which temperature and/or radiative forcing changes by a different average rate (γ + λ) year after year. Statistical tests fail to reject the hypothesis that temperature is trend stationary with a break (Gay et al., 2009), but radiative forcing cannot be modeled as being trend stationary with a break (Kaufmann et al., 2010; Kaufmann et al., in review) and equation (3) cannot be related the physical processes that drive climate change. [Robert Kaufmann, United States of America]	Noted. Useful to emphasise variety of approaches
10-600	10	11	7	11	12	A third statistical approach is to model temperature as fractionally integrated or exhibiting long-run dependence, see for example Bloomfield and Nychka (1992) and Rea et al (2011). Mann (2011) argues that, in finite samples, temperature data generated by a zero-dimensional energy balance model, with historical forcings, is capable of generating temperature data that appear to exhibit long-run dependence, but by construction do not. [Robert Kaufmann, United States of America]	Noted. Useful to emphasise variety of approaches
10-601	10	11	7	11	12	Literature Cited Bloomfield, P. and Nychka, D.: 1992, 'Climate spectra and detecting climate change,' Climate Change, 21, 275-287. Crowley, TJ, 2000 Causes of climate change over the past 1000 years, Science 289:270-277. Engle, R.F. and C.W.J. Granger, 1987, Co-integration and error correction: representation, estimation, and testing. Econometrica 55: 251-276. Estrada, F., C. Gay, and A. Sanchez, 2010, Reply to "Does temperature contain a stochastic trend? Evaluating conflicting results by Kaufmann et al" Climatic Change, 101(3-4):407-414, DOI:10.1007//s10584-010-9928-0. Folland, C.K. et al, Observed climate variability and change, in Climate Change 1992: The Supplementary Report to the IPCC Scientific Assessment edited by J.T. Houghton, B.A. Callander, and S.K. Varney, Cambridge University Press, Cambridge, 1992. Gay, C., Estrada, F. and Sanchez, A.: 2009, Global and hemispheric temperature revisited, Climatic Change 94:333-349 DOI 10.1007/s10584-008-9524-8 Granger, C. W. J. and P. Newbold, 1974, Spurious regressions in econometrics. Journal of Econometrics, 2:111-120. Hendry, D. and Juselius, K. 2000, Explaining cointegration analysis: Part 1, Energy Journal, 21, 1–4. Johansen, S. and K. Juselius, 1990, Maximum likelihood estimation and inference on cointegration with application to the demand for money, Oxford Bulletin of Economics and Statistics, 52, 169-209, 1990. Kaufmann, R.K. H. Kauppi, and J.H. Stock, 2006a, Emission, concentrations, & temperature: a time series analysis. Climatic Change 77:249-278 Kaufmann, R.K. H. Kauppi, and J.H. Stock, 2006b, The relationship between radiative forcing and temperature: what do statistical analyses of the instrumental temperature record measure? Climatic Change 77:279-289. Kaufmann, R.K. H. Kauppi, and J.H. Stock, 2010, Does temperature contain a stochastic trend? Evaluating conflicting statistical results. Climatic Change. 101:395-405. Kaufmann, RK, H. Kauppi, ML Mann, and J.H Stock, 2011, Reconciling anthropogenic climate change with observed temperature 1998-2008, Proceedings National Academy of Sciences 108(29):11790-11793 doi/10.073/pnas.1102467108. Kaufmann, RK, H. Kauppi, ML Mann, and J.H Stock, subm, Does temperature contain a stochastic trend:	Noted. Very helpful suggestions.

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						linking statistical results to physical mechanisms, Climatic Change. Mann, ME, 2011, On long range temperature dependence in global surface temperature series, Climatic Change, 107:267-276. Mills, T.C. 2009, How robust is the long-run relationship between temperature and radiative forcing? Climatic Change 94:351-361, DOI 10.1007/S10584-008-9525-7. Rea, W. M. Reale, and J. Brown, 2011, Long memory in temperature reconstructions, Climatic Change, 107:247-265, DOI 10.1007/s10584-011-0068-y. Stern, D.I. and R.K. Kaufmann, 2000, Is there a global warming signal in hemispheric temperature series: a structural time series approach Climatic Change. 47:411-438. Stock, J.H. and M.W. Watson, A simple estimator of cointegrating vectors in higher order integrated systems Econometrica 61, 783-820, 1993. Woodward, W. A. and Gray, H. L.: 1993, 'Global warming and the problem of testing for trend in time series data', Journal of Climate 6, 953-962. Woodward, W. A. and Gray, H. L.: 1995, 'Selecting a model for detecting the presence of a trend', Journal of Climate 8, 1929-1937. Yule, G. 1929 An Introduction to the Theory of Statistics, C. Griffin and Co., London. [Robert Kaufmann, United States of America]	
10-602	10	11	7	11	12	I know that the equations do not appear, but should you want them, I have the text saved as a word file and am happy to send it to you. [Robert Kaufmann, United States of America]	Taken into account. Very helpful, thank you.
10-603	10	11	8	11	10	This is a poor definition of Granger causality. The Granger causality test is a statistical hypothesis test for determining whether one time series is useful in forecasting another. “Significantly increases the magnitude of the estimated noise” is a very clumsy way of describing the significance testing, which tests true parameters not estimates and also accounts for the different number of parameters in time series models with and without the additional variable. It would be useful to provide a reference to where this is explained in more detail and successfully applied to a climate prediction problem e.g. Mosedale, T.J., Stephenson, D.B., Collins, M. and Mills, T.C. (2006): Granger Causality of Coupled Climate Processes: Ocean Feedback on the North Atlantic Oscillation, J. Climate, 19, pp 1182-1194. [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This section hasl been rewritten.
10-604	10	11	9	11	10	How can noise be required? [John McLean, Australia]	Taken into account. This section rewritten.
10-605	10	11	10	11	10	Change "of the relationship between them" to "for predicting the second variable" - otherwise the sentence doesn't make sense! [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This section rewritten.
10-606	10	11	11	11	11	Do you mean "Box 10.1"? [Francis Zwiers, Canada]	Yes
10-607	10	11	14	11	14	correct studied [LUCILA CANDELA, Spain]	Accepted
10-608	10	11	14	11	14	Granger causality', may be hard to understand for non-English readers. [Daoyi Gong, China]	Taken into account. This section rewritten.
10-609	10	11	16	11	16	Insert "the" ahead of "correlation". [Francis Zwiers, Canada]	This section rewritten.
10-610	10	11	23	11	23	An instance (one of many) of a place where "identification" might be preferable to "detection". [J. Graham Cogley, Canada]	Noted, but we decided that introducing a new concept would be unhelpful at this point.
10-611	10	11	23	11	23	It should be made clear whether "detection of the influence of influence of greenhouse gases in the global temperature record" relates merely to "Detection" (a weak finding, which might relate to only a minor influence), or covers Attribution as well. The Imbers et al. (2012a) analysis relies upon an EBM based allocation of the observed changes in annual global temperature between several different forcings. The extremely high co-linearity between greenhouse gas and sulphate aerosol forcings, on a global mean scale, and the lack of any significant short term fluctuations in either, makes separating their effects on global mean temperature very difficult. It is unclear that robust Attribution results for these two forcings can be obtained through EBM based studies, even in the least demanding case when an internal variability model with no memory at all is used. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account with material on degeneracy earlier in the chapter.
10-612	10	11	23			*Imbers et al. (2012a) demonstrate [Tibor Farago, Hungary]	Accept

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10-613	10	11	24			*increasing greenhouse gases emissions (or concentrations) .. [Tibor Farago, Hungary]	Taken into account. Concentrations
10-614	10	11	27	11	27	Suggest simplifying the title - "Optimal Fingerprinting Methods", for example. You might imagine studies that use signals from other sources than just GCMs. [Francis Zwiers, Canada]	Rejected. We disagree -- the "and" in the title is inclusive
10-615	10	11	29	12	14	The errors associated with optimal fingerprints (e.g. North and Stevens, 1992) are not discussed here. It may be valuable to mention them here. References: North, Gerald R., Mark J. Stevens, 1998: Detecting Climate Signals in the Surface Temperature Record. J. Climate, 11, 563–577.The errors associated with optimal fingerprints (e.g. North and Stevens, 1992) are not discussed here. It might be important to mention them here. [Government of United States of America]	Taken into account. This is relevant to the issue of degeneracy -- see 10-611
10-616	10	11	34	11	35	If "results" mean only "detection" vs "no detection", this is maybe correct. However, as soon as the contributions from some forcings are assessed (eg via the computation of attributable warming), the "precise specification of variability" is required to compute confidence intervals. [Aurélien Ribes, France]	Taken into account. Clarified.
10-617	10	11	36	10	36	missing word: ,as it is often... [Helga Nitsche, Germany]	Rejected. It is grammatical as is
10-618	10	11	36	10	36	regional or non-temperature indicators ?? What is meant here? [Helga Nitsche, Germany]	We have clarified
10-619	10	11	39	11	39	Remove hanging bracket in front of "for". [Government of Canada]	accepted.
10-620	10	11	44	11	44	Identify the box. [Francis Zwiers, Canada]	Accepted.
10-621	10	11	50	11	50	replace "inter-model noise" by "model structural uncertainty" or "model epistemic uncertainty" to avoid the confusion with noise being internal variability. [Laurent Terray, France]	Noted. Good suggestion.
10-622	10	11	52	12	6	again I find it difficult to follow the line of this passage [Helga Nitsche, Germany]	Taken into account. Clarified.
10-623	10	11	52	12	6	The paragraph seems overly technical for the target audience. Suggest you retain the first and last sentences and discard the rest. [James Renwick, New Zealand]	Rejected. The Ribes approach is the key technical innovation in optimal fingerprinting since AR4
10-624	10	11	52	12	6	This paragraph has considerably changed between the FOD and SOD and some important features have been removed or altered. The fact that ROF also allows to have a well conditioned and invertible covariance matrix should be mentioned again. The sentence page 11 line 57 and page 12 line 1 is not fully correct: ROF allows to have a more accurate estimate of the true covariance (and not of the "true inverse covariance"). The arbitrariness in truncation choice has been removed in the SOD while it remains unsolved within the standard OF method. The full sentence beginning by "Key attribution results ..." is at best much too vague and imprecise and possibly a bit misleading. I suggest to either remove it or alter it substantially. Note that all results presented in figure 10.4, from which the GHG and other forcing uncertainty ranges are estimated, do include optimization. Finally, the description of the TOD method is not correct: the smoothly varying time series is provided by the models, not the other way around. [Laurent Terray, France]	Taken into account. Revisited with refernence to the FOD
10-625	10	11	55	11	55	Internal variability can also be estimated (and has been estimated in some studies) from the variation between ensemble members in ensembles of historical forcing runs. [Francis Zwiers, Canada]	Noted. Good point.
10-626	10	11	55	12	3	It seems to me that the description of the Regularised Optimal Fingerprint method provided in the FOD was more accurate (in particular, p12, l31 to l37 in the FOD). In particular, I would say that the description provided here has two specific shortcomings: [p11 l57 to p12 l1], the Ledoit and Wolf estimate has been only shown to provide a more accurate estimate of the true covariance matrix (not its inverse); [p12 l1 to l2], I agree that some key attribution results can be obtained without any optimisation (e.g. Figure 10.1 could be mentioned as a useful illustration of that), but I would not say that "results do not depend on optimisation", as the way optimisation is applied may have substantial impact on the results (see e.g. sentivity of the results to the truncation in Jones et al., 2012, or Ribes and Terray, 2012). Then, about the ROF method in general, Ribes et al., 2012a, is potentially another relevent reference. [Aurélien Ribes, France]	Taken into account. Revisited with refernence to the FOD
10-627	10	12	1	12	6	The IPCC summary strongly makes the case for "major anthropogenic contribution", not just detectable anthropogenic contribution. A number of researchers are proposing the alternative model of minor anthropogenic contribution. e.g. Syun-Ichi Akasofu (2010), D'Aleo and Easterbrook (2011), Nicola Scafetta	Noted. The discussion of the null-hypothesis has been heavily shortened.

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						(2011), and Loehle & Singer (2010) etc. Some are holding that the uncertainties are so large and/or anthropogenic contribution so small relative to natural causes that statistical anthropogenic attribution has not been achieved. e.g., Singer (2011). To make the case for "major anthropogenic contribution", the IPCC needs further clearly distinguish between these three major options. I recommend adding: "The next stage is to quantitatively distinguish between the null hypothesis (negligible or indistinguishable anthropogenic controbution), minor anthropogenic contribution (>~5% to 50%) with major natural contribution, and major anthropogenic contribution (> 50%) with minor natural contribution." [David L. Hagen, United States of America]	
10-628	10	12	1	12	6	References: Syun-Ichi Akasofu (2010), On the recovery from the Little Ice Age, Natural Science, Vol. 2, No. 11, 1211-1224, doi:10.4236/ns.2010.211149; 'Aleo, J. and Easterbrook, D.J., 2011, Relationship of multidecadal global temperatures to multidecadal oceanic oscillations: in Easterbrook, D.J., ed., Evidence-Based Climate Science, Elsevier Inc., p. 161-184; Nicola Scafetta (2011); S. Fred Singer, (2011) NIPCC. NIPCC vs. IPCC, Addressing the Disparity between Climate Models and Observations: Testing the Hypothesis of Anthropogenic Global Warming (AGW) Interim Science Update Presented at Majorana Conference in Erice, Sicily August 2011 [David L. Hagen, United States of America]	We have focussed this assessment on peer-reviewed studies.
10-629	10	12	3	12	6	For the "Temporal Optimal Detection" method this is the smoothly-varying time-series that is estimated from a climate model (simulating past climate) and the spatial pattern of the signal is inferred from the application of the method. The reference of Santer et al. (1994) is missing in the reference list. It is thus difficult to check wether this reference remains appropriate after the correction. [Serge PLANTON, France]	This was what we meant -- have clarified.
10-630	10	12	3	12	6	There is a small confusion here, as the spatial pattern is not estimated from a climate model in the TOD method. A simple way to reformulate the sentence could be e.g. "…, under which each signal is assumed to consist of a smoothly-varying time-series (estimated from a climate model) modulated by a single spatial pattern." [Aurélien Ribes, France]	Noted. Have clarified.
10-631	10	12	8	12	14	needs citation [Andreas Walter, Germany]	Have added Allen & Tett (1999) & Ribes et al (2012a)
10-632	10	12	8	12	14	There should be some discussion here of careful analysis of the residual consistency test that was provided in Ribes et al (2012a), which noted that some improvements that could be made. [Francis Zwiers, Canada]	Accepted
10-633	10	12	10	12	11	Unclear how this relates to the fact that the magnitude of the model simulated changes is not important as stated on page 10, lines 25-29. [Albert Klein Tank, Netherlands]	Have clarified the earlier statement, which referred to robustness of detection results
10-634	10	12	11	12	11	Identify the box. [Francis Zwiers, Canada]	Accepted.
10-635	10	12	12	12	12	.. Is treated with caution…..'is treated' or 'should be treated'/'must be treated'? [Helga Nitsche, Germany]	Noted .Good point.
10-636	10	12	12			"If either of these checks fails, the attribution result is treated with caution." ? Actually, in such a case, generally: no attribution or very low confidence in such an attribution, since the selected model cannot "explain" the difference between the residual and the observed variabilities. This last step is also of crucial importance in attribution. Actually what is explained in this paragraph is on the first and second type errors related to statistical hypotheses (also used in the decision-making theory). [Tibor Farago, Hungary]	Taken int account. Language has been strengthened
10-637	10	12	13	12	14	What are the specific limitations of this test? E.g. The standard residual consistency test has a "liberal bias" (e.g. Gillet et al., 2005) and interpreting the cause of a failed F-test is ambiguous (Terray et al., 2012; Allen et al., 2006) [Oliver David Andrews, United Kingdom]	Taken into account. Clarified.
10-638	10	12	16	12	35	Reflecting how much of this Section 2 could probably be condensed, I think WGII Chapter 18 will have much less on the single-/multi-step issue. [Dáithí Stone, United States of America]	Accepted.
10-639	10	12	18	12	22	For the last 32 years, the first 16 showed substantial warming as noted "strong warming since the mid-1970s)." However, the last 16 years have show little if any statistically significant warming (as has been highly publicized). Strongly recommend clarifying the warming rate since 2000. e.g., recommend changing line 20 to read: "with little trend, and strong warming since the mid-1970s (Section 2.2.3, Figure 10.3) followed by little trend for at least the last decade." [David L. Hagen, United States of America]	Rejected. Misplaced comment?
10-640	10	12	20	12	21	I think this statement concerning when attribution is made needs to be better nuanced. First, there is a	Accepted. Good suggestion.

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						problem with statistical language noted earlier - attribution is supported by a significance test, and so the reference should be to a significance level rather than a "confidence level" (see earlier comment in the same vein). Secondly, the statistical test of consistency is not entirely satisfactory, as noted by Berliner et al (2000, J. Climate). That test uses the null hypothesis that the scaling factor(s) is(are) unity, and we claim consistency when we fail to reject the null. The problem with this is that not possible to describe the power to detect consistency, because the test assumes consistency as the default situation and then seeks evidence to the contrary. Berliner proposed a Bayesian alternative, which has been used a few times in the literature, but not broadly. A better statement would be to say that this test provides some guidance (e.g., if you reject the null, then attribution should not be made, while if you do not reject the null, then it would be appropriate to proceed by determining, through process understanding and expert judgement, whether confounding factors would impede making an attribution. See also the related discussion in Hegerl et al (2007). [Francis Zwiers, Canada]	
10-641	10	12	22	12	22	Delete the first two commas. [J. Graham Cogley, Canada]	Agreed
10-642	10	12	28	12	28	Insert "e.g.," ahead of the Stott et al reference. Also, it might be an idea to cite an example from WG2. [Francis Zwiers, Canada]	Accepted.
10-643	10	12	33	12	34	"the response of the variable in question to all external forcings". [J. Graham Cogley, Canada]	We have retained the second half
10-644	10	12	37	12	37	Shorten the title! [Francis Zwiers, Canada]	Accepted
10-645	10	12	38	12	38	Do not hyphenate "null hypothesis". ("null" is an adjective, just like "alternative".) [J. Graham Cogley, Canada]	Accepted
10-646	10	12	40	12	41	It is disingenuous to state that frequentist approaches minimize reliance on prior assumptions. Frequentist approaches make implicit assumptions that are incorrect (e.g. independence of climate model outputs, random sample of models etc.). So it is completely misleading to say that Bayesian approaches pay a price for making their assumptions explicit. The whole of the detection-attribution testing relies upon the choice of how one believes the real world might be related to climate model outputs and there is not yet any consensus on how to do such inference (see Section 9.8.3.1 and Stephenson, D.B, Collins, M., Rougier, J.C., and Chandler, R.E. (2012) Statistical Problems in the Probabilistic Prediction of Climate Change, Environmetrics). So it should be made clear in this section that the testing is dependent on the choice of the very simplistic regression approach used for D&A. Another caveat that should be mentioned here is that the power of the tests (i.e. the ability to reject the null when it is false) can be very low for short noisy series. Ideally the authors might be able to cite some D&A studies that have done some power testing? Is this why the authors arbitrarily choose the 10% level of significance here rather than the more standard 5% level? [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	taken into account. Happy to make clear the simplicity of the regression model: but also that there is no clear evidence that the world is not indeed that simple, at least for large scale temperatures
10-647	10	12	40	12	56	Is there any study that indicates the ocean warming is due to mainly human activities ? Why two statistical approaches (Bayesian and Frequentist Approaches) are considered ? Why not other skill scores are tested to evaluate the model results ? [Government of India]	Yes, assessed later. We will mention other skill measures with reference to Ch09
10-648	10	12	41	12	42	Your statement about anthropogenic greenhouse gases causing the warming is simply unsustainable. The output of a climate model depends on the assumptions built into it and climate models used by the IPCC have a pattern of over-estimating the influence of CO2. McLean et al (2009) showed that the ENSO is the likely driver of global average temperature and that there is very little, if any, warming that other forces (incl. CO2) bneed to account for. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing we authors to respond, and surely you don't condone that refusal?). [John McLean, Australia]	Rejected. This is assessed elsewhere -- focus here is on principles, not results
10-649	10	12	44	12	46	The submitted manuscript by Verheggen et al (submitted to Climatic Change, 2012) argues that the AR4 statement that this section refers to ("most of warming very likely due to GHG") was indeed a conservative	Noted. Useful reference, but unfortunately not accepted in time.

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						assessment, and moreover, that it probably led climate scientists to underestimate the GHG contribution to recent warming. This is based on the outcome of a detailed survey amongst a climate scientists. Citation: "Scientists' views about attribution of global warming", Bart Verheggen, Bart Strengers, John Cook, Rob van Dorland, Kees Vringer, Jeroen Peters, Hans Visser, Leo Meyer, submitted to Climatic Change, 31 July 2012. [Bart Verheggen, Netherlands]	
10-650	10	12	46	12	47	It is not necessarily true that a formal Bayesian analysis would result in tighter uncertainty band. It would presumably result in more defensible uncertainty estimates. [Peter Guttorp, United States of America]	Noted. Not necessarily true, but true in this example
10-651	10	12	48	12	48	Insert "to greater or lesser extents" ahead of "on those prior assumptions". The prior has little influence on inferences if the evidence in the data dominates (detection of human influence on global mean temperature is probably an example, where the choice between a reasonable range of prior distribution on beta would have little influence on the posterior distribution on beta; in contrast, the prior seems very important in making inferences about equilibrium climate sensitivity). [Francis Zwiers, Canada]	Accepted
10-652	10	12	51	12	51	The "experts" that you refer to are not independent of the IPCC processes or of climate modelling, so it is reasonable to ask whether they are biased. And what does it matter that a group of "experts" reach a consensus when scientific truth is not determined by whether a consensus exists? The sentence, as you present it, is false on multiple levels and should be deleted, in fact the entire paragraph should be deleted. [John McLean, Australia]	Noted. See 10-649
10-653	10	12	51	12	51	Suggest replacing "still" with "also". [Francis Zwiers, Canada]	Accepted
10-654	10	12	51	12	56	I found this confusing. The first half explains that expert judgement may be used to downweight a frequentist conclusion to allow for remaininig uncertainties. This is important to note as I am not sure how widely it is appreciated. The next sentence talks about prediction statements (what prediction statements?) based on observations and expert judgement, may seem more confident than attribution statements, even they refer to the same variable on succesive decades. I think I understand what it means, but I am not sure the last two sentences are helpful here. [John Mitchell, United Kingdom]	We think it is important to make the link to prediction in Ch11, but section has been heavily edited.
10-655	10	13	1	13	6	This paragraph appears to confuse Attribution with Detection, a confusion that seems to occur repeatedly in the Chapter. The null-hypothesis of no or negligible human influence on any particular climate variable is not conventionally used for Attribution, as implied here by the reference to the role of the null hypothesis in any attribution assessment. The null-hypothesis is only used for Detection, which is a weak finding that does not imply that the detected influence of human effects on the climate system is either material or consistent with the magnitude of the influence simulated by climate models. When it comes to Attribution, assessing whether the influence detected is consistent with simulations thereof by a climate model, the null hypothesis is conventionally (but, in my view, wrongly) reversed. Consistency between model and observations is then the null hypothesis, with a 95%+ probability of inconsistency having to be shown for Attribution to be rejected. That is an extremely weak test. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Attribution means detection when all physically plausible alternative explanations are taken into account, so the role of the null is the same. The "consistency with model-simulated amplitude" test is not, in fact, generally applied, although earlier wording in the chapter was unclear on this, which we have clarified.
10-656	10	13	1	13	6	The Attribution framework also has perverse effects. If model (ensemble) ensemble simulations indicate that human influence will affect an observable climate variable to the extent Z, and the measured effect is 0.6Z, then Detection will be achieved (assuming a typical statistical test) if the observational standard error does not exceed 0.3Z. If that error lies between 0.2Z and 0.3Z, Attribution will also be achieved. But if the error is below 0.2Z, implying a stronger Detection result, Attribution will not be achieved. The Attribution part of the framework seems unsatisfactory. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Attribution means detection when all physically plausible alternative explanations are taken into account, so the role of the null is the same. The "consistency with model-simulated amplitude" test is not, in fact, generally applied, although earlier wording in the chapter was unclear on this, which we have clarified.
10-657	10	13	1	13	6	This is not entirely clear and I think gives a bit too much weight to testing null hypotheses. First, as discussed in previous comments, D&A involves two null hypotheses that are tested sequentially, not just one - ie., first that the scaling factors are zero (i.e., no human influence) and secondly, whether scaling factors are unity (i.e., the expected signal is present with the expected magnitude). But statisticians would point out that the testing paradigm is rather narrow - and that it is preferrable to focus on interval estimates (e.g., confidence intervals) of the free parameters of the fitted regression models once it has been determined that those models fit well (e.g., through examination of the residuals - as in the residual consistency test). The way D&A is practiced is	Taken into account. Have revised

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						much more along these lines than strictly being a testing exercise - inferences are made after studying the residual consistency test results, and based primarily confidence intervals rather than yes-no test result. Moreover, the implications of the uncertainty in scaling factor estimates that is expressed through the confidence intervals is carried through when D&A results are used to constrain projections. So I don't think I would buy onto the implied criticism as strongly as is done in this paragraph. [Francis Zwiers, Canada]	
10-658	10	13	2	13	6	You continue to ignore the demonstrated link between a change in ENSO conditions - not merely El Nino and La Nina states but the entire spectrum - and a corresponding later change average global temperature. Unless you can demonstrate that ENSO has no impact, having first removed all the short-term variation in temperature and SOI, then you are being very remiss by failing to mention this link and it needs to be corrected. [John McLean, Australia]	Rejected. This is assessed later in the chapter-- this section is about methods
10-659	10	13	3	13	5	Unclear. What does "biased towards well-observed, well-modelled variables and regions" mean? From the preceding I think it means that attribution results using this approach will be biased towards the low end of human influence. [Bart Verheggen, Netherlands]	Taken into account. Clarified.
10-660	10	13	4	13	4	Clarify, I presume this does mean there are persistent errors, just that attribution is more likely to be possible where there is good data etc. [John Mitchell, United Kingdom]	Correct
10-661	10	13	4	13	6	This important note may be considered for inclusion in the E.S. of the chapter. [Albert Klein Tank, Netherlands]	Rejected. This is more of an issue for WG2
10-662	10	13	7	13	7	It would be useful to have a clearer summary of what are considered as the main assumptions and limiting factors in arriving at attribution, and how they affect confidence and likelihood assessments for various attribution problems Linearity? Models? Observations? The relative importance must vary depending on the problem. [Government of United States of America]	Taken into account. The link between detection and attribution has been clarified earlier
10-663	10	13	18	13	30	This text is remarkably evasive. If expressed with integrity it should be stated that temperatures failed to rise from 1945 to 1977 and that there has been no statistically significant warming since 1997. [John McLean, Australia]	Taken into account. The text has been rewritten and now states that the trend since 1998 has been small. We defer discussion of the significance of the trend to box 9.2.
10-664	10	13	18			In my view the reasons for the mid-1970s climate shift need to be addressed in ch 10, something along the lines of: "A significant climate change in the 20th century was the mid-1970s climate shift in the Pacific when the internally-generated Interdecadal Pacific Oscillation (IPO, see Ch. 14, section 14.2.5) transitioned from negative to positive (i.e. somewhat cooler tropical Pacific SSTs went to somewhat warmer). This occurred at a time when globally averaged surface air temperatures also suddenly started to increase. Meehl et al (2009) used climate model simulations of the 20th century and a long control run to show that the pattern of response to external forcing in the Pacific, related mainly to increasing GHGs, had some similarities to the pattern of the internally generated IPO, making definitive attribution difficult with regards to how much of the mid-1970s shift was natural and how much was externally forced. Using a regression-based approach, Meehl et al (2009) inferred that about 25% of the mid-1970s shift in the Pacific was internally generated by a transition of the IPO from negative to positive, and about 25% was externally forced mainly due to the increase of GHGs." Meehl, G. A., A. Hu, and B.D. Santer, 2009: The mid-1970s climate shift in the Pacific and the relative roles of forced versus inherent decadal variability, J. Climate, 22, 780--792. [Gerald Meehl, United States of America]	Rejected. Our focus her is on an assessment of the detection and attribution literature, in particular considering which external forcings have had a detectable influence in observations. Our main focus is not on the mechanisms underlying unforced climate changes which is beyond the scope of our chapter.
10-665	10	13	18			Another significant feature of 20th century climate that needs to be addressed in ch. 10 is the warming hole: "A curious attribute of the spatial pattern of regional surface air temperature change in the second half of the 20th century was nearly a total lack of warming trend over the southeastern U.S., often referred to as the "warming hole" (e.g. Kunkel et al., 2006). This was associated with an east-west differential in the increase of heat extremes, with fewer heat extremes in the eastern U.S., and more heat extremes in the western U.S. (Meehl et al., 2009). A subsequent study determined that the warming hole was most likely due to atmospheric circulation anomalies over the U.S. related to decadal timescale variability of tropical Pacific SSTs (Meehl et al., 2012). These circulation anomalies were associated with processes that overcame the warming that would have occurred in that region due to an increase in GHGs by advecting cooler air into that part of the U.S. during the cold weather months, and providing increased moisture convergence that produced greater clouds and precipitation in the warm weather months (Meehl et al., 2012). Kunkel, K.E., X.-Z. Liang, J. Zhu, Y. Lin, 2006: Can CGCMs simulate the Twentieth-Century "Warming Hole" in the Central United	Rejected. Our focus her is on an assessment of the detection and attribution literature, in particular considering which external forcings have had a detectable influence in observations. Our main focus is not on the mechanisms underlying unforced climate changes.

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						States? J. Climate, 19, 4137-4153. Meehl, G.A., C. Tebaldi, G. Walton, D. Easterling, and L. McDaniel, 2009: The relative increase of record high maximum temperatures compared to record low minimum temperatures in the U.S. Geophys. Res. Lett., 36, L23701, doi:10.1029/2009GL040736. Meehl, G.A., J.M. Arblaster, and G. Branstator, 2012: Mechanisms contributing to the warming hole and the consequent U.S. east-west differential of heat extremes. J. Climate, doi: http://dx.doi.org/10.1175/JCLI-D-11-00655.1. [Gerald Meehl, United States of America]	
10-666	10	13	19	13	19	The Figure does not show "global mean temperature" which currently cannot be determined, but "Mean Global Surface Temperature Anomaly" which is subject to much uncertainty and upward biases, so these conclusions are dubious. [Vincent Gray, New Zealand]	Taken into account. We now use the term 'Global mean surface temperature' which is defined in the glosssary. If the anomalies in GMST warmed over the period concerned then GMST warmed over the same period, so the statement is true as written.
10-667	10	13	19	13	20	"global mean temperatures" -> "global mean surface air temperatures" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. We now use 'Global mean surface temperature', which is defined in the glossary. This has been replaced throughout this section.
10-668	10	13	19	13	30	Figure 10.1: Error bars in observational estimate of global surface temperature estimates are neither discussed or plotted in this figure. Please add them. [Government of United States of America]	Rejected. The main aim of this figure is to compare simulated temperatures with observed temperatures. We use three different observational datasets to give an idea of observational unceratinty. However, the figure would become too cluttered if we included the HadCRUT4 uncertainties. These are discussed in detail in chapter 2. Note that we do now assess a study which explicitly accounts for the HadCRUT4 uncertainties in detection and attribution analyses.
10-669	10	13	19	13	30	Delete "(Figure 10.1)" from line 19 and change the cross-reference in line 20 to "(Section 2.4.3, Figure 10.1)". Section 2.2… should be 2.4… throughout the paragraph. Line 22 refers to Figure 10.2 not 10.3. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Suggested corrections made.
10-670	10	13	19	13	30	A reference to Simmons et al. (2010) could be included for balance along with that to Hansen et al.(2010), as Simmons et al. pointed out that CRUTEM3 gave less warming in all-land integrals because of poor sampling by CRUTEM3 (and implicitly HadCRUT3) of high-latitude regions where ERA-Interim exhibited stong warming. Jones at al. (2012), in publishing the CRUTEM4 dataset, showed that the inclusion of additional high-latutude station data in CRUTEM4 brings it into closer agreement with ERA-Interim. This is an instance where reanalysis was ahead of the traditional CRUTEM approach (as regards variations since 1979 at least), so it would be fair to acknowledge this. [Adrian Simmons, United Kingdom]	Rejected. We have now shortened this discussion and defer more detailed consideration of observational uncertainty to chapter 2.
10-671	10	13	19	13	30	All cross-references to Chapter 2 sections are out of date. The temperature section of Chapter 2 is now Section 2.4 and the number of sub-sections (and therefore the indexing) has changed in some cases. [Peter Thorne, United States of America]	Taken into account. These have been updated based on the SOD.
10-672	10	13	20	13	22	"Almost the whole global has seen warming since 1901 while over the satellite period since 1979 some regions have seen cooling" - this statement does not make sense. It could be interpreted as there has been cooling since 1979. [Government of Australia]	Taken into account. 'Some regions' replaced with 'a few regions' to clarify that this is only a small part of the globe.
10-673	10	13	20	13	22	Revise sentence. The cited figure 3 is of zonal averages, and thus one cannot infer the conditions for the "whole globe" there from. The appropriate figure to reference, given the sentence content, is Fig. 2. The revised sentence then must also state that temperature over many areas of the globe have not been observed since 1901. One suggestion is to rewrite as follows.... "almost all locations having observed records since 1901 have warmed...". [Martin Hoerling, United States of America]	Taken into account. Figure reference corrected. Rephrased to say that the globe has warmed at observed locations.
10-674	10	13	21	13	21	"globe". [J. Graham Cogley, Canada]	Editorial. Corrected.
10-675	10	13	21	13	21	global what? Or: Globe? [Helga Nitsche, Germany]	Edutorial. Corrected - globe.
10-676	10	13	21	13	21	"...globe has seen surface warming…" ? [James Renwick, New Zealand]	Editorial. Corrected.

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10-677	10	13	21	13	21	a word is missing after global [Laurent Terray, France]	Editorial. Corrected.
10-678	10	13	21	13	21	globe not global [Peter Thorne, United States of America]	Editorial. Corrected.
10-679	10	13	21			*instead ”global”: globe [Tibor Farago, Hungary]	Editorial. Corrected.
10-680	10	13	22	13	22	The correct reference here is maybe Figure 10.2 (instead of Figure 10.3). [Aurélien Ribes, France]	Editorial. Corrected.
10-681	10	13	24	13	26	Unclear that the actual reason is that HadCRUT does not interpolate over the areas without data, whereas the other datasets do. [Albert Klein Tank, Netherlands]	Taken into account. We now defer discussion on this point to chapter 2 and box 9.2, since this is an observational issue.
10-682	10	13	24	13	28	This sentence could be clearer. It is also reptitive from Ch 2. HadCRUT4 has the error ranges that could quantify this issue of recent trends better. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We have deleted this text comparing datasets, and now just refer to chapter 2.
10-683	10	13	24	13	30	seems rather unspecific to me, I can hardly see the line. [Helga Nitsche, Germany]	Text on past decade now condensed, so lines on figure 10.1 are no longer referred to.
10-684	10	13	28	13	29	Urbanisation is treated as a contamination of the observations now. Should urbanisation form an attribution result instead? [Albert Klein Tank, Netherlands]	Rejected. Urbanisation increased the measured land surface temperature by up to 10%, not the actual mean land surface temperature. It would be hard to model this, owing to the small scales involved, and there are no published attribution studies which do this to our knowledge.
10-685	10	13	28	13	30	Please explain, how urbanisation could cause 10% (!) of the centennial trend, or more in some regions? [Government of Germany]	Rejected. This is assessed in section 2.4.1.3 (observations chapter), which we refer to here.
10-686	10	13	28			“likely” in italics? [Dáithí Stone, United States of America]	Editorial. Yes - changed.
10-687	10	13	29	13	29	The reference to Chapter 2.2.1.2 is not valid, please check. [Government of Germany]	Editorial. Corrected - now 2.4.1.3.
10-688	10	13	29	13	29	This is borderline inconsistent with the characterization in Chapter 2 which says that urbanization may account for up to 25% of reported warming in some regions. [Peter Thorne, United States of America]	Taken into account. This has been revised to more closely follow the wording used in chapter 2.
10-689	10	13	29	13	30	Such as those in which people live? [Dáithí Stone, United States of America]	Taken into account. We are focused on large-scale temperature changes here. We do now give more information on chapter 2's conclusions regarding the regional influence of urbanisation (likely less than 25% of the warming).
10-690	10	13	30			sections 10.3.1 and .2 way too long. Shorten to closer to summary results toward end of section 10.3. emphasis regional results; the global mean temperature stuff has been hashed out over nearly 15 years. [tim barnett, United States of America]	Taken into account. We have shortened these sections. However, we keep considerable discussion of the global mean, because there are many ongoing topical issues related to quantifying how much of the observed warming is attributable to different anthropogenic and natural components.
10-691	10	13	32	13	49	Recently Jha et al. (2012) documented the diversity in the historic experiments of 10 CMIP5 models in simulating different aspects of SST, particularly those associated with ENSO, as well as the impact of low frequency variations on the ENSO variability and its global connection. It is shown that the majority of the CMIP5 models capture the relative large SSTA variance in the tropical central and eastern Pacific, as well as in North Pacific and North Atlantic. Meanwhile, the frequency of ENSO is hardly captured by almost all models, particularly for the period of 5-6 years. The models reproduce the global averaged trends, particularly since 1970s. However, almost no model correctly simulates the spatial pattern of the trends. These results suggest that it is still a challenge to reproduce the features of global historical SST variations with the state-of-the-art coupled general circulation model. The analysis slso suggests that the low frequency variations caused by external forcing’s enhance the SST variability and also modify the global connection of ENSO.	Rejected. This appears to be mainly a model validation issue. A reference to this specific paper is not suitable in this brief description of the simulations which is focused on the simulation of temperature trends.

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						Jha, B., Z.-Z. Hu, and A. Kumar, 2012: SST and ENSO variability and change simulated in historical experiments of CMIP5 models. Clim. Dyn. (submitted). [Zeng-Zhen Hu, United States of America]	
10-692	10	13	36	13	36	The statement "broadly spans" is subjective and has no place in a scientific report. The correlation should be expressed numerically and with standard deviations. [John McLean, Australia]	Taken into account. Revised. We now say 'spans… in almost every year'.
10-693	10	13	36	13	36	Nowhere in this report has it satisfactorily been demonstrated that climate models accurately simulate natural climate forces, whioch means that this sentence has no foundation and must be deleted. [John McLean, Australia]	Rejected. Figure 9.33b compares CMIP5 simulations of the past 1000 years with recoinstructions. Natural forcings are dominant drivers through most of these simulations. Simulated variability is as high or higher than reconstructions on periods less than 1000 years.
10-694	10	13	37	13	38	Your statement assumes that climate models accurately simulate all natural forces but in fact they do not, ergo the graph of "natural net forcing" is very likely incorrect. [John McLean, Australia]	Rejected. The caption to Figure 10.1 notes that the left hand figure shows simulated forcing in the models. Also see response to comment 10-693.
10-695	10	13	40	13	41	The use of a very different period over which the long-term average is calculated is deplorable and lacks integrity. As well as that, the global coverage of 1881-1920 data is much lower than during 1961-90 and the error margin in the data far greater. [John McLean, Australia]	Rejected. As noted in the caption to Figure 10.1, temperature anomalies in each grid cell are calculated relative to a 1961-1990 base period when the coverage was good. This base period was chosen in order to approximate temperature changes relative to preindustrial. The same base period was used for models and observations, so we do not understand why the reviewer objects to this.
10-696	10	13	40	13	41	It would be useful to point out that showing anomalies is unavoidable for the observations because otherwise changes in annual global mean averages of absolute temperatures are confounded with changes in the configuration of the observing system. [Francis Zwiers, Canada]	Taken into account. Suggested change made.
10-697	10	13	40	13	42	Do you mean absolute temperatures here? It is an odd way to say this. You're not testing how far the models are away from the mean of the obs - which is about 14 deg C for the global mean. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This text justifies why we are showing anomalies rather than absoluate temperatures. Edited to clarify this.
10-698	10	13	40			So the collateral of this decision is that we appear to be more confident about 100 years ago and even about some purely hypothetical natural climate than about the one that we have been observing with every tool known to humanity for the past few decades. This needs to be explained. [Dáithí Stone, United States of America]	Rejected. Our focus here is on representing the forced components of temperature change, which is why we choose a base period close to preinudstrial. Observational unceratinty is not our focus here. This is covered in detail in chapter 2. For example, Fig 2.21 compares multiple observaional dataset's global mean surface temperature anomaly relative to a 1961-1990 base period.
10-699	10	13	40			There is a bit of a jump in the middle of the paragraph "anomalies…" [Simon Tett, United Kingdom]	Taken into account. Sentence beginning 'Simulations with greenhouse gas' moved to after sentence beginning 'Showing anomalies' to improve flow.
10-700	10	13	42	13	45	Please add a sentence that explains what one is to infer from that fact that some models "overestimate the warming trend, while others underestimate it"? Are these models thereby to be judged unsuitable for either detection, attribution, or projection? Is one to infer that the observed trend is deterministic in forcing, and thus a difference between any particular model simulated trend (in fully forced runs) and observed is indicative of model bias, or forcing bias? Some clarification would be rnedered if the assessment could provide a figure of the simulated global mean temperatures (perhaps as an inset to Fig, 10.1a of Fig, 10.1c) of an ensemble drawn for a single model. This would give some, albeit not definitive, appraisal of how deterministic the observed time series of global mean temeprature is in the forcing. [Martin Hoerling, United States of America]	Taken into account. This issue is already discussed in more detail later in section 10.3.1.1.3. We have added additional text here discussing the implications for detection and attribution analyses of such inconsistencies.
10-701	10	13	43	13	44	Delete "and CMIP5" because you have already presented these; and change "grey" to "blue". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Suggested change made.

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10-702	10	13	43	13	44	Delete "and CMIP5" (CMIP5 was discussed near the beginning of this paragraph). [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-703	10	13	44	13	44	Grey lines ? [Aurélien Ribes, France]	Noted. Corrected to thin blue lines in Fig 10.1.
10-704	10	13	53			Temperature estimates from a JMA analysis are included in Fig. 10.1, but the JMA analysis is not discussed in Chapter 2, where the reference is to the Met Office, NCDC and GISS products (as used also in Fig. 10.1) but also to an analysis by the Berkeley group. [Adrian Simmons, United Kingdom]	Taken into account. JMA analysis has been removed from this figure.
10-705	10	13	56	13	57	Delete the sentence defining the thin lines because you have already defined them. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Suggested change made.
10-706	10	13	57			A reason should be given for why the HadCRUT4 data mask was used. [Chris Forest, United States of America]	Taken into account. Justification is now given - HadCRUT4 has the most restricted coverage.
10-707	10	13				Fig 10.1.: The graph a) is not consistent with Fig. 9.8., in which there is much less agreement between models and obs for the last decade. Please check this critical inconsistency. [Government of Germany]	Taken into account. This is due to the 1880-1920 base period used here, which is already highlighted in the text. The issue of model-observations agreement in the last 15 years will now be considered in detail in box 9.2. Also in the SOD, in Figure 9.8 models were not masked with observational coverage, although this has been corrected in the next draft.
10-708	10	14	1			*From here on, I did not continue the indication of misprints and similar wording „nuisances", but apparently, there is a need for such a check .. (let me mention one more by random from the latter part of text: page 61, line 5: „of the of the" [Tibor Farago, Hungary]	Editorial. The chapters will be carefully copy-edited before publication.
10-709	10	14	10	14	11	Why did CMIP3 models with larger sulfate aerosols demonstrate higher sensitivity? If known, the reason should be mentioned here. [Government of United States of America]	Taken into account. The reason is not known, though this has been taken as evidence that the sulphate forcing was selected/tuned in order to give better agreement with 20th century temperature evolution. Still this has not been found in CMIP5. We have re-phrased the sentence to make the implication a bit clearer, but we do not wish to add extensive discussion, since this is not an issue for CMIP5 on which this assessment mainly focuses.
10-710	10	14	11	14	12	Delete "there is" at the end of line 11, and insert "is" after "relationship" on line 12. [Francis Zwiers, Canada]	Editorial. Change similar to that suggested made.
10-711	10	14	13	14	17	It may be literally true that climate model parameters are typically chosen primarily to reproduce features of the mean climate and variability. However, as stated in Mauritsen et al (2012):Tuning the climate of a global model, Journal of Advances in Modeling Earth Systems, doi:10.1029/2012MS000154: "Climate models ability to simulate the 20th century temperature increase with fidelity has become something of a show-stopper as a model unable to reproduce the 20th century would probably not see publication, and as such it has effectively lost its purpose as a model quality measure." [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. No significant relationship bewteen aerosol forcing and climate sensitivity was found in CMIP5, so this point is moot. Also, as we note in the text, the spread in 20th century warming is wider in CMIP5 than in CMIP3, and we also note that some models simulate significantly more warming than observed while others simulate less.
10-712	10	14	13	14	17	Mauritsen et al (2012) also state: "Most other observational datasets sooner or later meet the same destiny, at least beyond the first time they are applied for model evaluation. That is not to say that climate models can be readily adapted to fit any dataset, but once aware of the data we will compare with model output and invariably make decisions in the model development on the basis of the results". Therefore, models undergo a form of evolution and, just as with evolution in the natural world, the result is a product that is very well adapted to satisfy whatever tests it has to undergo, without any conscious direct tuning of its characteristics. This point should be made. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejceted. Model tuning is discussed extensively in Box 9.1, to which we refer here. Box 9.1 cites and discusses Mauritsen et al. (2012) as well as other relevant studies.
10-713	10	14	17			I think this statement gives Curry and Webster too much credance. They made the claim that aerosols were tuned to get the 20th century correct in CMIP3 simulations. Though possible I think at the time it would be difficult for modelling groups to do that and my impression is that groups could just about run thier models once... Curry and Webster made the claim but show no evidence in thier paper that this tunign took place.	Rejected. We wish to retain the reference to Curry and Webster here, since this is a published criticism of previous detection and attribution studies. Note that we are arguing against their conclusions here and we

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						[Simon Tett, United Kingdom]	also cite the Hegerl et al response.
10-714	10	14	19	14	19	Suggest inserting "surface" after "pattern of". [Francis Zwiers, Canada]	Editorial. Suggested change made.
10-715	10	14	19	14	20	This statement dishonestly mixes a period of warming that IPCC climate modellers claim was due to natural forces with a period, of similar rate of warming that IPCC modellers claim is man-made. The inconsistency in the periods chosen for analysis is deplorable and unprofessional. [John McLean, Australia]	Rejected. The periods were chosen in a cross-chapter meeting with chapter 2 and others based on the availability of data. There was no selection bias of the kind implied by the reviewer.
10-716	10	14	19	14	20	This statement dishonestly mixes data from a period of low global data coverage with a period of higher data coverage. Why haven't you told the readers the extent of global data coverage so they can draw there own conclusions? [John McLean, Australia]	Rejected. Coverage is shown in Figure 10.2. Trends are only plotted where no more than 5 consecutive years have missing data. This is explained in Jones et al. (2013) which we cite.
10-717	10	14	19	14	32	Nicely articulated summary of essential features. [Martin Hoerling, United States of America]	Noted. Thanks
10-718	10	14	20	14	20	Revise to read "Warming has been observed over almost all areas having observational records since 1901……" [Martin Hoerling, United States of America]	Accepted. A change similar to that proposed was made.
10-719	10	14	20	14	21	omit "with the exception of a few regions" (or "almost") [John Mitchell, United Kingdom]	Accepted. Suggested change made.
10-720	10	14	21	14	21	Their are big gaps in data over Africa, South America and Asia before 1950 which mean it is not obvious the land is warmer- clearer after 1950 [John Mitchell, United Kingdom]	Taken into account. A reference to the 1951-2010 period has been added.
10-721	10	14	23	14	25	This statement and graph have no credibility unless it can be shown that climate models accurately encompass all natural forces, and I see no evidence of that anywhere. [John McLean, Australia]	Rejected. Natural forcings are assessed in Section 8.4. Chapter 9 discuss the effect of volcanic forcing on stratospheric temperature. Figure 9.33b compares CMIP5 simulations of the past 1000 years with recoinstructions. Natural forcings are dominant drivers through most of these simulations. Simulated variability is as high or higher than reconstructions on periods less than 1000 years.
10-722	10	14	26	14	28	The large differences between observations and simulations could be explained in certain extent with the observational bias. For instance, the larger warming in observations in mid-to north Asia are consistent with the findings that urban warming detected for the commonly used datasets accounts for a significant proportion of the overall warming in mainland China, Korea and Japan (Ren, G., Yaqing Zhou, Ziying Chu, Jiangxing Zhou, Aiying Zhang, Jun Guo and Xuefeng Liu, 2008, Urbanization effect on observed surface air temperature trend in North China, Journal of Climate, 21(6), 1333–1348; Zhang, A. Y., G. Y. Ren, J. X. Zhou, et al. 2010. Urbanization effect on surface air temperature trends over China. Acta Meteorologica Sinica, 68(6): 957-966(in Chinese); Chung U, Choi J, Yun J I. 2004. Urbanization effect on observed change in mean monthly temperature between 1951-1980 and 1971-2000 in Korea. Climate Change, 66(1-2): 127-136; Chung et al., 2004; Fujibe, F. 2009. Detection of urban warming in recent temperature trends in Japan, Int. J. Climatol., 29, 1811–1822, doi:10.1002/joc.1822). In mainland China on a whole, the proportion reaches at least 27% (Zhang, A. Y., G. Y. Ren, J. X. Zhou, et al. 2010. Urbanization effect on surface air temperature trends over China. Acta Meteorologica Sinica, 68(6): 957-966(in Chinese); Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977). [Guoyu Ren, China]	Taken into account. We have added a comment to this effect. Note that we have not cited the references mentioned here, since the assessment of observational biases belongs in chapte 2, to which we refer here.
10-723	10	14	42	14	43	If one were to compare only the common areas of analysis for the 2 periods (1901-2010) vs (1979-2010) then there are quite substantial regional differenes in magnitudes, and some areas in sign. I suggest therefore a revision of this sentence which currently begins by stating the patterns are similar between the 2 periods. Other differences of note are the lack of appreciable warming in the eastern/southeast US for 1901-2010, while a more substantial warming occurs there in 1979-2010. Western Asia shows very strong warming in 1901-2010, but the same area exhibits a minimum of warming rate in 1979-2010. These in additon to the differences in teh Pacific SST trends. The subsequent text (lines 50-53) are helpful in assist the reader in	Accepted. Revised as suggested. Text no longer states that the trend patterns in the two periods are similar.

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						understanding these differences in trends for long vs short periods. [Martin Hoerling, United States of America]	
10-724	10	14	44	14	45	This statement has no credibility unless it can be shown that climate models accurately encompass all natural forces, and I see no evidence of that anywhere. [John McLean, Australia]	Rejected. See response to 10-721.
10-725	10	14	48	14	53	In the segment 'unusually strong manifestation of internal variability', it should be mentioned that this is about the real climate system's internal variability and not that of climate models, to be clear. [Government of United States of America]	Accepted. Suggested change made.
10-726	10	14	52	14	52	I have noticed in the last couple of years that these figures (i.e. similar figures from AR4) are very convincing to many climate scientists. They are not so convincing though and I will give a number of reasons. First, this outcome (i.e. the observations) was known by the modellers in advance, so it's more a matter of data fitting. John von Neumann once said: "With four parameters I can fit an elephant, and with five I can make him wiggle his trunk." It is well-known by now that the first thing the models are tuned at is the global average surface temperature. Ch. 9 mentions the work of Mauritsen (2012) about the tuning of models. That paper is relevant here as well. It says: "Climate models ability to simulate the 20th century temperature increase with fidelity has become something of a show-stopper as a model unable to reproduce the 20th century would probably not see publication, and as such it has effectively lost its purpose as a model quality measure." Second, although the fit between models and observations look pretty good, as soon as you start to compare the trends in different periods the fit is actually not that good. Models don't simulate the early 20th century warming very well (see e.g. Crook (2012). The recent warming since the 70-ies they do quite well but still, as has been shown on the blog The Blackboard, see http://rankexploits.com/musings/2012/trends-relative-to-models/, the model mean trend is considerable higher than the observed trend. As soon as we leave the global average surface temperature things get worse. Models overestimate the warming of the oceans since 1980 and also the warming of the troposhere. Models simulate a "hot spot" above the tropics which hasn't been observed (see this chapter). If we zoom out even more we discover that models are not able to simulate temperature trends on a continental scale (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.) Models are also not able to simulate very well the global dimming and brightening that has happened in the the period '60-'80 and since the 90-ies respectively. These are huge fluxes compared to the effect of an increasing CO2 concentration. Models have on average precipitation wrong (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.) Models have great difficulty in simulating natural modes of variability like ENSO, PDO and AMO and it is therefore highly questionable that models have internal variability right. This is confirmed by the paper Koutsoyiannis, D., A. Efstratiadis, N. Mamassis, and A. Christofides, 2008: On the credibility of climate predictions, Hydrological Sciences Journal, 53 (4), 671-684. that says: "GCMs do not reproduce natural over-year fluctuations and, generally, underestimate the variance and the Hurst coefficient of the observed series. Even worse, when the GCM time series imply a Hurst coefficient greater than 0.5, this results from a monotonic trend, whereas in historical data the high values of the Hurst coefficient are a result of large-scale over-year fluctuations (i.e. successions of upward and downward 'trends')." Taken together, models have a long way to go before they are ready for prime time and attribution is prime time. [Marcel Crok, The Netherlands]	Taken into account. We include a discussion on the role of model tuning in the second paragraph of 10.3.1.1.2. The main place in the report where model validation is discussed is chapter 9. However, further consideration of the realisism of models' spatial trend patternshas been included in this section, including the consideration of model errors, based on the results of Ribes and Terray (2013).
10-727	10	14	52	14	52	Some models do include sea salt aerosols. For example, this is a feature of the Canadian atmospheric model. [Francis Zwiers, Canada]	Taken into account. We now say 'not included in most CMIP5 simulations'.
10-728	10	14	53	14	55	This statemenrt is blatantly untrue. Changes in the extent of global data coverage and in the ENSO can account for the changes in temperature, the meriodional Hadley Circulation increasing when ENSO conditions are on the El Nino side of absolutely neutral. For details about the ENSO see McLean et (2009), especailly Figure 7, which plots the monthly data. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?). [John McLean, Australia]	Rejected. Changes in coverage do not account for the warming, since anomalies are used to construct the global mean temperature (see also Chapter 2). ENSO has not caused a long-term warming (see e.g. Figure 10.5). McLean (2009) show that ENSO explains interannual variability in the tropospheric temperature, but does not examine the contribution to tropospheric temperature trends (or to surface temperature trends

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							considered here). See alo the response to McLean (2009) by Foster et al. (2010).
10-729	10	15	6	15	6	These are not :"observed" or :"global mean": temperatures but a series of multiple averages of a varying number of means of maximum and minimum tempeture from a variety of weather staions and ship measurements . [Vincent Gray, New Zealand]	Rejected. The reviewer is correct about how estimates of global mean temperature are constructed, but incorrect that this means that they are not good estimates of the global mean. This is discussed in chapter 2.
10-730	10	15	6	15	15	While decadal variability of CMIP5 models is discussed, the interannual variability of CMIP5 models is not discussed (only CMIP3's interannual varaibility is discussed). Is there a reason? If so, it should be mentioned. [Government of United States of America]	Rejected. Interdecadal variability is our focus here, since this is important for detection of external influence on climate. We have deleted the reference to interannual variability in CMIP3, since most of our analysis focuses on CMIP5, and we have added references to chapter 9 on the topic of CMIP5 variability.
10-731	10	15	8	15	8	typo: "and" [Albert Klein Tank, Netherlands]	Editorial. Text now deleted.
10-732	10	15	12	15	15	The observed change in mean global temperature since 1950 is not "very large" compared to internal variability but "very consistent" with the ENSO (which I understand you regard as internal variability). Figure 7 of McLean et al (2009) demonstrated this point. Now unless climate modellers can accurately predict the ENSO state you have no basis whatsover for claiming that a discrepancy exists between model outputs and the observed change in mean global temperature. [John McLean, Australia]	Rejected. See response to 10-728.
10-733	10	15	13	15	13	"very large …" is too vague, be more precise [Laurent Terray, France]	Taken into account. We have revised this sentence and now compare observed warming with simulated natural variability, and refer to Fig 10.1, so the reader can assess the relative size of the warming and the natural variability.
10-734	10	15	19	15	23	.. And McLean et al (2009) demonstrated a clear link between ENSO, which cannot be predicted with any accuracy more than about 12 months ahead, with average global temperature about 7 months into the future. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) Your statement on these lines tries to imply that a single cause is responsible but in fact because of limitations in modelling a second plausible cause has not been thoroughly investigated. Words should be added to this effect. [John McLean, Australia]	Rejected. See response to 10-728.
10-735	10	15	19	15	23	Rather long run-on sentence. [Francis Zwiers, Canada]	Editorial. Shortened and re-written for clarity.
10-736	10	15	19	15	28	These two sentences are very hard-going. I think the problem is that in both sentences, subject and verb are separated by a great many words. Moreover, subject and verb in the first sentence are "studies…support previous studies" and in the second sentence are "results….are shown". This can be made both clearer and stronger in these very long sentences. [Jochem Marotzke, Germany]	Editorial. Both sentences have been edited for clarity.
10-737	10	15	20	15	20	It would be appropriate to include Ribes and Terray (2012) in this list as well. [Francis Zwiers, Canada]	Accepted. Reference added.
10-738	10	15	21	15	21	It's not quite clear to me what is meant here - the sentence seems to suggest that the "new generation of models samples a wider range of …. observational uncertainty". How is that done with models? I think this sentence needs to be crafted with a bit more care. D&A studies are primarily designed to analyse changes in observations - so it seems odd to have a sentence that starts out by saying that the studies are applied to new models (and ultimately only refers to observations in a muddled kind of way). New models provide updated estimates of the expected responses to forcing and of internal variability - but this new information is used to analyse observations. I think it is really important to continually remind the reader that while models are a critical aspect of D&A research, their primary focus is on the observations. [Francis Zwiers, Canada]	Taken into account. We have edited the sentence for clarity, and now no longer refer the 'new generation of models'.

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10-739	10	15	25	19	42	Your statement assumes that all natural forces are accurately simulated in models when this is completely untrue. On this basis your statements cannot be sustained because attribution is likely flawed. [John McLean, Australia]	Rejected. See response to 10-721.
10-740	10	15	26	16	27	see comment N°2 [Laurent Terray, France]	Taken into account. Our conclusions take account of the results of Ribes and Terray (2013).
10-741	10	15	28	15	28	Jones et al., 2012 appears not to be a published paper (and its entry in References lists no journal), but is not stated only to have been submitted. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted. Jones et al. has now been accepted for publication. (It was submitted at the time of preparation of the SOD, as required).
10-742	10	15	28	15	28	Add Ribes and Terray 2012 to the references. I suggest to add the two-signal (ANT and NAT) results from Ribes and Terray 2012 to figure 10.4 [Laurent Terray, France]	Taken into account. A reference to Ribes and Terray has been added . Two signal Ant vs NAT results have been added to Fig 10.4.
10-743	10	15	31	15	31	Replace "the disadvantage" with "while a disadvantage". There is also the problem of dimension reduction - a longer time period using the same dimension reduction (i.e., to the same number of EOFs) would inevitably lead to an analysis that is more restricted to the time dimension. Also, estimating the covariance structure becomes more difficult since the length of available control runs (and sizes of forced ensembles) presumably does not change. [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-744	10	15	32	15	32	"Verify climate models estimates of internal variability" is not truly appropriate here as one does not have observational estimate of internal variability. [Laurent Terray, France]	Taken into account. We now say 'difficult to validate' instead of 'more difficult to verify'. We agree with the review comment. Paleo observations could conceivably be used to do this. See Fig 9.33b.
10-745	10	15	33	15	33	Remove the subjective words "broadly consistent with" and replace with an objective quantified comparison. This is supposed to be a scientific report. [John McLean, Australia]	Taken into account. This text has now been deleted. We now no longer describe individual model results as broadly consistent with the multi-model mean results.
10-746	10	15	34	15	34	… lending confidence… - is this the proper word? [Helga Nitsche, Germany]	Taken into account. Text now deleted.
10-747	10	15	36	15	39	Revision is needed. From the information provided, it appears that the observed warming falls within the 5%-95% confidence interval of the GHG simulated warming. One cannot conclude, as this sentence appears to do, that the GHG attributable warming is significanrly larger than the observed warming. Rather, the observed warming is seen to be consistent with the range of possible warmings simulated in response to GHG forcing alone. One can perhaps speak about this in a probabilistic manner, but the current sentence structure is inappropriately deterministic in its language usage. Similarly, nor is it quite correct to state that the GHG warming effect is compensated by an aersol-induced cooling, since there is a probability (though evidently low) that the aerosol induced cooling is 0. [Martin Hoerling, United States of America]	Taken into account. This text has been revised to reflect a new assessed range of GHG-attributable warming based on new evidence.
10-748	10	15	36	15	39	The method used in Jones et al (2012) to estimate the 0.6 - 1.4K range should be explicitly presented in the text. Some discussion on the uncertainty on the range itself should also be added (is it sensitive to the truncation used in the D&A methodology ? is it dependent on the climate models used to define the signal patterns ?). The same applies for the -0.8 - 0K range for the aerosol induced cooling. [Serge PLANTON, France]	Taken into account. This text has been revised and a more complete description of how the uncertainty ranage is derived has been included.
10-749	10	15	36	15	39	The computation of the confidence interval of the GHG attributable warming (resp. aerosol-induced cooling) does not seem to me to be enough described. Based on Jones et al., 2012, the .6-1.4K interval is roughly provided by the two multi-model analyses (Simple avg and Weighted avg, Fig 19). Based on these multi-model patterns, the .6-1.4K interval would be the confidence interval obtained by considering no uncertainty from the method or from the patterns. The sensitivity of this result to the truncation is lower than for individual models, but variations in the GHG scaling factor (only due to the choice of the truncation, Fig 17 from Jones et al.) seem sufficient to increase this confidence interval substantially (eg with truncation 35). Then, if one wants to take into account some uncertainty in the patterns, one option is to look at the bounds provided by single model analysis, and this would provide a much wider confidence interval (several lower bounds under .6K, several upper bounds higher than 1.4K). Jones et al., 2012, also mention that most "best estimates" are	Taken into account. This has been revised and a description of how the confidence range is derived has been added.

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						between .6 and 1.4K, but best estimates do not take into account internal variability. Jones et al., 2012, also provide some cautious conclusions (l867-875), and stated that "For the period 1951-2010 [...] we find a wider range of attributed greenhouse warming across a variety of models than assessed in [the AR4] for the 1950-1999 period". This finding is quite consistent with Ribes and Terray, 2012. Finally, I don't know if such a narrow interval may be obtained based on observational contraint only (at least I believe it would have to be better justified). Similarly, still based on observatinal constraint, it is maybe not so clear why the GHG contribution is considered to be "very likely" between .6K and 1.4K, whereas it was only "likely" higher than the observed warming in the AR4. Note that another option could be to assess this GHG (resp aerosols) contribution from historicalGHG simulations, with no observational constraint. The spread from model simulation obtain in this simple way would be potentially close to .6-1.4K (as Fig 10.1c suggests). [Aurélien Ribes, France]	
10-750	10	15	36	15	39	see commentN°2, it also applies indeed to the estimated uncertainty range for aerosol + other ant. Forcings [Laurent Terray, France]	Taken into account. This has been revised.
10-751	10	15	36	15	42	These statements are unsustainable because models do not accurately simulate all natural forces. If natural forces were accurately included the conclusions might be quite different. [John McLean, Australia]	Rejected. See response to 10-721.
10-752	10	15	37	15	37	Is "significantly" really meant in the statistical sense? If so, the significance appears weak, given overlapping ranges. Or should it read "substantially"? [Jochem Marotzke, Germany]	Taken into account. This text has been revised.
10-753	10	15	37	15	37	Replace "warming at" with "warming, which is estimated to be", insert a comma after "K" in order to emphasize that this is an estimated range. Also replace "significantly" with "substantially" to avoid making allusion to statistical significance (unless that was specifically intended). [Francis Zwiers, Canada]	Taken into account. This text has been revised.
10-754	10	15	37			How can warming of 0.6-1.4K be "significantly larger" than warming of 0.6K? [Peter Guttorp, United States of America]	Taken into account. This text has been revised.
10-755	10	15	38	15	39	a cooling between 0 and -0.8 -> a cooling from zero to 0.8K [Simon Tett, United Kingdom]	Taken into account. This text has been revised.
10-756	10	15	40			These authors used an EMIC, not a simple model. [Reto Knutti, Switzerland]	Accpted. We now say that these authors used an EMIC.
10-757	10	15	44	15	44	Please explain how the absence of warming over the last 15 years has helped to better constrain the magnitude of greenhouse gas driven warming. With the absence of warming one would have to conclude that greenhouse gases made a very minor or non-existent contribution. [John McLean, Australia]	Rejected. This is explained in the cited studies. Of course the better constraint refers partly to a lower constraint on the upper bound of GHG-attributable warming.
10-758	10	15	44	15	51	This paragraph needs to be a bit more careful to use assessment language where appropriate (as opposed to language that would be used in a review). For example, at lines 46-47, the text says "... Hegerl et al (2007b) found ...". That's not quite appropriate because they didn't find, themselves - they assessed what was reported in the literature. On line 51, I would adopt assessment language by deleting, on both occasions "found to be", so that the sentence reflects what is, presumably, your assessment - that detection is robust to observational uncertainty. [Francis Zwiers, Canada]	Accepted. Suggested changes made.
10-759	10	15	46	15	50	McLean et al (2009) shows the primary driver of temperature to be the ENSO. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) Its Figure 7(a),(b) and(c), which are of monthly data rather than derivatives, show that mean global lower tropospheric temperature is closely linked to the ENSO conditions about 7 months earlier. This implies that natural climate forces have a far greater influence on temperature than you give them credit for. [John McLean, Australia]	Rejected. See response to 10-728.
10-760	10	15	48	15	50	It may be mentioned that in 2008 the eruptions of Mt. Okmuk and Mt. Kasatochi have contributed to the stratospheric aerosol layers, see Schmale et al., 2010, although Kravitz et al, 2010, think that their climatic effects my be negligible. References: Schmale, J., and 13 co-authors, 2010: Aerosol layers from the 2008 eruptions of Mount Okmuk and Mount Kasatochi: In situ upper troposphere and lower stratosphere measurements of sulfate and organics over Europe. J. Geophys. Res., 115, doi: 10.1029/2009JD013628 (18	Rejected. These are relevant to the discussion of trends over the past decade, but not to the discussion here which focuses on CMIP5 simulations of the response to natural forcings - these volcanoes weren't included in the models.

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						pages); Kravitz, B., Robock, A., Bourassa, A., 2010: Negligible climatic effects from the 2008 Okmok and Kasatochi volcanic eruptions. J. Geophys. Res., 115, doi: 10.1029/2009/JD013525 (16 pages). [Christian-D. Schoenwiese, Germany]	
10-761	10	15	49	15	50	This text appears to ignore or downplay the recent papers by Solomon and colleagues and another group which escapes me right now on the impacts of the series of volcanic eruptions in mid-latitudes yielding an elevated stratospheric aerosol loading in the last decade. [Peter Thorne, United States of America]	Rejected. The eruptions weren't in the CMIP5 simulations.
10-762	10	15	49			It is stated that "Pinatubo cooled the 1990s", but it might be better to state that "Pinatubo cooled the early 1990s". The e-folding time for the stratospheric aerosols from an eruption such as Pinatubo is stated in Chapter 8 to be about a year, and examination of the tropospheric temperature record shows a clear cooling signal only in 1992 and 1993. Perhaps 1994 also, but one then begins to run into separating out effects of internal variability. Maybe there was some residual weak cooling for longer due to the thermal inertia of the ocean, but if there's quantitative evidence for this it should be quoted. [Adrian Simmons, United Kingdom]	Accepted. 'early' inserted before 1990s.
10-763	10	15	51	10	51	Does this mean that obs uncertainty is an effect about 0.1 deg C? This will vary depending on the period. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. This doesn't necessarily follow. The impact on the attributable warming will depend on the spatial pattern of the uncertainties and its covariance. This is base on the spread of regression coefficients shown in Fig 10.4e.
10-764	10	15	53	15	55	The results reported in Ribes and Terray, 2012, were also suggesting some sensitivity of the results, in particular in a 3-forcing analysis, in order to discriminate between GHG vs other anthropogenic forcings. [Aurélien Ribes, France]	Taken into account. This is now discussed in the previous paragraph.
10-765	10	15	53	15	57	I would suggest again to show some results from Ribes et Terray 2012 on figure 10.4 and include a bit more detailed discussion pointing out the consistency between the different papers. As mentioned in comment 2, it is needed to know exactly how the GHG (and other ANT.) contribution uncertainty range is estimated. [Laurent Terray, France]	Taken into account. Ribes and Terray 3-forcing analysis is now discussed in the previous paragraph. More information on how the confidence ranges are derived has been included.
10-766	10	16	1	16	5	The summary of McKitrick and Tole, 2012 is on climate models is incorrect and opposite their actual finding. Please correct lines 2-5 to read: "from 1979-2002. McKitrick and Tole (2012) find that spatial patterns of temperature trends over 1979-2002 strongly depends on socioeconomic development, while climate models were no better than or were worse than random numbers in describing those trends." [David L. Hagen, United States of America]	Taken into account. Discussion on this topic has now been deferred to chapter 2, where the influence of non-climatic factors on the surface temperature record is considered in more detail.
10-767	10	16	11	16	11	Which CSIRO model? [Francis Zwiers, Canada]	Taken into account. CSIRO-Mk-3-6-0. This is now stated in the text.
10-768	10	16	13	16	17	ENSO does not have three distinct states but a continuum of states. Any attempt to "compensate" for ENSO conditions must therefore take into account the entire range of states, but as McLean et al (2009) showed, there is a consistent link between ENSO and subsequent mean global temperatures across the entire spectrum of ENSO states. [John McLean, Australia]	Noted. Fyfe et al. (2010) do not assume three distinct states, but remove ENSO influence through a regression on NINO 3.4 SST.
10-769	10	16	16			Figure 10.4 - scaling factors need to be better explained. [European Union]	Taken into account. Caption to Fig 10.4 re-written to better explain scaling factors.
10-770	10	16	17	16	19	Ribes et al argue that the use of a regularized (full dimension) covariance matrix estimate should help in this regard as well - which is a point that I now agree with having had an indepth discussion recently with Aurelean. So I think it would be useful to discuss that point here as well. [Francis Zwiers, Canada]	Taken into account. We have included Ribes and Terray results in Fig 10.4 and have added more discussion of their results earlier in this section.
10-771	10	16	21	16	26	Your statement is refuted by McLean et all (2009) in which it was shown that temperatures are in fact quite consistent with natural forces, moreover the findings are supported by numerous papers and accepted knowledge about the variations in Hadley Circulation and Walker Circulation. McLean et al (2009) has the advantage of not requiring the use of models, which is important given that climate models do not accurate portray all natural climate forces. [John McLean, Australia]	Rejected. See response to 10-728.
10-772	10	16	21	16	27	This summary paragraph claims that the spatial patterns of warming from models forced with GHG's and other	Taken into account. We have revised the discussion

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						anthropogenic forcings agrees well with observations. But the underlying text (p. 10.14) provides no statistical tests to support this claim. All it gives is an eyeball comparison of spatial colour maps for the 1901-2010 intervals, and later on the same page notes that the similarity is not as good for the 1979-2010 interval, with evidence of model over-prediction of warming in a number of areas. Statistical comparisons are not provided: readers do not even get a correlation coefficient, let alone a significance test. Nor is any such information given in the one paper cited (Sedlacek and Knutti 2012, which isn't really on point here). For the 1979-2002 interval, extensive statistical tests are provided in McKitrick and Tole (2012, cell 34). Looking at their Table 3, only 2 out of 22 CMIP3 climate models have significant explanatory power for the spatial pattern of warming trends over land, and the rest have no significant explanatory power or are even anticorrelated with the observed trends. This finding emerges whether the models are tested individually, all at once, or in any linear combination. The Sedlacek and Knutti paper is only about oceanic temperatures, not the land record, it shows that the models do a poor job matching observed oceanic changes over the 20th century when relying only on natural forcing, and that if the natural-only runs are scaled to have an overall trend that matches the observations, the models predict a more heterogeneous distribution of trends than was observed. It's an interesting enough paper, but the argument ultimately depends on the premise that the model is fundamentally correct, so if the natural-only control run doesn't look like the real world, then the natural-only assumption must be wrong. In other words, the paper assumes the spatial validity of the models, so it cannot simultaneously be cited as evidence in support of the same assertion, otherwise you are begging the question. Consequently, if you are going to make a summary statement that the models are able to simulate correctly the spatial pattern of trends (especially after 1979), you need to find some published support, and you also need to address the counter-evidence in McKitrick and Tole 2010. [Ross McKitrick, Canada]	of McKitrick and Tole (2012) to explain that the models are tested against an alternate hypothesis in which observed lower tropospheric temperature trends are included as an explanatory variable. The Sedlacek and Knutti reference has been moved to later in the section where non-regression based methods are discussed. We have edited the text to mention that some inconsistencies between simulated and observed forced responses have been identified in some studies.
10-773	10	16	25	15	25	These are not :"observed" or :"global mean": temperatures but a series of multiple averages of a varying number of means of maximum and minimum tempeture from a variety of weather staions and ship measurements . [Vincent Gray, New Zealand]	Rejected. We are not sure what text this refers to, since the end line number is before the start line number, and observations aren't mentioned on pg 16, ln 25.
10-774	10	16	26	16	27	This sentence has multiple possible interpretations. [Dáithí Stone, United States of America]	Taken into account. This statement has been revised.
10-775	10	16	26	16	27	see comment N°2 [Laurent Terray, France]	Taken into account. This statement has been revised.
10-776	10	16	30	16	38	See comment 286 concerning the inclusion of JMA results, which appear in Fig 10.4 as well as Fig. 10.1. Also, are the HadCRUT2v and HadCRUT3v datasets sufficiently different to justify the inclusion of both? Would it not be better to replace them by the median values from the newer HadCRUT4 dataset? [Adrian Simmons, United Kingdom]	Taken into account. JMA results have been removed from this figure. For the right hand panels we retain the results published in Jones and Stott (2011), rather than adding new analysis.
10-777	10	16	38	16	38	The horizontal grey observational lines in panels b.d.f should be defined in the caption [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Suggested change made.
10-778	10	16	40	16	43	McLean et al (2009) showed very little change in temperature that remained to be attributed to other forces therefore claims about black carbon aerosols and greenhouse gas contributions are superfluous. [John McLean, Australia]	Rejected. See response to 10-728.
10-779	10	16	46	17	6	Your summary of McKitrick and Tole (2012) is incorrect on two points. First, we didn't just apply BMA, we used two other methods as well, namely encompassing tests and non-nested regressions. Second, we didn't apply the method to both surface and lower tropospheric temperatures, we only looked at the surface patterns, though we used the LT series as a control. You dismiss the findings by stating that in Chapter 2, socioeconomic activity is not assessed to be a major issue for the land data. The use of the passive voice here is noteworthy, since you don't have any published citations to support your position. The Chapter 2 material is more subtle than you make it out to be. They do not overturn any of the evidence of surface data contamination and they note that the disputes are unresolved. Their claim that the problem is relatively small (< 10%) is simply made up at the end of the discussion. So you are compounding the problem by citing their conjecture as evidence for your assumption. I suppose it would complete the circle if the Chapter 2 authors cited your assumption as evidence for their conjecture! [Ross McKitrick, Canada]	Taken into account. We have revised thediscussion of McKitrick and Tole (2012) here.
10-780	10	16	46	17	6	It is also important to note that the findings of McKitrick and Tole that are relevant for this discussion are not dependent on what you make of the role of the socioeconomic variables in that analysis. The encompassing	See response to comment 10-772.

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						tests show, with unambiguous clarity, that the explanatory power of the socioeconomic variables is independent of the explanatory power of the climate models. There is no sense in which the explanatory power of the socioeconomic data could be reduced to a spurious effect properly attributable to climatic processes represented in the climate models. They are completely orthogonal to each other: the p-values on this matter are all on the order of 5e-6 and smaller; see Table 4 and cross that escape route off the list. The BMA analysis also allows each group of variables to be considered independently of the others. So for your purposes, the takeaway message is that you can look only at the results pertaining to the GCMs and ignore everything else if you like, and you won't be misinterpreting anything. And the findings in M&T are that only 2 of the 22 GCMs have significant explanatory power for the surface trend pattern, and in the Bayesian sense only 3 have a posterior probability above 20% of belonging in the correct model of the surface temperature trend pattern. So your quick, offhand treatment of the paper, in the context of a chapter that depends heavily on the assumption that the models get the spatial pattern of warming over land correct, doesn't look very sound. [Ross McKitrick, Canada]	
10-781	10	16	47	16	50	Drost's claims are comprehensively refuted by McLean et al (2009) which showed that the ENSO, which the IPCC consistently but falsely seems to regard as merely internal variability, accounts for virtually all of the temperature variation in temperature since 1960. The small amount of temperature variation that the ENSO does not account for can for the most part be attributed to the cooling influence of volcanic eruptions in the tropical Pacific. [John McLean, Australia]	Rejected. See response to 10-728.
10-782	10	16	51	16	52	McLean et al (2009) likewise refutes Drost and Karoly (2012). (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) [John McLean, Australia]	Rejected. See response to 10-728.
10-783	10	16	54	16	56	McLean et al (2009) refutes Zorita et al and shows that the sustained dominance of the El Nino side of absolutely neutral (ie. SOI = 0) can indeed account for the sustained elevated temperatures. [John McLean, Australia]	Rejected. See response to 10-728.
10-784	10	16	57	17	1	McLean et all (2009) refutes Smirnov and Mokhov (2009) by showing that the dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. SOI=0) accounts for the observed variation in temperature and that any other warming is negligible or non-existent, and therefore does not require some other force such as greenhouse gases. [John McLean, Australia]	Rejected. See response to 10-728.
10-785	10	16	58	16	58	"...are the principal determining…" [James Renwick, New Zealand]	Accepted. Correction made.
10-786	10	16	58	17	5	What is the releavnce of these sentences? All this paper is looking at is the period 1979-2002. It also uses NCEP. Results could be quite different if ERA-Interim was used. I'd remove this point and leave it to 2.4.1.3 to deal with. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We have revised the discussion of McKitrick and Tole (2012).
10-787	10	16				Figure 10.4 : Given the sensitivity of results to choices in the method (such as the truncation in Jones et al., 2012, or the spatial resolution, in Ribes and Terray, 2012), I sometimes wonder whether the presentation of the results obtained in a 3-forcing analysis with individual models is required. At least one could wish this sentivity to be discussed at some point. As the results obtained in a 2-forcing analysis seem to be much more robust, with a clear identification of the ANT influence with each individual model (at least, in Ribes and Terray, 2012, as long as the spatial resolution is low), why not illustrating also these 2-forcing results ? Another minor comment about this figure: in b), the horizontal grey line indicating the observed warming seems to be exactly the same over both the 1951-2010 and the 1861-2010 periods. Is that correct ? [Aurélien Ribes, France]	Taken into account. While we prefer to keep the 3-forcing results in Fig 10.4, since these underly our assessment of the contribution of GHGs and other forcings to observed trends, we have also included the ANT vs NAT regression results in this figure.
10-788	10	17	1	17	1	"McKitrick and Tole (2012) apply…" - and check other EndNote citation formatting [James Renwick, New Zealand]	Editorial. References will be checked before final version is submitted.
10-789	10	17	1	17	5	There have already been some repetitions from Ch 2 and I have also seen more from Ch 5. This Chapter is already way over length. These are areas where reductions could take place. I read Ch 10 after Ch 5 and there are quite a few repetitions. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We have deleted this text and will defer discussion of the influence of socioeconomic factors on observed trends to chapter 2.
10-790	10	17	1	17	5	I'm very unclear what this is trying to say. Can it be rewritten in clearer language. [Simon Tett, United	Taken into account. Text now deleted.

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						Kingdom]	
10-791	10	17	1	17	5	"Data contamination" in what sense - I think this needs a critical appraisal. [Francis Zwiers, Canada]	Taken into account. Discussion of this topic has now been deferred to chapter 2.
10-792	10	17	2	17	5	See earlier comment for page 13, lines 28-29. Why are regional socioeconomic variations treated as contamination rather than forcings in the context of D/A? Seems inconsistent with LULC change mentioned as forcing on page 20, lines 1-2. [Albert Klein Tank, Netherlands]	Rejected. See response to 10-684.
10-793	10	17	3	17	3	what does "contamination by socio economic variations " mean? [John Mitchell, United Kingdom]	Taken into account. Discussion of this topic has now been deferred to chapter 2.
10-794	10	17	7	17	10	McLean et al (2009) showed that the ENSO, which the IPCC falsely refers to as internal variability, accounts for the observed variations in temperature. [John McLean, Australia]	Rejected. See response to 10-728.
10-795	10	17	7	17	21	In general, I wouldn't think that confounding with the AMO would not be an issue for space-time fingerprinting (or even time-only fingerprinting) approaches provided models simulate AMO like variability reasonably well. Under that assumption, the variability induced by the AMO would be reflected in the covariance matrix of internal variability - and thus the amplitude of it's confounding effect would be reflected correctly in the uncertainty bands for the scaling factors. This same argument holds for other sources of multi-decadal internal variability, and for the possibility of long-memory behaviour. A strength of the space-time fingerprinting approach is that no assumptions need to be made about the nature of internal variability - except, of course, that it is well simulated by models. [Francis Zwiers, Canada]	Accepted. We have added a sentence along the lines proposed to this section.
10-796	10	17	10	17	12	This apparent periodicity in the AMO disappears in reconstructions of it before the late 19th century. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. We think the lack of robustness of the 70 year period is already clear from our discussion here. We also cite 14.6.7 where changes in the AMO over longer timescales are discussed.
10-797	10	17	10	17	42	More attention should be given to the possible influence of AMO, NAO and PDO on global and regional average temperature changes. Country-average annual mean surface air temperature in China also witnesses an approximate 60-70-year oscillation with 1920s-40s and 1980s-2000s being the two distinct warm periods (Tang, G. L., and G. Y. Ren, 2005: Reanalysis of surface air temperature change of the last 100 years over China. Climatic and Environmental Research, 10, 791-798. (in Chinese); Zhou, T. J., and R. C. Yu, 2006: Twentieth century surface air temperature over China and the globe simulated by coupled climate models. J. Climate, 19, 5843-5858; Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q. and Zhang, J., 2007, Detection, causes and projection of climate change over China: an overview of recent progress, Advance in Atmospheric Sciences, 24 (6), 954-971). The obvious multi-decadal variation also occurred in Arctic regions. It is also worth noting that the recent stagnation in surface temperature may be consistent with the influence of the above mentioned oscillations. [Guoyu Ren, China]	Taken into account. Our focus in the chapter is mainly on attributing observed changes to forcings. Climate modes are considered here as possible confounding influences: Documenting the contribution of all climate modes to all regional trends is beyond the scope of our chapter. Nonetheless, we have added a comment that the AMO has been invoked as an explanation of the recent slowdown in warming.
10-798	10	17	17	17	18	One could say, with even more justification, that because modelling of the ENSO is poor, climate models could mistakenly attribute ENSO-driven temperature to CO2, particularly across 1977 to 1997. [John McLean, Australia]	Rejected. Simulation of ENSO in the CMIP5 models is reasonably good - see 9.5.3.4.1. Also because it is mainly an interannual mode it will not be a strong confounding influence on multidecadal timescales.
10-799	10	17	20	17	26	The increase in Hadley Circulation, entirely consistent with ENSO conditions since 1977 averaging on the El Nino side of absolutely neutral (ie. SOI = 0), can account for the observational data. (I notice that you make no mention of observation data in this sentence but only claims based on the output of models, which begs the question of why you failed to put your statement into its real-world context.) [John McLean, Australia]	Taken into account. We disagree that ENSO can account for the observed pattern of warming. We have added a reference to the observed pattern of warming here in response to this comment.
10-800	10	17	28	17	36	This is an important point, but it is also important that anhtropogenic forcing could be working to increase SSTs in the North Atlantic. So AMO contributes to climnate variability but some will be natural and some could be forced by external factors. I'm saying it's not just aerosols that could alter the AMO. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. GHGs have been proposed as a driver of the warming trend but not the interdecadal variability. We cite anthropogenic forcing as a driver of the warming trend in the North Atlantic in the previous paragraph.
10-801	10	17	28	17	36	see and possibly cite Terray 2012 (exact reference in comment N°7)for a multimodel CMIP5 analysis of these issues including a multimodel estimate of the contribution of internal variability along the 20th century to	Taken into account. Terray (2012) is now cited here.

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						Atlantic multidecadal variability. [Laurent Terray, France]	
10-802	10	17	28	17	36	This paragraph feels very convoluted and also unduly confrointational / judgemental of the work being discussed in a way not necessarilly backed up in the discussion. Some work to clarify and improve the tone of this discussion would improve this text considerably. [Peter Thorne, United States of America]	Taken into account. This paragraph has been edited considerably.
10-803	10	17	31	17	36	Text on the AMO is partially repeated, partially contradicted on following page (p. 18, l. 40-43). [Jochem Marotzke, Germany]	Taken into account. We have moderated the language on pg 18 to be in better agreement with that used here.
10-804	10	17	31	17	36	Long run-on sentence. [Francis Zwiers, Canada]	Editorial. This sentence has now been divided into two.
10-805	10	17	33	17	35	"claiming that" has negative connotations. Explain implied failings (as with other examples in this chapter)? Does the clause "this model has been shown to be an outlier.." refer to Booth et al (2012a) only, or to all of the cited examples? [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We now no longer use 'claiming that' and use 'find that' instead. The sentence has been rewritten to make it clear that the caveat about the model being an outlier refers specifically to Booth et al.
10-806	10	17	35	17	36	The citation is wrong, Zhang et al 2012 doesn't show the model used in Booth et al. 2012 is an outlier. In stead, [Rong Zhang, United States of America]	Taken into account.We have revised the text here and now point out that Zhang et al. find discrepancies between HadGEM2-ES and the observations, not with the other models.
10-807	10	17	35	17	36	please add the following: Zhang et al. 2012 shows that the simulations used in Booth et al. 2012 have important [Rong Zhang, United States of America]	Taken into account. See response to 10-806.
10-808	10	17	35	17	36	discrepancies with many observed changes in the North Atlantic. The discrepancies are strongly influenced by [Rong Zhang, United States of America]	Taken into account. See response to 10-806.
10-809	10	17	35	17	36	aerosols, and cast considerable doubt on the claim that aerosols drive the bulk of the Atlantic multidecadal variability. [Rong Zhang, United States of America]	Taken into account. See response to 10-806.
10-810	10	17	38	17	47	It doesn't seem to have crossed your mind that HadCRUT4 data may be wrong or particularly distorted. The fact that since 1990 the temperature anomaly for February in eastern Russia is consistently positive and quite different to January and March suggests that the HadCRUT4 data may be flawed. If that data is flawed then one must question the output of climate models that claim to replicate HadCRUT4 data. [John McLean, Australia]	Rejected. The quality of observational datasets is assessed in chapter 2 and we use their assessment here.
10-811	10	17	39	17	39	"attributability of change in global temperature to external influence" [J. Graham Cogley, Canada]	Rejected. No the text is correct as written. Section 10.2 will be revised to explain what is meant by the detection of the response to a particular forcing.
10-812	10	17	42	17	42	I'm not sure what is intended by "variability in forcing". Does this mean temporal variation in the size of the departure from radiative equilibrium? Would the discussion here be any different if aerosol loading were maintained in such a way that the size of the departure from radiative equilibrium remained constant over a period of time? If not, then I suppose one could delete "variability in". [Francis Zwiers, Canada]	Taken into account. We have deleted 'variability in'.
10-813	10	17	42	17	47	AMOC discussion could include some discussion that would expect AMOC changes to redistribute heat -- i..e cool NH and warm SH. [Simon Tett, United Kingdom]	Rejected. The assessed studies do show some projection on the global mean. We rely on an assessment of the published literature here.
10-814	10	17	44	17	47	Your statement is false. McLean et al (2009) showed the close link between ENSO and subsequent average global lower tropospheric temperature. On the basis that the IPCC regards ENSO as internal variability, the honest conclusion is that it is virtually certain that warming since 1950 can be explained by internal variability and there is no need to resort to external forcing to explain any warming. [John McLean, Australia]	Rejected. See response to 10-728.
10-815	10	17	46	17	47	It is important, when possible, to use the same language in the text-body as in the Executive Summary. So, here it is written that "it is virtually certain that warming since 1950 cannot be explained without extrernal	Taken into account. Revised to follow ES wording.

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						forcing". But, in the Executive Summary (pg. 10-3, lines 34-35) it is written that "it is virtually certain that this warming [global avg temperatures since the 1950s] is not due to internal variability alone" . If these statements ae intended to have identical meaning for the reader, then revise to use only one articulation. [Martin Hoerling, United States of America]	
10-816	10	17	46	17	47	It would be better to use "very likely or extremely likely" here. Also, the external forcings include CO2 or GHGs, aerosols, volcanoes and solar variability. Do the authors want to say "anthropogenic forcing"? [Guoyu Ren, China]	Rejected. No this statement is intended to include natural forcings. And our assessment is 'virtually certain' not one of the qualifiers suggest.
10-817	10	17	46	17	47	The AMO may or may not be a major feature of the climate, but either way I'm surprised you justify a statement on global change based solely on a discussion of the Atlantic. [Dáithí Stone, United States of America]	Rejected. We justify it mainly on the basis that observed warming is large compared to simulated internal variability.
10-818	10	17	49	17	8	This para on solar effects is far more succinct thn that on the AMO. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted.
10-819	10	17	49	18	8	Solar irradiance is just one type of emission from the sun, so where is your discussion of the modelling of other solar emissions? [John McLean, Australia]	Rejected. This paragraph considers solar forcing in general.
10-820	10	17	49	18	14	Claims that persistent high levels of forcing cannot cause continued warming must be judged highly suspect until backed up by GCM tests Chapter 7 of the SOD acknowledges strong evidence for a solar driver of temperature more powerful than can be accounted for by the slight variation in TSI (p. 7-43, lines 1-4): "Many empirical relationships have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system (e.g., Bond et al., 2001; Dengel et al., 2009; Ram and Stolz, 1999). The forcing from changes in total solar irradiance alone does not seem to account for these observations, implying the existence of an amplifying mechanism such as the hypothesized GCR-cloud link." This raises the question of whether late 20th century warming might have been caused by the high level of solar activity between 1950 and 2000 rather than by human increases in atmospheric CO2. In each place where the SOD addresses this question it makes the highly unscientific claim that late 20th century warming cannot have been caused by the sun because solar activity was not rising over this period. For instance, in Chapter 10 on attribution, page 10-18, lines 3-5, directly states that solar-driven temperature change should be driven by the trend in solar activity, not the level of solar activity: "... several studies show that solar variations cannot explain warming over the past 25 years, since solar irradiance has declined over this period (Lockwood and Fröhlich, 2007, 2008; Lockwood, 2008(Lockwood, 2012))." Lockwood claims that the smoothed level of solar activity started turning down at about the end of solar cycle 21, in the mid 80s. By most measures solar cycle 22 was stronger than cycle 21, so I would put the turn down ten years later, but set that aside. The point here is that what Lockwood thinks, and what the SOD here repeats, is that temperature is driven by the trend in the forcing, not the level of the forcing. Chapter 7 says the same thing (p. 7-44, lines 16-18): "The lack of trend in the cosmic ray intensity over the last 50 years (Agee et al., 2012; McCracken and Beer, 2007) provides another strong argument against the hypothesis of a major contribution of cosmic rays to ongoing climate change." The same argument is also made less explicitly in FAQ 5.1 (p. 5-50, lines 32-33). Do the authors really think that you can't heat a pot of water by turning the flame to maximum and leaving it there? Solar activity was at high levels (strong solar cycles) over the entire period in question. According to Usoskin	Rejected. The effect of cosmic rays on clouds is assessed in 7.4.5 and 8.4.1.5 where it is assessed that 'there is high confidence (medium evidence and high agreement) that the GCR-ionization mechanism is too weak to influence global concentrations of cloud condensation nuclei or their change over the last century or during a SC in a climatically-significant way'. Nonetheless, the attribution studies assessed in this chapter in which solar forcing is treated separately avoid making any assumption about the magnitude of the solar response in the real world, but estimate this in a regression. GCMs simulate the delay between forcing and response based on physical principles. So if a strong amplification mechanism exisited it would be identified in such studies.

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						2007 the 20th century experienced a "grand maximum" of solar activity that continued through about the end of century. Thus regardless of where the exact peak was, the entire second half of the century maintained average levels of solar activity that can be described as somewhere between high (Muscheler 2007) and very high (Usoskin). The claim that these persistent high levels of solar forcing cannot caused continued warming is highly counterintuitive and must rely on some unstated assumptions. The authors of these lines are probably assuming that the oceans had equilibrated to high post-50s warming by 1970 or so. Then yes, continued high solar forcing would be necessary just to maintain that equilibrium temperature. But any assumption that the oceans must have equilibrated by ANY 20th century date is highly speculative. That makes it a highly UNCERTAIN grounds for dismissing a solar explanation of late 20th century warming, not the "strong argument" that the SOD repeatedly asserts and implies. Also, such unstated assumptions obviously need to be made explicit and, most importantly, they need to be tested. Do the GCM test-runs In particular, the repeated claims that persistent high levels of solar forcing (beyond what can be accounted for by TSI) would not cause continued warming can and should be tested by GCM model runs. It seems clear that GCM tests of models with enhanced solar forcing effects have NOT yet been run. Otherwise these tests would be cited along with the claims that high post-50s solar activity could not have caused post-70s warming, but no such citations are listed at any of these points. Running the tests should be straightforward. The most likely avenue of enhanced solar forcing is some effect on cloud formation, whether through Svensmark's GCR-cloud mechanism or through the effects of UV-shift on atmospheric circulation, or through the earth's electrical circuit. Svensmark and Friis-Christensen (1997) suggest about a 2% variation in low clouds as solar activity varies, which would be a simple addition to existing GCMS. Climate sensitivity would have to be lowered as necessary to get the best fit to observed temperatures, and other parameters could be adjusted as well. Numerous parameters have been tweaked to bring the CO2-driven models (models where the only solar forcing is TSI) into their best fit with past climate data and the same would ideally be done for models with enhanced solar forcing, but conceptually this would be nothing new. The outcome is easy to predict. It is simple logic that a GCM test of continued high solar forcing WOULD show continued warming. You already know this from your commitment studies. You can also deduce it from the working of a simple 3-box model with an upper ocean layer, an intermediate layer and a deep layer. The upper ocean layer is known to warm quite rapidly after an increase in forcing, with a response time on the order of 4 to 7 years, but it will take much longer for this warmed-up upper ocean layer to warm up the intermediate layer of ocean below. As this slower warming takes place, he temperature differential between the upper and intermediate layers will steadily decrease which in turn will decrease the rate of heat loss from the upper ocean layer to the intermediate layer. That decrease in heat loss will cause the upper layer to continue to warm on an intermediate time scale, which will in turn cause continued warming of GMAST. The claim that continued high levels of post 50's solar forcing would not cause continued warming is clearly wrong. The best thing would be to simply remove it, but at the very least you should note that this is a speculative argument that at present is awaiting verification by GCM test runs. Of course it is embarrassing that these tests have not already been run. Along with internal variability, solar warming is THE alternative hypothesis to the favored CO2-warming theory. Given over $100b in public funding so far, "consensus" scientists really should have bothered by now to test the alternative hypothesis, but it is too late to rectify this failure in AR5. The only thing to do now is admit the omission and acknowledge that until the tests are run the proposed grounds for dismissing a solar explanation for late 20th century warming must be considered very uncertain.	

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						GCM test-runs are the ONLY evidence for the CO2-warming theory For some perspective on how big a role the GCM tests are playing, and how big a problem it is that the alternate hypothesis has not been GCM tested, note that the DIRECT evidence for the CO2-warming theory is virtually non-existent, while the paleo evidence for a powerful solar driver of climate is overwhelming. If traditional forms of evidence were used as a guide for which hypotheses received the attention of our new computational modeling tools then the primary object of study would be the enhanced-solar hypothesis. Instead, the only hypothesis that gets modeled is the one for which traditional forms of evidence are notably lacking. About that evidence, the paleo archives show a strong correlation between atmospheric CO2 and temperature, but with CO2 following temperature by an average of about 800 years (Caillon 2003), indicating that temperature drives atmospheric CO2 (as in theory it should, since warmer oceans hold less CO2). The reverse COULD also be taking place. CO2 could also be driving temperature, but thanks to the causality going in the other direction no such effect can be separated out from the paleo data. We do have evidence for a mechanism by which CO2 should cause some warming. A doubling of CO2 should have a modest temperature forcing effect, somewhere on the order of 1°C, but that forcing effect could either be amplified or dampened by feedback effects. The forcing does not in itself tell us whether CO2 explains much of recent warming, and there is no indication in the paleo records that CO2 is doing ANYTHING. In contrast, there is a veritable mountain of evidence in the paleo records for a powerful solar driver of climate, far more powerful than can begin to be accounted by the small variation in TSI. In my FOD comments I cited 2 dozen papers that have found something between a .4 and .7 degree of correlation between solar activity and various climate indices. That is, solar activity "explains" in the statistical sense something like half of all past temperature change. CO2 is invisible in the paleo data while solar activity screams out like a neon sign. So here is what it looks like is going on with the GCM testing. In the absence of direct evidence that CO2 is a powerful driver of climate the CO2-warming "consensus" has turned to this new and different kind of evidence: model-fitting, where the fact that CO2-driven GCMs are able to produce a not-so-bad fit to the last 150 years of observed climate data is presented as an affirmation of the CO2-warming theory. In actuality the evidence here is only negative. The models only demonstrate that CO2 can't be RULED OUT as the primary driver of recent temperature history: that there is a way to concoct a plausible scenario of how CO2 could be responsible. That is useful information, well worth collecting. What is perverse is to selectively apply this new kind of test only to the hypothesis that is NOT supported by traditional evidence. When it comes to the enhanced-solar hypothesis, this theory IS declared to be ruled out, and on the most unscientific grounds imaginable (continued high levels of forcing can't cause warming!), without ever being allowed access to the test that is used to say that the CO2 hypothesis can't be ruled out. If GCM data-fitting is a new kind of test, offering a new kind of evidence, then obviously it can't be applied selectively as a way of boost to one theory while hiding what it says about competing theories, yet this is exactly what is being done. The GCM tests are being used to provide what is presented as a strong new kind of evidence (supercomputers!) when it is actually weak evidence (negative evidence), and this inflated evidence is presented only for the favored CO2 hypothesis. We have this powerful new tool available and it is immediately subverted by this complete misuse. Instead of enhancing our ability to discern and follow evidence it is being used as a ruse to evade the evidence. It's time to start applying the GCM tool in the one place where traditional evidence says it most needs to be applied: to the solar-warming theory of late 20th century warming. It is time to start testing the alternate hypothesis, and AR5 should say so. Solar-warming GCM tests will blow CO2-warming GCM tests out of the water	

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						That's a guarantee. Both theories have trouble with mid-20th century dip in temperature. The cold dragon of the vasty deeps seems to have flicked its tail (part of the internal variability that could well turn out to be the biggest player of all). Both theories also do fine with post-WWII warming as a whole (solar activity and CO2 both attained historic highs over this period). But everywhere else the solar-theory fits better. It's a better fit for pre-WWII warming where the increase in CO2 forcing was still relatively minor but there was a substantial ramp up in solar activity from the turn of the century solar lull to the onset of the grand maximum in the 20s and 30s. Then there is the lack of warming over the last 15 years. The CO2 theory says that warming should have accelerated over this period. To be maintained it needs to again invoke a major episode of internal variability, while for the solar-warming theory the end to recent warming is just what one would expect now that the sun has dropped into a quiescent phase. If solar activity and CO2 have similarly sized forcing effects then warming should merely stop. If solar activity is the stronger effect then temperatures should next begin to fall. Allow a solar-driven GCM to be tweaked for fit the way the CO2 driven GCMs are and the solar theory would win in a walkover. Is this what is keeping "consensus" scientists from running GCM tests of the alternate hypothesis? Is it because they know that their CO2 theory will be routed? It is hard not be suspicious when "consensus" scientists are not only refusing to run enhanced-solar GCM tests but are at the same time making obviously wrong statements about what these tests would show if they WERE run. Continued high levels of enhanced solar forcing won't cause continued warming? Wanna bet? Maybe it has to be written into a bill. Maybe Senator Inhofe will have to make continued climate science funding contingent on the alternate hypothesis finally being included in the GCM modeling. But come on, some of you IPCC authors have to be real scientists. I'm sure you understand that it is wrong (bad science) to take this new kind of test and only apply it to what is not otherwise evidenced. So say it. Just note this curious omission—that the alternate hypothesis is still waiting to be GCM tested—and acknowledge that the proposed grounds for dismissing a solar explanation for late 20th century warming must be considered suspect until this oversight is rectified. [Alec Rawls, United States of America]	
10-821	10	17	49			Recommend summarizing the major recent quantitative proponent of phenomenological natural forcing Akasofu's modeling a multi-decadal oscillation on a linear rise. "Akasofu (2010) developed the null hypothesis of approximately linear warming from the Little Ice Age (LIA) about 1800-1850 to the present suuperimposed with a multi-decadal oscillation with a period of 50 to 60 years with extensive supporting evidence. Peaking of temperature about 1940 and 2000 results in temporarly little warming after 2000 due to natural changes." Reference: Syun-Ichi Akasofu (2010), On the recovery from the Little Ice Age, Natural Science, Vol. 2, No. 11, 1211-1224, doi:10.4236/ns.2010.211149 [David L. Hagen, United States of America]	Rejected. This study is not an attribution study, and we do not think it is relevant to the issue of the attribution of temperature changes to solar irradiance.
10-822	10	17	53	17	57	The one-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, United States of America]	Rejected. The first response mentioned here is not published in the peer reviewed literature. The second cited reference does not mention or cite Benestad and Schmidt.
10-823	10	17	53	18	14	Overall, the assessment on sun's role in climate change is less convincing and is felt unbalanced. There are many studies investigating the possible influence of solar activity on earth climate, and it is generally hold that the influence could not be overlooked. Not only in Europe but in eastern Asia including mainland China, weak solar activities are usually related to the cold winters and cool climate condition in varied time scales (e.g. Holmes, J.A., E.R. Cook, B. Yang, 2009. Climate change over the past 2000 years in Western China, Quaternary International, 194, pp. 91–107; Woollings, T., M. Lockwood, G. Masato, C. Bell, L. Gray. 2010. Enhanced signature of solar variability in Eurasian winter climate. Geophys Res Lett 37:L20805. doi:10.1029/2010GL044601; Ineson, S., A.A. Scaife, J.R. Knight, J.C. Manners, N.J. Dunstone, L.J. Gray and	Taken into account. The regional influences of solar forcing on climate will be discussed in a new box, Box 10.2.

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						J.D. Haigh. 2011. Solar forcing of winter climate variability in the Northern Hemisphere. Nature Geoscience, DOI:10.1038/NGEO1282). Certain association may exist between the solar drive and multi-decadal ocean oscillations which in turn affect the global and regional surface temperature. Considering possibly larger internal variability and the likely influence of solar activities, it is probably improper to use "extremely likely" and "extremely unlikely" for the conclusions drawn in this subsection. [Guoyu Ren, China]	
10-824	10	18	1	18	3	The assumption of no GHG-driven anthropogenic warming prior to 1950 is an approximation, not an "incorrect assumption." About 90% of anthropogenic GHGs in the atmosphere are the result of human activity after 1950. [David Burton, United States of America]	Rejected. The radiative forcing due to the anthropogenic increase in GHGs was more than 10% of its present day value in 1950. See Figure 10.1.
10-825	10	18	1	18	46	Given that there is a large contribution to warming in the early 20th century from internal variability and a suggestion that recent lack of warm is also due to internal variability is the extremely likely statement justified? I think the text needs some broader discussion of the role of internal variability, Perhaps a discussion on what a reasonable limit is might be helpful here. [Simon Tett, United Kingdom]	Taken into account. We now include more justification for the extremely likely statement, and include 2-forcing results in Fig 10.4 which support his conclusion.
10-826	10	18	5	18	5	Remove additional bracket between references. [Government of Canada]	Editorial. References will be reformatted.
10-827	10	18	7	18	8	Doesn't the conclusion about the contribution from "solar forcing" to the warming since 1950 in fact relate to the contribution from "solar irradiance" (TSI), which is what all the studies cited appear to investigate? It is possible that the sun might give rise to other forcings than that simply from TSI (e.g., via changes in UV radiation or influence on galactic cosmic rays), so the term "solar forcing" is too wide here. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. These studies generally include all aspects of solar forcing which vary congruently with TSI. So we think the term 'solar forcing' is justified here.
10-828	10	18	7	18	8	Although the old AR4 terms "extremely likely" and "extremely unlikely" are included as acceptable terms in the new uncertainty guidance document it would be preferable if one of the 7 primary likelihood terms could be used. Also relevant for line 12. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. We prefer to keep these terms since they are included in the GPGP.
10-829	10	18	7			The authors repeatedly mention "solar variability" throughout the chapter, but never address it or assess the literature that makes the case for influences of solar variability on 20th century climate. Though these studies are not formal D and A, in my view the authors need to assess the role of solar variability as opposed to trends in solar output in influencing 20th century climate. To omit an assessment of the considerable recent literature in this area that has provided much insight into how solar variability affects 20th century climate would be a serious omission in my view. As noted in my general comments above, this should be done to provide information to governments that would ask what influence the sun has on earth's climate. In cross chapter meetings in Marrakech, it was agreed that ch 10 should include this material, but it has not yet been included. I previously supplied a much longer text, but the following or some version thereof is quite a bit shorter and could be inserted here: [Gerald Meehl, United States of America]	Taken into account. This is assessed in a new box, Box 10.2.
10-830	10	18	7			"Even though the globally averaged signal is small in terms of trends, the 11 year solar cycle has been shown to produce measurable regional effects on the earth's climate system (Gray et al., 2010; Lockwood, 2012; National Research Council, 2012) which have implications for the decadal climate prediction problem (Ch. 11). Based on analyses of observations for years of peak sunspot number, it has been shown that, on average, the equatorial eastern Pacific sea surface temperatures (SSTs) tend to be below normal, the sea level pressure (SLP) in the Gulf of Alaska and the South Pacific above normal, and the tropical convergence zones on both hemispheres are strengthened and displaced polewards (van Loon et al., 2007; Camp and Tung, 2007; Meehl et al., 2008; van Loon and Meehl, 2011; Roy and Haigh, 2010; Hood and Soukharev, 2012). There is often a warm SST response in that region, which lags the cool response by a couple of years in the composites and in some models (Meehl and Arablaster, 2009; Meehl et al., 2009; Tung and Zhao, 2010; Zhao and Tung, 2010; Bal et al., 2011). For northern summer, there is evidence that for peaks in the 11 year solar cycle, the Indian monsoon is intensified (Kodera, 2004; van Loon and Meehl, 2012), with solar variability affecting aspects of interannual variability in the Indian sector as well (Kodera et al., 2007). Thus, though there is little trend in the climate system response to solar forcing over the past 50 or so years, peaks in the 11 year solar cycle have notable effects on regional decadal climate variability that involve the intensification of the mean climatological patterns of surface temperature and precipitation in the Indo-Pacific region (Gray et al., 2010). Additionally, the 11 year solar cycle has also been shown to be connected to decadal variability in the North	Taken into account. See response to 10-829.

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						Atlantic (Ineson et al., 2011). [Gerald Meehl, United States of America]	
10-831	10	18	7			References Bal, S., S. Schimanke, T. Spangehl, and U. Cubasch, 2011: On the robustness of the solar cycle signal in the Pacific region. Geophys. Res. Lett., 38, L14809, doi:10.1029/2011GL047964. Camp, C.D., and K.K. Tung, 2007: Surface warming by the solar cycle as revealed by the composite mean difference projection. Geophys. Res. Lett., 34, L14703, doi:10.1029/2007GL030207 Gray, L.J., and coauthors, 2010: Solar influences on climate. Rev. Geophys., 48, RG4001, doi:10.1029/2009RG000282. Hood, L.L., and R.E. Soukharev, 2012: The lower-stratospheric response to 11-yr solar forcing: Coupling to the troposphere-ocean response, J. Atmos. Sci., 69, 1841—1864. Kodera, K., 2004: Solar influence on the Indian Ocean monsoon through dynamical processes. Geophys. Res. Lett., 31, L24209, doi:10.1029/2004GL020928. Kodera, K., K. Coughlin and O. Arakawa, 2007: Possible modulation of the connection between the Pacific and Indian Ocean variability by the solar cycle. Geophys. Res. Lett., 34, L03710, doi:10.1029/2006GL027827 Ineson S., A.A. Scaife, J.R. Knight, J.C. Manners, N.M. Dunstone, L.J. Gray, and J.D. Haigh, 2011: Solar forcing of winter climate variability in the Northern Hemisphere, Nature Geoscience, 753-757, DOI: 10.1038/NGEO1282 Meehl, G.A., J.M. Arblaster, G. Branstator, and H. van Loon, 2008: A coupled air-sea response mechanism to solar forcing in the Pacific region. J. Climate, 21, 2883—2897. Meehl, G.A., and J.M. Arblaster, 2009: A lagged warm event-like response to peaks in solar forcing in the Pacific region. J. Climate, 22, 3647--3660. Meehl, G.A., J.M. Arblaster, K. Matthes, F. Sassi, and H. van Loon, 2009: Amplifying the Pacific Climate System Response to a Small 11-Year Solar Cycle Forcing. Science, 325, 1114-1118. Roy, I., and J.D. Haigh, 2010: Solar cycle signals in sea level pressure and sea surface temperature, Atmos. Chem. Phys., 10, 3147-3153. National Research Council, 2012: The Effects of Solar Variability on Earth's Climate: A Workshop Report. The National Academies Press, Washington, D.C., U.S.A., www.nap.edu. Tung, K.-K., and J. Zhou, 2010: The Pacific's response to surface heating in 130 yr of SST: La Niña-like or El Niño-like? J. Atmos. Sci., 67, 2649-2657. van Loon, H., G.A. Meehl, and D.J. Shea, 2007: Coupled air-sea response to solar forcing in the Pacific region during northern winter. J. Geophys.Res., 112, doi:10.1029/2006JD007378. van Loon, H. and G.A. Meehl, 2012: The Indian summer monsoon during peaks in the 11 year sunspot cycle. Geophys. Res. Lett., 39, L13701, doi:10.1029/2012GL051977. Zhou, J., and K.-K. Tung, 2010: Solar cycle in 150 years of global sea surface temperature data. J. Climate, 23, 3234-3248. [Gerald Meehl, United States of America]	Taken into account. See response to 10-829.
10-832	10	18	10	18	14	This paragraph is blatant nonsense. McLean et al (2009) showed that the ENSO is closely linked to average global tropospheric temperatures about 7 months later except during periods of cooling caused by volcanic eruptions in the tropical Pacific, and the little if any temperature is unaccounted for. According to this paper it is extremely UNLIKELY that human activities have any more than a negligible influence at most. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) [John McLean, Australia]	Rejected. See response to 10-728.
10-833	10	18	10			Do you mean "anthropogenically generated greenhouse gases"? [Peter Guttorp, United States of America]	Taken into account. Revised to 'the anthropogenic increase in greenhouse gases'.
10-834	10	18	12	18	12	There is discussion on the effect of a specific aspect of the natural internal variability (the AMO) but what about others aspects , eg the so called PDO/IPO [John Mitchell, United Kingdom]	Comparison of observed trends with simulated variability in global mean temperature is discussed at the end of 10.2.1.1.2, and validation of simulated variability is discussed there. We do not consider all

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							other modes individually, but the role of internal variability is already discussed.
10-835	10	18	13			What is the quantitative basis for "at least 50%"? [Peter Guttorp, United States of America]	Taken into account. This is better explained in the revision.
10-836	10	18	13			See comment 280 concerning the words "most of (at least 50%)". [Adrian Simmons, United Kingdom]	Taken into account. This is better explained in the revision.
10-837	10	18	17	18	17	abbreviation SAT maybe introduced much earlier. [Albert Klein Tank, Netherlands]	Editorial. Abbreviation SAT has been deleted here.
10-838	10	18	20	18	25	The discussion here seems to partition early 20th century change into three components (solar, volcanic, and a residual which apparently is interpreted as internal variability); that is, it makes no mention of an anthropogenic contribution - but presumably there was one since you seem to agree with the AR4 assessment in that regard. So, I think this needs to be clarified. [Francis Zwiers, Canada]	Taken into account. The role of the anthropogenic contribution in the Shiogama et al. and Hegerl et al. studies is now clarified.
10-839	10	18	29	18	29	Kennedy et al. (2011) - the SST data set used in Morice et al. (2012) - notes that the period of the second world war is likely more uncertain than the ensemble spread suggests. Cross-checking with marine air temperature is also problematic because there are notable biases in NMAT during the second world war. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. We do not wish to add additional discussion on observational uncertainties here, since this is covered in more detail in chapter 2.
10-840	10	18	30	18	35	It is very incorrect to say "there was no unusual warming in Australia and much of Asia" during the time period 1900-1940. As pointed out above, there was an obvious warming from 1910s to 1940s in mainland China, which forms a major part of eastern Asia (Tang, G. L., and G. Y. Ren, 2005: Reanalysis of surface air temperature change of the last 100 years over China. Climatic and Environmental Research, 10, 791-798. (in Chinese); Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q. and Zhang, J., 2007, Detection, causes and projection of climate change over China: an overview of recent progress, Advance in Atmospheric Sciences, 24 (6), 954-971; Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977). [Guoyu Ren, China]	Taken into account. 'and much of Asia' deleted.
10-841	10	18	40	18	40	These are not :"observed" or :"global mean": temperatures but a series of multiple averages of a varying number of means of maximum and minimum tempeture from a variety of weather staions and ship measurements [Vincent Gray, New Zealand]	Noted. This is discussed in chapter 2.
10-842	10	18	40	18	43	Partially repeats, partially contradicts statement on preceding page (p. 17, l. 31-36). [Jochem Marotzke, Germany]	Taken into account. This statement has been revised to note that these studies generally only attribute part of the early century variations to aerosols.
10-843	10	18	40	18	44	How does this relate to the discussion on page 17 of the possible role of the AMO? [Francis Zwiers, Canada]	Taken into account. This text has been revised to better reflect the discussion on page 17.
10-844	10	18	40	18	46	Same as above [Laurent Terray, France]	Taken into account. Terray (2012) is now cited here.
10-845	10	18	41	18	41	Getting confused as to what North Atlantic SSTs are and what is the AMO? You said earlier that the contribution of the AMO to global warming is quite small, but the AMO is just an SST measure which does contribute to the whole. I agree that the AMO contribution is small, you rtext is confusing at times on pp17-18. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We have revised the text. Hopefully it is clearer now.
10-846	10	18	43	18	44	is very likely in part is I think rather meaningless. In part could 10^-3K which would be negligiable. I think some more quantification here is required. Perhaps a more useful statement about the early 20th century is the mean warming +/- 2 sigma from Natural forcings + internal var would be more useful [Simon Tett, United Kingdom]	Taken into account. We no longer use 'in part'.
10-847	10	18	43	18	44	"in part due to" does not have a quantitative meaning here, and so the likelihood statement is meaningless. In the broadest sense we are certain that any signal is "in part due to external forcings" unless we have reason to believe that the external forcing is absolutely constant. Either quantify "in part" or otherwise clarify the statement. [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We no longer use 'in part due to'.
10-848	10	18	43	18	46	Given the statement that it is difficult to quantify the controibutions of natural forcing, anthropogenic forcing,	Taken into account. We now characterise the warming

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						and internal coupled system variability to early 20th century global warming, why then only provide a statement regarding the likely role of external forcing? Equally plausible statements (and Im not sure that the sentence beginning "In conclusion, the early...." is needed), " In conclusion, the early 20th century warming is very likely in part due to internal variability". Please state the justification for the particular concluding comment being made. This also applies to the Executive Summary, where on pg 10-3, line 50, the text elects to call out the role of external forcing in early 20th century warming, when one could just have plausibly written a statement to the effect of internal variability. [Martin Hoerling, United States of America]	as very unlikely to be due to internal variability alone.
10-849	10	18	48	18	56	Obviously, the multi-decadal variability with a magnitude larger than ever thought exists in the global and regional surface temperature series, and it had not been reproduced in almost all the simulations by the current climate models. This implies that the response of global mean surface temperature to increase in atmospheric CO2 concentration in the time period 1951-2000 and the climate system sensitivity were overestimated in the AR4, and the conclusions of attribution and projections made at that time may have been assigned a confidence a little bit higher. [Guoyu Ren, China]	Noted. The climate evolution of the last 15 years is one factor which is considered in our overall conclusions on detection and attribution in the AR5.
10-850	10	18	48	19	48	Why does IPCC consider a trend since 1998, a very strong El-Nino year und thus an outstanding warm year in the temperature time series? At least it should be mentioned that 1998 is a very warm year due to internal climate variability and that the considered time period is considerably shorter than the typical time period applied in the definition of climate. [Government of Germany]	Taken into account. This issue is now discussed in Box 9.2. We will mention that 1998 was an El Nino year, and show the effect of removing ENSO-related variability from the record.
10-851	10	18	48			I would advocate some redrafting of this section. It is astonishing that the words "El Nino" (and indeed "La Nina") do not appear in it. 1998 was an El Nino year, and there is little doubt that this was responsible for its warmth. So 1998 was warm compared with immediately preceding and following years because of natural variability, and it should be dismissed from the discussion at the outset. Discounting 1998, near-surface temperatures go on rising, with some year-to-year variability, until 2005, so there is now a much shorter period to discuss. 2010 was not much cooler than 2005 near the surface. Moreover, whereas the warmth of 2005 was shallow and confined to high latitudes, that in 2010 was deep, associated again with El Nino conditions. Time series of global-mean vertically integrated temperature or (almost equivalently) thermal energy computed from ERA-Interim have 2010 as the warmest year on record, followed by 1998. According to this measure, 2002-2007 are relatively warm years, but there are cooler recent years apart from 2010. But these may in part be explained by internal variability associated with occurrence of La Nina (linked also, almost certainly, with the abatement of the SE Australian drought discussed elsewhere in this chapter). Note that in its statement on the climate in 2011, WMO remarked that the year was nominally only the 11th warmest on record, but that it was in fact the warmest year in which a moderate or strong La Nina occurred. [Adrian Simmons, United Kingdom]	Taken into account. This issue is now discussed in Box 9.2.
10-852	10	18	49	18	49	Since 1998 the trend is actually negative, so to write 'have not increased strongly' is misleading. I refer again to the excellent analyses of Lucia Liljegren on her blog The Blackboard. It would be informative for example if AR5 answered the question 'since which year the warming ceases to be significant?' A relevant blog post about the recent standstill is http://rankexploits.com/musings/2012/rose-v-met-office/ in which she shows this graph: http://rankexploits.com/musings/wp-content/uploads/2012/10/Rose_Snit_Annotated.png AR5 would improve by showing such a graph. This one shows clearly that since 1997 there still is (although a very small) warming trend. However it also shows that HadCrut3 (AR5 should do it with HadCrut4 of course) is just outside the 95% uncertainty range around the multimodel mean. So the models are running warm. [Marcel Crok, The Netherlands]	Taken into account. This issue is now discussed in Box 9.2.
10-853	10	18	49	18	49	Revise sentence. The data indicate that there has been little change in globally averaged temperature over the 15-yr period since 1998. The statement that it has "not increased strongly" gives the wrong impression that an increase has been observed. [Martin Hoerling, United States of America]	Taken into account. This issue is now discussed in Box 9.2.
10-854	10	18	49	18	49	The statement that global mean temperatures "have not increased strongly since 1998" seems rather misleading. The annual mean global temperature per HadCRUT4 actually decreased between 1998 and 2011, and the linear trend in temperature over that period was insignificant. It is necessary to go back to a start date before 1970 to find a period of the same length as 1998 - 2011 with as low a linear trend (per the HadCRUT4 annual global mean temperature record). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This issue is now discussed in Box 9.2.

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10-855	10	18	49	18	49	Correction required - No statistically significant warming has occurred since January 1997 and it is unclear when warming might resume. [John McLean, Australia]	Taken into account. This issue is now discussed in Box 9.2.
10-856	10	18	49	18	49	"Global mean surface temperatures have not increased strongly since 1998" is very vague wording, and implies that they have increased....just not strongly, and what does 'strongly' mean? The wording used on the next page (p. 19, l. 44) "the trend in global mean temperature since 1998 is not significantly different from zero" is much clearer in this regard. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This issue is now discussed in Box 9.2.
10-857	10	18	49	18	50	This opening sentence states '...temperatures have not increased strongly…' which is a different statement to that used in the executive summary '…not significantly different from zero.' As with a previous comment, starting with this negative statement detracts from the key message that this section is trying to convey. Recommend starting with 'Global mean temperature trends since 1998 are consistent with internal variability overlying the forced trends seen in climate model projections'. [Government of Australia]	Taken into account. This issue is now discussed in Box 9.2.We will avoid using the phrase 'has not increased strongly' there. Using updated analysis we will likely conclude that there is a discrepancy between simulated and observed trends.
10-858	10	18	49	19	13	Could some assessment of how much internal variability would need to contribute to explain the lack of warming and a more definitive statement about the consistency (or not) with model simulations of decadal variabilty be included? [Simon Tett, United Kingdom]	Accepted. This issue is now discussed in Box 9.2. A more definitive statement on the consistency between simulated and observed trends has bene included, and another on the role of internal variability included.
10-859	10	18	49	19	20	The last line of the section states that 'Kaufmann et al. 2011 argue….warming is ....explained by increase in tropospheric aerosols'. While the first line of the section 'Global mean…constant or declining aerosols'. If the aerosols have been increasing since 1998, why were the models forced with constant or declining aerosols. This should be clarified. [Government of United States of America]	Accepted. This issue will now be discussed in Box 9.2. We will include an updated assessment on the role of aerosol forcing there.
10-860	10	18	49	19	48	It might be worthwhile to mention that GHG forcing should have caused a warming of about 0.2 K in the period since 1998, as can be seen from the second last plot in Figure 10.5 [Government of United States of America]	Taken into account. This issue is now discussed in Box 9.2. A histogram ise shown comparing simulated and observed trends over the 1998-2012 period. The simulated trends include the response to GHG increases over this period.
10-861	10	18	50	18	50	Suggest replacing "increased" with "increases" (this describes a reproducible model response to prescribed forcing, not an event that was observed). [Francis Zwiers, Canada]	Taken into account. We avoid using this phrasing in Box 9.2 where this is discussed.
10-862	10	18	51	18	55	This sentence assumes that anthropogenic warming is significant when there is no evidence at all that this is the case. McLean et al (2009) showed that the ENSO correlates closely with global mean lower tropospheric temperature seven months later. The "slowdown" (actually cessation) of warming can be attributed to a sustained period of ENSO conditions on the El Nino side of of absolutely neutral (i.e. zero). Little variation in the ENSO implies little variation in temperature, and that is what has been observed. [John McLean, Australia]	Rejected. See response to 10-728.
10-863	10	18	55	19	2	Again, McLean et al (2009) showed the link between ENSO and temperature and that there is ngeligible if any need to assume, as climate models do, that greenhouse gas emissions made any contribution whatsoever. [John McLean, Australia]	Rejected. See response to 10-728.
10-864	10	18	57	18	57	"multimodel". [J. Graham Cogley, Canada]	Taken into account. This issue will now be discussed in Box 9.2. We will correct this typo there.
10-865	10	18	57	18	57	typo multimodel [European Union]	Taken into account. This issue will now be discussed in Box 9.2. We will correct this typo there.
10-866	10	18	57	18	57	"...and multimodel response…" [James Renwick, New Zealand]	Taken into account. This issue will now be discussed in Box 9.2. We will correct this typo there.
10-867	10	18	57	18	57	Should "Figure 1.1" be "Figure 10.1"? [Francis Zwiers, Canada]	Accepted. This issue will now be discussed in Box 9.2.
10-868	10	19	1	19	13	While the influence of AMO and AO are mentioned in the associated figure 10.5, it might be worthwhile to discuss them briefly in the text as well. [Government of United States of America]	Accepted. This issue will now be discussed in Box 9.2. We will consider this suggestion when preparing the box.

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10-869	10	19	3	19	3	Insert "an estimate of the" after "After removal of", so that this reads "After removal of an estimate of the ENSO influence…". [Francis Zwiers, Canada]	Accepted. We will try to ensure this phrasing when Box 9.2 is prepared.
10-870	10	19	3	19	5	And what if any proof to you have that Knight et al (2009) correctly compensated for ENSO effects? If he did not incorporate a time lag as shown in Figure 7 of McLean et al (2009), where the monthly data is plotted with the SOI shifted forward by 7 months, then Knight has failed to properly recognise the influence of ENSO and his findings may be dismissed. [John McLean, Australia]	Rejected. See response to 10-728.
10-871	10	19	5	19	7	Reconsider this sentence. It is stated that ENSO contributed to a warming influence over 1999-2008, somewhat evident from visual inspection of Fig. 10.5b. However, the question of this section is about the period since 1998 (not 1999), and the beginning/end year choices matter. In this regard, it is evident also from Fig. 10.5b that, when calculated since 1998, ENSO has contributed to a cooling influence (1998-2010). In reconsider this sentence therefore, the authors may wish to indicate that ENSO variability has contributed to a cooling influence since 1998, owing largely to the strong El Nino warming at the beginning year, whereas the end years tended to experience more La Nina-like variations. [Martin Hoerling, United States of America]	Taken into account. This issue will now be discussed in Box 9.2. We will consider ENSO influence over the whole 1998-2012 period, and will explicitly mention that the effect of the start year.
10-872	10	19	5	19	7	The statement assumes that El Nino conditions will cause warming and that sustained El Nino conditions will progressively increase that warming. The claim is false. McLean et al (2009) showed that temperatures are consistent with the ENSO conditions of 7 months earlier, so if the ENSO is varying only slightly then the temperature variation will be negligible. Sustained elevated temperatures is an entirely different matter to continued warming because "warming" reflects a rate of change. [John McLean, Australia]	Rejected. We do not understand the argument being made here.
10-873	10	19	5			Is HadCM3 so famous now that it doesn't need to be mentioned what it is? It might be! [Dáithí Stone, United States of America]	Noted. This issue will now be discussed in Box 9.2. HadCM3 will not be mentioned by name there.
10-874	10	19	6	19	8	In line with the finding of McLean et al (2009), the sustained dominance of conditions on the El Nino side of neutral (I.e. zero) means elevated and sustained strength of the Hadley Circulation, which according to numerous papers will cause widespread elevated temperatures. It is therefore highly likely that the widespread elevated temperatures can be attributed to the ENSO and, as McLean et al (2009) showed, the ENSO-temperature link leaves little if any further warming to be explained, meaning that it is highly unlikely that anthropogenic emissions of greenhouse gases have contributed any meaningful warming. [John McLean, Australia]	Rejected. See response to 10-728.
10-875	10	19	7	19	10	Unclear what refers to Meehl 2011 and 2012 [Emma Daniels, Netherlands]	Taken into account. This issue will now be discussed in Box 9.2. The reference to Meehl (2011) will be made clear.
10-876	10	19	7	19	10	This sentence is important but too difficult to follow. Does 'below 300m' refer to below sea surface? [Government of Australia]	Taken into account. This issue will now be discussed in Box 9.2. We will clarify this discussion.
10-877	10	19	10	19	13	The statement about Trenberth et al. (2009) arguing that observed heat uptake in the upper ocean is inconsistent with top of atmosphere radiation measurements showing a similar radiative imbalance is at best confusing, since it is unclear what the "similar radiative imbalance" referred to is. If the reference is to the 0.9 W/m^2 imbalance in Table 2b of Trenberth et al., it should be made clear that they cite Hansen et al (2005a)'s estimate of 0.85 +/- 0.15 W/m^2 as the source for this figure being known with any degree of accuracy. It should also be pointed out that Hansen et al. (2005a) derive this figure using an estimate of 0.60 +/- 0.10 W/m^2 for 1993-2003 mean heat uptake in the 0-700m ocean layer, averaged over the whole of the Earth's surface, whereas the latest estimates indicate that the true figure was much lower than that (Box 3.1, Figure 1 of AR5 WG1 shows a trend of ~ 0.3 W/m^2 for the 0-700m ocean layer over that period, as does updated data from Levitus et al 2009 available at http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/index.html). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This issue will now be discussed in Box 9.2. Trenberth et al. (2009) will likely not be cited there.
10-878	10	19	10	19	13	The reference to Trenberth et al (2009) is incorrect. This should refer to Trenberth, K. E., and J. T. Fasullo, 2010: Tracking Earth's energy. Science, 328, 316-317, and also, Trenberth, K. E., and J. T. Fasullo 2011: Tracking Earth's energy: From El Niño to global warming. Surveys in Geophys., 33, 413-426. doi: 10.1007/s10712-011-9150-2. There are errors in Loeb et al. (2012) because they included a systematic error component in their random errors that determine the variations from year to year. As a result they	Taken into account. This issue will now be discussed in Box 9.2.

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						overestimated the uncertainty error bars. What they found was consistency only because their error bars were so large. In fact their are real differences between the CERES and OHC data that remain. However a paper submitted resolves the issue by finding the missing energy deeper in the ocean. This paper was not submitted in time for IPCC but you should know about it: it uses ECMWF ocean reanalyses ORAS4 (Balmaseda et al.) [Kevin Trenberth, United States of America]	
10-879	10	19	11	19	12	Rather than referring to the "2000-2009 temperature trend" (this is a bit confusing because a 10-year trend is very hard to estimate well), this would be clearer if the text said that Solomon et al estimate that the decade warmed 0.04K less due to the change in the observed reduction in stratospheric water vapour. [Francis Zwiers, Canada]	Taken into account. This issue will now be discussed in Box 9.2. We will avoid referring to the ten year temperature trend in this context.
10-880	10	19	15	19	15	Actually it is about 15 years from 1998 to 2012. [Guoyu Ren, China]	Taken into account. This issue will now be discussed in Box 9.2, and will focus on the 15-year trend 1998-2012.
10-881	10	19	15	19	20	The statement about residual progressive warming is not true. I was the person who performed the original analysis of this type of study (Christy and McNider, Nature 1994) and have redone it (i.e. accounting for volcanoes and El Ninos) in the tropospheric trend, and the result today is identical to the result from 1994 of +0.09 C/decade as the residual trend. There has NOT been a progressive warming. Rahmstorf uses an incredible magnitude of solar influence on the climate that cannot be verified - indeed the correlation of solar flux anomalies and global tropospheric temperature residuals is insignificantly negative - hence no signal at all. My result makes common sense - the El Ninos exert essentially a trend of zero since 1979 while the two volcanoes cool the planet in the first half of the record. So, the actual trend of +0.14 C/decade (using either UAH or RSS data) must be reduced when removing the volcanic effect (this is for 1979-2011), leaving a residual trend of, again, +0.09 C/decade. (This updated result has been written up and is being prepared for submission as I type this.) [John Christy, United States of America]	Taken into account. This issue will now be discussed in Box 9.2.
10-882	10	19	15	19	20	This para could conclude by saying that for this period ENSO variability is by far the most important component, particularly the size of the event in 1997/98. A plot factoring out just ENSO would be very useful. FAQ5.1 Figure 1 illustrates this vey well, with no large El Nino events in the last 12 years. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This issue will now be discussed in Box 9.2.
10-883	10	19	15	19	20	Here the assessment speaks of "less rapid warming" and "muted warming" over the past decade. Again this seems to be wording that is open to misinterpretation, and overall gives the impression that it has warmed, just not as strongly as expected. This seems to contradict the clear assessment that the trend in global mean temperature over this time was not significantly different from zero. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This issue will now be discussed in Box 9.2. We will avoid this ambiguity in the text there.
10-884	10	19	15		20	Church et al. 2011 GRL is also relevant here [John Church, Australia]	Noted. This issue will now be discussed in Box 9.2.
10-885	10	19	16	19	18	This statement is nonsense unless it can be proven that the removal of these forces is 100% accurate. McLean et al (2009) showed that global average lower tropospheric temperatures are very closely linked to ENSO conditions seven months earlier, except when volcanic eruptions cause cooling; negligible if any sustained temperature variation remains unaccounted for. [John McLean, Australia]	Rejected. See response to 10-728.
10-886	10	19	23	19	23	Why is HadCRUT3 used here when HadCRUT4 used elsewhere in this report (e.g. page 17 of this chapter). This needs to be consistent. [John McLean, Australia]	Rejected. We use HadCRUT3 here for consistency with published studies.
10-887	10	19	23	19	23	Update Figure 10.5 to include HadCRUT4. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected. We use HadCRUT3 here for consistency with published studies.
10-888	10	19	23	19	27	Was HadCRUT3 independently audited before it was cited? HadCRUT3 is compromised by poor global coverage prior to 1950 and then distorted from about 1989 onwards by the scarcity of data from eastern Russia where the anomalies vary from about -6 to +6 in the same month. Further, the HadCRUT3 is a poor match to data from Russian meteorological services with discrepancies exceeding 10 degrees. [John McLean, Australia]	Rejected. The studies shown here used HadCRUT3. It is a published dataset.
10-889	10	19	23			Would it be much work to produce this figure using HadCRUT4 (using the median values) rather than HadCRUT3? By the time this assessment report is published, the newer dataset will have been available for	Rejected. We use HadCRUT3 here for consistency with published studies.

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						some eighteen months, and it is desirable to have figures as up-to-date as possible. [Adrian Simmons, United Kingdom]	
10-890	10	19	25	19	25	How do you explain the discrepancy between this graph, which purports to convert an ENSO signal into degrees, with McLean et al (2009) whose figure 7 plotted monthly Troup SOI (shifted forwards by 7 months) and mean global lower tropospheric temperature anomaly? The Troup SOI averaged about +2 (slightly towards La Nina) for almost any time span from 10 to 30 years prior to 1976 and then averaged -4 (double the distance towards El Nino) for almost any time span from 10 to 30 years after 1976. The Pacific Climate Shift, mentioned several times in 4AR as a watershed for temperatures, occurred in 1976. (refer also Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures") Why does Figure 10.5 show virtually no trend in SOI from 1960, well prior to the shift, until 1990, well after the shift? [John McLean, Australia]	Rejected. See response to 10-728.
10-891	10	19	37	19	42	Is this worth including giving only reported by one model? [John Mitchell, United Kingdom]	Taken into account. This issue will now be discussed in Box 9.2.
10-892	10	19	44	19	44	In this summary you should make it very clear that the AR4 model projections didn't project this standstill and that over the last more or less 15 years observations and model projections are not consistent with eachother, see my comment above about the analyses on The Blackboard. [Marcel Crok, The Netherlands]	Taken into account. This issue will now be discussed in Box 9.2. We will directly compare simulated and observed trends over the past 15 years.
10-893	10	19	44	19	44	Use consistent language. Here it is stated that the "trend in global mean temperature since 1998 is not significantly different from zero", whereas on pg 10-18, line 49 it is stated that "global mean surface temperatures have not increased strongly since 1998". I suspect the former is closer to the data evidence, and should be used. [Martin Hoerling, United States of America]	Taken into account. This issue will now be discussed in Box 9.2. We will ensure consistent use of language there.
10-894	10	19	44	19	44	Purposefully left blank. [Martin Hoerling, United States of America]	No repsonse required.
10-895	10	19	44	19	44	What does "significant" mean here? Statistically significant? If so, give details. Be more specific. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This issue will be discussed in Box 9.2. We will no longer discuss the significance or otherwise of observed trends over short periods, but focus on the coinsistency or otherwise of simulated and observed trends.
10-896	10	19	44	19	44	At last you admit that there has been no statistically significant warming since 1998 (should be 1997), but why has this information withheld from the reader for so long? [John McLean, Australia]	Taken into account. This issue will now be discussed in Box 9.2. The conclusions will be presented in the executive summary of Chapter 10.
10-897	10	19	44	19	48	Explanation to this has to be strengthened here or in previous paragraphs. What candidate mechanisms? Who proposed the mechanisms? And how do the mechanisms work? [Guoyu Ren, China]	Taken into account. This issue will now be discussed in Box 9.2. Reasons for the discrepancy will be discussed in more detail there.
10-898	10	19	44	19	48	I am a bit worried about this fascination with 1998 as an end point. Your state here that the trend since 1998 is consistent with our understanding. But what about since 1999, 2000, 2007, or whenever? Can you not state anything more general? You are only banging on one gopher hole as it stands. [Dáithí Stone, United States of America]	Taken into account. This issue will now be discussed in Box 9.2. We have to pick some start point to focus our discussion here. We will note the role of ENSO.
10-899	10	19	44	19	48	I think this text overstates the case -- though see my earlier remarks. i.e. I think with more evidence presented above that the report might be able to say this. [Simon Tett, United Kingdom]	Taken into account. This issue isnow discussed in Box 9.2 and the conclusion has changed.
10-900	10	19	45	19	46	Also state that the observational data is consistent with the ENSO varying only slightly across most of that period, as per the findings of McLean et al (2009). (High confidence) [John McLean, Australia]	Taken into account. This issue will be discussed in Box 9.2 .
10-901	10	19	46	19	46	See Point 8. Suggest to include "(high confidence)" in Executive Summary to emphasize that this feature of the global temperature record is relatively well understood. [Oliver David Andrews, United Kingdom]	Rejected. This conclusion will change in the final draft based on new analysis not available at the time of the SOD. This issue will be discussed in Box 9.2. The cause of the hiatus is not fulll understood.
10-902	10	19	46	19	46	Suggest to include "(high confidence)" in Executive Summary to emphasize that this feature of the global temperature record is relatively well understood. [European Union]	Rejected. This conclusion will change in the final draft based on new analysis not available at the time of the

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							SOD. This issue will be discussed in Box 9.2. The cause of the hiatus is not fully understood.
10-903	10	19	51	19	52	Why does it have to be "useful for applications" in the context of this assessment? [Albert Klein Tank, Netherlands]	Noted. The IPCC Assessment is aimed at policymakers.
10-904	10	19	52	19	52	Replace "are useful" with "is more useful". [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-905	10	20	3	20	4	Unclear what "grid cell variability" means. [Albert Klein Tank, Netherlands]	Taken into account. Replaced with 'variability in individual grid cells'.
10-906	10	20	4	20	4	Replace "variability is not generally underestimated" with "variability in surface temperature is not generally underesimated" (the statement might not be true for other variables, such as precipitation). [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-907	10	20	6	20	7	"in every continent except Antarctica" should follow "likely", which is the adjective that it is intended to qualify. The present text is at risk of being misread by hasty readers as suggesting that an anthropogenic contribution is unlikely in Antarctica. [J. Graham Cogley, Canada]	Rejected. This is just a direct quote from AR4.
10-908	10	20	6	20	27	Results from the Berkeley land surface temperature data might also be useful here. [Government of United States of America]	Rejected. We are not aware of any regional attribution studies which use this dataset.
10-909	10	20	9	20	9	Morice et al. (2012) not (2011) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted.
10-910	10	20	10	20	10	Typo: "temperatures" [Jochem Marotzke, Germany]	Accpted.
10-911	10	20	10	20	12	On the contrary, ENSO conditions can account for the observed warming. Technically your statement is correct, but only because ENSO modelling and predictions continue to be poor. [John McLean, Australia]	Rejected. See response to 10-728.
10-912	10	20	14	20	15	Gillettet al (2008a) is the same as Gillett et al (2008c). [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. This has been corrected.
10-913	10	20	25	20	27	The previous sentences summarize both the observational and modelling evidence. I would leave the assessment of the observational evidence to Chapter 2, and simply conclude by saying something like "Consequently, while human influence has been detected in the available surface temperature records, it is currently not possible to attribute changes in Antarctic region land surface temperatures to causes.". The problem with making a confidence assessment in this case is that it would not be clear what we are not confident about, so perhaps it is best to leave it as an assessment of the evidence only. [Francis Zwiers, Canada]	Rejected. We just report the assessment of the observational evidence from chapter 2, and then give our own attribution assessment.
10-914	10	20	27			Attribution to what? Do you really mean detection? [Peter Guttorp, United States of America]	Taken into account. We now say 'to anthropogenic influence'.
10-915	10	20	27			Attribution to what? [Dáithí Stone, United States of America]	Taken into account. We now say 'to anthropogenic influence'.
10-916	10	20	36	20	40	“Since the publication of the AR4 several other studies have applied attribution analyses to continental and sub-continental scale regions. Min and Hense (2007) apply a Bayesian decision analysis to continental-scale temperatures using the CMIP3 multi-model ensemble and conclude that forcing combinations including greenhouse gas increases provide the best explanation of 20th century observed changes in temperature on every inhabited continent except Europe, where the observational evidence is not decisive in their analysis.” [Government of United States of America]	Noted. This is just a quote from the draft - not clear what is being suggested here.
10-917	10	20	36	21	40	This section seemed overly detailed and could be condensed to shorten the text. [Chris Forest, United States of America]	Rejected. Other reviewers suggested we should put more emphasis on regional attribution analyses.
10-918	10	20	37	20	37	Suggest inserting "technique" after "Bayesian decision analysis", since there is probably more than one way to do this. [Francis Zwiers, Canada]	Accepted.
10-919	10	20	37	20	40	Add to the end of the sentence "but without accurate modelling of the ENSO state the findings of Min and	Rejected. See response to 10-728.

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						Hense (2007) cannot be regarded as conclusive because McLean et al (2009) showed the ENSO to have a major influence on temperature." [John McLean, Australia]	
10-920	10	20	46	20	49	Don't see how these two sentences link together. [Albert Klein Tank, Netherlands]	Taken into account. The reviewer is right - these are two separate results. We have deleted 'Nonetheless'.
10-921	10	20	51	20	56	Sentence is awkward - suggest revising if possible. [Government of Canada]	Taken into account. We have revised and repeat 'anthropogenic influence has been found in' to clarify.
10-922	10	20	51	21	2	Please add "Analyses of 1950-2005 western U.S. weather station data also detected a shift of 1.5 days/decade of spring warmth to earlier in the year and found that only one-third of the shift was attributable to natural variability (Ault et al. 2011)." [Patrick Gonzalez, United States of America]	Rejected. This is not an attribution study which identifies the forced component of an observed trend.
10-923	10	20	51	21	5	please add: The attribution studies in China that increasing greenhouse gas concentrations in the atmosphere are likely to be a main factor for the observed surface warming over China (Ren et al., 2012). Reference: Ren, G., Y.H.Ding, Z.C.Zhao, J.Y.Zheng, T.W.Wu, G.L.Tang and Y.Xu, 2012, Recent progress in studies of climate change in China, Advances in Atmospheric Sciences, 29(05), 958-977 [Zong-Ci Zhao, China]	Rejected. This study only makes qualitative comparisons of simualted and observed climate change in China. It does not make quantiative assessments comparing the observed change with simulated internal variability, which are needed to make a detection statement.
10-924	10	20	55		58	The statement "Anthropogenic increases in greenhouse gases are found to be the main driver ..." is a large overstatement. These studies did not make any serious attempt to define with uncertainty that part of GHG increases that was anthropogenic. There are numerous other instances in this chapter where the term "anthropogenic" is used incorrectly or certainly without published backup. Please note that AR4 in the SPM used this word to modify the gases, not the observed increase. [Michael Prather, United States of America]	Taken into account. We now say 'increase in athropogenic greenhouse gases'.
10-925	10	20	58	21	2	What evidence do you have that Gillett et al (2008b) correctly simulated all natural forces when it was clear from 4AR that many natural forces are poorly modelled? If you have no evidence then logically you cannot endorse the findings of Gillett et al by citing them. [John McLean, Australia]	Rejected. We disagree that it is clear from AR4 that the response to natural forcings is poorly modelled.
10-926	10	21	5	21	6	This claim is remarkable given that others have attributed Arctic temperature changes to the AMO, PDO and increased Hadley Circulation associated with El Nino. [John McLean, Australia]	Noted. The reviewer cites no literature in his comment, nor does he recommend changes.
10-927	10	21	7	21	11	This sounds circular [John Mitchell, United Kingdom]	Noted. We agree. We think this is clear from the text as written.
10-928	10	21	10	27		Recommend addressing the quantitative arguments of Ross McKitrick who comes to the opposite conclusion to that of Sedlacek and Knutti, 2012. (See also above at ch 10 p 16 #1-5 above.) "McKitrick(2012) found temperature data to be strongly affected by local population growth and socioeconomic development." Reference: Ross McKitrick (2012) Encompassing Tests of Socioeconomic Signals in Surface Climate Data, Discussion Paper 2012-02, Dept. Economics, University of Guelph, Feb. 6, 2012. [David L. Hagen, United States of America]	Rejected. This comment does not seem relevant. Seems to refer to Pg 16 ln 21-27. Still, this is an issue for the observations chapter. This is discussed in chapter 2.
10-929	10	21	10	27		Recommend adding: "Recent analysis by Watts et al. (2012) using the Leroy (2010) classification system found strong differences in thrends between the best weather stations (classes 1&2) and the poor weather stations (clases 3-5). Raw mean temperature trends for well sited stations are 0.145 deg C per decade lower than adjusted mean temperature trends for poorly cited stations". Reference: "Anthony Watts, Evan Jones, Stephen McIntyre & John R. Christy, An area and distance weighted analysis of the impacts of station exposure on the U.S. Historical Climatological Network temperatures and temperature trends. Pre-Print Draft Discussion Paper", Posted at: http://wattsupwiththat.files.wordpress.com/2012/07/watts-et-al_2012_discussion_paper_webrelease.pdf Leroy, M., 2010: Siting Classification for Surface Observing Stations on Land, Climate, and Upper-air Observations JMA/WMO Workshop on Quality Management in Surface, 937 Tokyo, Japan 27-30 July 2010 [David L. Hagen, United States of America]	Rejected. This comment does not seem relevant. Seems to refer to Pg 16 ln 21-27. Still, this is an issue for the observations chapter. This is discussed in chapter 2.
10-930	10	21	14	21	14	Should this not be very likely - and also it should be italicised? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Assessment changed to very likely and italicized.

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10-931	10	21	14	21	14	Assessment terms, such as likely, are not always consistently shown in the text in italics. [Francis Zwiers, Canada]	Accepted. Italicized.
10-932	10	21	17	21	17	Insert "the" ahead of "climate model grid box scale". [Francis Zwiers, Canada]	Accepted.
10-933	10	21	17	21	27	"Some attribution analyses have considered temperature trends at climate model grid box scale. At these spatial scales robust attribution is very difficult to obtain, since climate model often lack the processes needed to simulate regional details realistically, regionally important forcings may be missing in some models and observational uncertainties are very large for some regions of the world at grid box scale (Hegerl et al., 2007b; Stott et al., 2010). Nevertheless an attribution analysis has been carried out on Central England temperature, a record which has been corrected for the influence of urbanisation and which, extending back to 1659, is sufficiently long to demonstrate that the representation of muiti-decadal variability in the single grid box in the model used, HadCM3 is adequate for detection (Karoly and Stott, 2006). The observed trend in Central England Temperature is inconsistent with either internal variability or the simulated response to natural forcings, but is consistent with the simulated response when anthropogenic forcings are included (Karoly and Stott, 2006)". From the two statements (paragraphs) above can it be deduced that in Karoly and Stott's (2006) experiment, the correction for the influence of urbanization, the length of data and the use of HadCM3 might have nailed down what was missed by the continental scale experiment of Min and Hense (2007) hence leading to conflicting results over Europe (at least Central England). Does this raise urbanization to a higher level (relative to other continents) as a key player in continental Europe in the determination of temperature changes? Might adding a sentence or two help place these two experiments in perspective. [Government of United States of America]	Taken into account. The data used by Min and Hense was also corrected for urbanisation. The difference in their results does not demonstrate the importance of urbanisation. We have removed the reference to the correction for urbanisation of CET to avoid this false impression.
10-934	10	21	17	21	40	These two paragraphs, especially the second are a bit out of date now. As I think this whole chapter is far too long, perhaps you need to think about removing some sections and avoiding repetitions. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. We are not aware of any more recent relevant literature here which we have not cited. We think it is important to have a discussion about attriution at grid box scales, because this is very relevant to Working Group II among other reasons.
10-935	10	21	22	21	22	What is the relevance that CET has been corrected for urbanization. Useful to give a correlation between CET and the 50-55N, 0-5W grid box from CRUTEM4. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. We have deleted the text saying that CET was corrected for urbanisation.
10-936	10	21	23	21	23	Multi is spelled incorrectly. [Francis Zwiers, Canada]	Accepted. Corrected.
10-937	10	21	24	21	27	This statement is unsustainable given that natural climate forces are poorly simulated by models. [John McLean, Australia]	Rejected. The reviewer does not offer any evidence in support of his claim.
10-938	10	21	24			Stated this way it sounds a bit circular, as in HadCM3 would have been deemed bad if it didn't get this answer. [Dáithí Stone, United States of America]	Rejected. The record is sufficiently long to assess multi-decadal variability in the absence of anthropogenic forcing.
10-939	10	21	29	21	32	This statement is unsustainable given that natural climate forces are poorly simulated by models. [John McLean, Australia]	Rejected. The reviewer does not offer any evidence in support of his claim.
10-940	10	21	30	21	30	Replace "10% confidence level" with "10% significance level". [Francis Zwiers, Canada]	Accepted. Changed as suggested.
10-941	10	21	30			90% confidence level? [Peter Guttorp, United States of America]	Taken into account. Replaced with '10% significance level'.
10-942	10	21	33	21	33	Insert "since 1951" after "warming trends". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Changed as suggested.
10-943	10	21	36	21	37	Suggest replace "attribution" with "detection"; if you detect on a global scale in this way, you might still not be confident in using the local results to quantify the contributions of external forcing to local changes. [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-944	10	21	37	21	40	This statement is unsustainable given that natural climate forces, especially the ENSO, are poorly simulated by models. [John McLean, Australia]	Rejected. The reviewer does not offer any evidence in support of his claim.

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10-945	10	21	39			Do 91% have sufficient coverage, of are 91% of those with sufficient coverage showing trends inconsistent with natural forcing etc? [Peter Guttorp, United States of America]	Taken into account. It is the second of these. We have re-phrased to clarify this.
10-946	10	21	42	21	43	This statement is unsustainable given that natural climate forces, especially the ENSO, are poorly simulated by models. Figure 7 of McLean et al (2009) showed the close relationship between average global temperature and the ENSO conditions of about 7 months earlier. The relationship was so close that little if any warming remained to be accounted for by other forces. [John McLean, Australia]	Rejected. See response to 10-728.
10-947	10	21	42	21	43	Is this a collective statement or one that applies to each region individually? [Dáithí Stone, United States of America]	Taken into account. It applies to each one individually. Re-phrased to make this clear.
10-948	10	21	42	21	47	This overall section 10.3.1.1.4 is well-written. [Martin Hoerling, United States of America]	Noted.
10-949	10	21	43	21	44	See my previous comment concerning the assessment for Antarctica (referring to page 20, lines 25-27). The suggestion is say that an assessment is currently not possible. [Francis Zwiers, Canada]	Rejected. We make an assessment but at the low confidence level.
10-950	10	21	44	21	47	Is one of your summary conclusions that this field of research is hard? [Dáithí Stone, United States of America]	Noted. We already say something along these lines in the text.
10-951	10	21	47	21	47	It might be useful to add a further sentence saying what has been learned at smaller scales. Perhaps something like "Nevertheless, statistically significant warming trends are observed at a majority of grid cells, and the observed warming is inconsistent with estimates of possible warming due to natural causes at the great majority of grid cells with sufficient observational coverage". [Francis Zwiers, Canada]	Accepted. Suggested change made.
10-952	10	22	5	22	7	akward formulation with expected opposing signals, this is not true as depletion causes both stratospheric and troposheric cooling [Emma Daniels, Netherlands]	Accepted- text revised. A sentence is added which clarifyes that the impact of stratospheric ozone depletion on the troposphere is small.
10-953	10	22	12	23	13	This section is written in a manner that tries to overlook the major model problem - models significantly over-warm the troposphere, espectially in the tropics. This is most noticeable since 1979. By not showing the actual results from 1979 which reveal this significant discrepancy, it will be easy to demonstrate the lack of objectivity of this section. Using 1961-2010 appears as a dodge to distract from the real problem in my view. Indeed, the forcing since 1979 has been the strongest for warming, yet the temperature record does not agree with the models. Another feature overlooked is the strong and significant difference between the ratio of tropospheric trends to surface trends, especially in the tropics. The observed ratio of trends is 0.8 to 1.0 while models are signficantly higher (see Chapter 9). This is a model metric that should be shown to the readers - if not, this section will be demonstrated to be misleading (easily shown when all the numbers are availalbe.) Someone will expose these results, and it should come from the IPCC itself, otherwise accusations will be well-founded that there is apparent bias in the presentation of material. Fig.10.7 should at the bare minimum show 1979-2012 comparisons and the reader will see the problems. If the IPCC doesn't do it, others will (as mentioned) to the IPCC's detriment. [John Christy, United States of America]	Taken into account: In the main body of the chapter we show trends since 1961 since that is when the radiosonde record begins. A figure is added to the supplementary material that adapts Figure 10.7 for the period 1979 to 2010. This supplementary figure illustrates that simulated free troposphere warming trends are larger than observations especially in the tropics consistent with other studies (Chapter 9). The free troposphere warming bias is consistent with a surface warming bias that results from the fact that models do not capture the lack of warming observed since 1998. Potential causes for this bias are discussed in a new box in chapter 9. In text we discuss that the disrepency between observed and simulated warming trend above 300hPa. We add in the text that these differences are more marked in trends since 1979 togher with a larger uncertainty in observations consistent with Seidel et al (2012). There are no detection and attribution studies that investigate the causes for changes of tropical lapse rate trends. We note that there are large uncertainties in the rate of observed tropospheric warming in the tropics (Chapter 2) as well as observed lapse rate changes since 1979 (Seidel et al 2012).
10-954	10	22	15	22	16	The evidence is not actually that clear. See Seidel et al., GRL, doi:10.1029/2012GL053850 (in press) showing large observational uncertainties from radiosonde data. [Melissa Free, United States of America]	Accepted-text revised
10-955	10	22	17	22	18	Suggest replacing "are not understood and include" with "are not fully understood, but include". [Francis Zwiers, Canada]	Taken into account-text is modified based on other comments and changes of chapter 9 statement

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10-956	10	22	22	22	22	Remove brackets around "Lott et al., 2012", and change ", detect influences" to "have detected influences" [Government of Canada]	First part: accepted-text revised, second part rejected, writing style is present tense
10-957	10	22	22	22	22	In Fig. 7, state explicitly the forcing included in the "anthropogenic forcing" simulations, and likewise in the "greenhouse gas forcing only" simulations. [Martin Hoerling, United States of America]	Taken into account-text earlier in the section is modified to describe more clearly which forcing components are included in the natural and ghg only experiment,
10-958	10	22	22	22	22	Open this paragraph with the sentence "McLean et al (2009) showed the close relationship between ENSO conditions and average global lower tropospheric temperature seven months later." (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) [John McLean, Australia]	Rejected- While ENSO affects tropospheric and regional stratospheric temperature on the interannual time scale, there is no evidence that ENSO has an effect on multidecadal time scale (long-term trend).
10-959	10	22	25	22	29	Sentence is difficult to read. Suggest simplifying if possible. [Government of Canada]	Accepted-text revised
10-960	10	22	27			How were the six models chosen? [Peter Guttorp, United States of America]	Accepted-text revised
10-961	10	22	31	22	32	Rather than making the assessment by saying "very likely not", it would be clearer if you could say "It is very unlikely that natural forcings alone could have causes the observed tropospheric warming (green profiles)." [Francis Zwiers, Canada]	Accepted-text revised
10-962	10	22	35	22	35	Suggest replacing "which warm" with "which both warm", to make it clear that both absorbing aersols and trop ozone warm the troposphere. [Francis Zwiers, Canada]	Accepted-text revised accordingly
10-963	10	22	39	22	41	This is a bit confusing, because earlier you seemed to buy onto an argument (Kaufmann et al 2011; page 19, line 19) that aerosols could have been partly responsible for the lack of further warming during the past decade. Also, isn't the aerosol loading record disputed? [Francis Zwiers, Canada]	Accepted-text removed.
10-964	10	22	41	22	41	Change"reanalysis" to "observational". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted-text revised
10-965	10	22	41	22	41	These are observational and not reanalysis products. Reanalysis should be reserved exclusively for numerical reanalyses products such as ERA-40 / MERRA which these products under discussion are not. [Peter Thorne, United States of America]	Accepted-text revised
10-966	10	22	41			There is reference here to "three reanalysis datasets" which makes no sense. Should "reanalysis" should be replaced by "radiosonde"? [Adrian Simmons, United Kingdom]	Accepted-text revised
10-967	10	22	42	22	43	This should probably cross-reference to chapter 2. [Francis Zwiers, Canada]	Accepted-text revised
10-968	10	22	46	22	46	Haimberger et al should be a 2012 paper. It is referenced in Chapter 2. There is no 2011 Haimberger et al paper in existence to my knowledge. [Peter Thorne, United States of America]	Accepted-text revised
10-969	10	22	55	22	56	Increase instead of "decrease"? [Albert Klein Tank, Netherlands]	Accepted-text revised
10-970	10	22	55	22	56	S/N really decreasing with increasing record length? The opposite should be expected. [Christian-D. Schoenwiese, Germany]	Accepted-text revised
10-971	10	22	55	22	56	This sentence is a bit confusing as it is not fully clear in the text that meteorological data refers here to predictability of the first kind. [Laurent Terray, France]	Accepted-text revised
10-972	10	22	55	22	56	I don't understand what this sentence is saying - wouldn't I generally expect S/N ratio to increase with a longer record (assuming that the signal of interest is present for the entire period that is covered by the record)? [Francis Zwiers, Canada]	Accepted-text revised
10-973	10	22	55	23	4	Overall, this paragraph is hard to understand. It might help if the signal-to-noise ratio that is being discussed were to be described to the reader, and if it could be made clear that what is being described here is essentially the result of a non-optimal fingerprint analysis rather than one that is optimized. [Francis Zwiers, Canada]	Accepted-text revised.

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10-974	10	22	56	22	56	I think it must read "increases with increasing record length", because the sentence is about the ratio of signal to noise. [Jochem Marotzke, Germany]	Accepted-text revised
10-975	10	22	57	22	57	5% significance level, not 95% confidence level. This is a very common error of statistical language. The thing that the analyst controls when doing a test of significance is the significance level, which is the probability of incorrectly rejecting the null hypothesis when it is true. Tests of a parameter (such as the mean or variance) are sometimes performed by constructing an interval estimate (confidence interval) of the parameter, and then determining whether the value specified in the null hypothesis is included in the confidence interval. If not, the null hypothesis is rejected at a significance level that is 100% minus the coverage of the confidence interval (typically 90% or 95%). It is not rejected at the corresponding "confidence level". Using that term suggests confidence that the alternative hypothesis is correct - which is something that cannot be inferred from standard "frequentist" tests; you would have to be a Bayesian statistician to determin whether the alternative hypothesis is more likely than the null hypothesis. See also page 23, line 2 (which should claim a significance level of 1% or less). [Francis Zwiers, Canada]	Accepted-text revised
10-976	10	22	58	23	3	This claim about anthropogenic forcing cannot be sustained unless it can be shown that climate models are 100% accurate when it comes to simulating natural forces. McLean et al (2009) showed that the ENSO could account for most of the variation in average global temperature, save for irregular cooling due to volcanic eruptions and the short-term "noise" of other climate forces. [John McLean, Australia]	Rejected-see comment 10-958
10-977	10	23	4			The statement "found to be detectable in over 50% of all tests" is confusing. It sounds like a weak statement but should not be. [Chris Forest, United States of America]	Accepted-text revised.
10-978	10	23	6	23	13	The AR4 also explicitly stated that "it is very unlikely that the contributon from solar forcing to the warming of the last 50yrs was larger than the GHG forcing". The results of Lott et al.(2012) appear to support, and perhaps even strengthen, that statement (at least regarding overall natural radiative forcing). I recommend that the authors add a statemant about the likelihood that the observed zonal mean warming trends in the troposphere since 1961 could be due to natural forcing. [Martin Hoerling, United States of America]	Rejected-because of uncertainties in free atmosphere-observations we wish not to elevate such a statement in the summary.
10-979	10	23	10	23	13	Why does this sentence introduce a new start year for the claimed warming? At various points in this WGI contribution we've seen 1950s, "mid twentieth century", 1970 and 1979. Some consistency please! [John McLean, Australia]	Rejected-attribution statements are made based on availability of observational records with sufficient spatial resolution.
10-980	10	23	11	23	13	And McLean et al (2009) showed that it was highly unlikely that anthropogenic forcing made any more than a negligible contribution, if any, because the ENSO conditions seven months earlier showed a very close link, moreover the physical processes by which the ENSO would cause temperature variations are well recognised and documented. [John McLean, Australia]	Rejected-see comment 10-958
10-981	10	23	15			Section 10.3.1.2.2 It seems highly likely that the observed stratopheric temperature variation can also be attributed to the ENSO, after all McLean et al (2009) showed that lower tropospheric temperatures could be linked to ENSO and it seems unlikely that Stratospheric temperatures could have a completely different driver. [John McLean, Australia]	Rejected-see comment 10-958
10-982	10	23	16	23	16	Suggest replacing "did not evolve" with "have not evolved". [Francis Zwiers, Canada]	Accepted-text revised
10-983	10	23	16	24	49	The lack of trend in stratospheric temperatures from 1998 onwards should perhaps also be discussed. [Government of United States of America]	Accepted-text revised
10-984	10	23	17			Words such as "with sufficient regularity and spatial coverage" are needed after "has been observed". There were observations of the stratosphere before 1958. [Adrian Simmons, United Kingdom]	Accepted-text revised
10-985	10	23	18	23	18	Delete "Furthermore", since this is an elaboration of the previous sentence, not an additional factor contributing to the non-uniform evolution. [Francis Zwiers, Canada]	Accepted-text revised
10-986	10	23	23	23	35	Define low-top and high-top, in terms of the pressure top of the models in question. Confirm that the different sensitivities of low vs high top models is due solely to the height of the top model layer, as this section implies. Is the apparently different sensitivities in high vs low-top models merely a function of the top, or is it due to other physical parameterzations? For instance, does a low-top model with a sponge layer exhibit similar	Taken into account-we now state that that the stratopause is at an altitude of about 50km, we also state that these model have incorporated an improved stratospheric physics

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						sensititivty as a high-top model? [Martin Hoerling, United States of America]	
10-987	10	23	24	23	24	typo: "One" [Albert Klein Tank, Netherlands]	Accepted-text revised
10-988	10	23	39	23	44	This discussion ignores the presence of residual cooling biases in radiosonde records in the LS as documented in the literature by the dataset providers. This discussion occurs in Section 2.4.4.4 [Peter Thorne, United States of America]	Accepted-text revised
10-989	10	23	40	23	41	It should be pointed out that it may not be wholly undesirable that the CMIP3 models show less cooling than the radiosonde data. Unhomogenised radiosonde data show a spurious cooling trend due to radiative effects on the measurements that were either larger for the older instruments or have been corrected as part of measurement-station data processing in more recent years. Homogenised data may still show a residual effect of this, as noted in Chapter 2 (section 2.4.4.2). This could be noted as one cause of the discrepancy between the models and the radiosonde data. [Adrian Simmons, United Kingdom]	Accepted-text revised
10-990	10	23	48	23	48	For clarity it should be stated explicitly that the datsets under consideration are all satellite data records given that earlier paragraphs have been discussing radiosondes. [Peter Thorne, United States of America]	Accepted-text revised
10-991	10	23	48	23	50	I would suggest the slight underestimation of the TLS trends by the MME (see Santer et al. 2012) [Laurent Terray, France]	Accepted-text revised
10-992	10	23	49	23	49	Signal to noise ratio really as large as 21 to 29? [Christian-D. Schoenwiese, Germany]	Yes.
10-993	10	23	51	23	52	This is a discussion of a modelling study, and thus it might be appropriate to use slightly more cautious wording. On line 51, perhaps replace "determine" with "estimate" or "suggest", and on line 52, perhaps replace "emerges by the early 20th century" with "may already have emerged early in the 20th century". To help interpret this statement, it would be useful to also say how "emergence" is defined, since this term seems to be used in different ways in the current literature. [Francis Zwiers, Canada]	Accepted-we remove this statement because it is solely a modeling study without observations analyzed.
10-994	10	23	52	23	52	Is this meant to be early 20th century when there were no stratospheric observations? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Yes, text has been removed. See comment 10-993
10-995	10	23	53	2	53	"simulated". [J. Graham Cogley, Canada]	Accepted-text revised
10-996	10	23	53	23	53	typo: "simulated" [Albert Klein Tank, Netherlands]	Accepted-text revised
10-997	10	23	53	23	53	Typo: "simulated" [Jochem Marotzke, Germany]	Accepted-text revised
10-998	10	23	53	23	53	simulated not simulate [Peter Thorne, United States of America]	Accepted-text revised
10-999	10	23	56	23	56	Remove brackets around "Gillett et al., 2011b" [Government of Canada]	Accepted-text revised
10-1000	10	23	57	23	57	Capitalize Climate [Peter Thorne, United States of America]	Rewritten ..chemistry-climate models and some CMIP5 models..
10-1001	10	24	2	24	2	"ODS (ozone-depleting substances)". [J. Graham Cogley, Canada]	Accepted-text revised
10-1002	10	24	2	24	2	Introduce "ODS" [Albert Klein Tank, Netherlands]	Accepted-text revised
10-1003	10	24	2	24	2	Define ODS. [Francis Zwiers, Canada]	Accepted-text revised
10-1004	10	24	2	24	5	Spell out Ozone Depleting Substances (ODS) and indicate what these substances are (or provide suitable reference). [Martin Hoerling, United States of America]	Accepted-text revised
10-1005	10	24	7	24	7	"effects". [J. Graham Cogley, Canada]	Accepted-text revised
10-1006	10	24	7	24	7	"...explained by the combined effect…" [James Renwick, New Zealand]	Rejected: The next sentence in text gives the explanaition.
10-1007	10	24	15	24	19	The disctinction between Wmgghg and Allforc is not well described in the caption and seems to be inconsistent with the text above. [Chris Forest, United States of America]	Accepted-text revised

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10-1008	10	24	24	3	4	Define ODF = Ozone Depleting Substances? [Simon Tett, United Kingdom]	Accepted-text revised
10-1009	10	24	28	24	36	Appears repetitive, especially so late in the chapter. [Jochem Marotzke, Germany]	Rejected: Subsections are summarized throughout the chapter
10-1010	10	24	33	24	35	You claim about anthropogenic forcing is unsustainable while the modelling of natural climate forces continues to be less than 100% complete and 100% accurate. That which you attribute to anthropogenic factors might well be found to be due to natural forces if the modelling was accurate. [John McLean, Australia]	Rejected:
10-1011	10	24	33			I would prefer "long-lived" to "well-mixed". Either way, terminology needs to be made uniform across this WG1 report. Please see comments 235-239 on Chapter 8. [Adrian Simmons, United Kingdom]	Taken into account with use of well mixed ghgs.
10-1012	10	24	36	24	38	Is the phrase "dominated by ozone depleting substances" correct? Perhaps it is meant "dominated by ozone destruction caused by ozone depleting substances"? The sentence can lead to misunderstandings. [Government of Germany]	Accepted-text revised
10-1013	10	24	47	24	49	Italicize "very likely"? [Government of Canada]	corrected
10-1014	10	24	53	24	56	What about the fact that, unlike temperature, there may (often) not be a signal, or at least it won't be that big? [Dáithí Stone, United States of America]	Taken into account - but the point here is that, even if a signal is present, then D&A is made difficult for the reasons presented in this sentence.
10-1015	10	24	57			"global" has 2 opposite potential meanings, "averaged over the globe" & "varying over the globe". It is completely unclear which is meant "global" used here has 2 opposite potential meanings, "averaged over the globe" & "varying over the globe". It is completely unclear which is meant here [William Ingram, United Kingdom]	accepted - wording changed
10-1016	10	25	17			Section 10.3.2.1 Changes in Atmospheric Water Vapour: Given the discussion on recent work on the use of homogenised rediaosonde records in detection of tropospheric humidity trends in section 2.5.6.1 Radiosonde, some mention of this work and the pertinent references (Dai et al., 2011; Durre et al., 2009; McCarthy et al., 2009) would seem appropriate here. [Anthony Hirst, Australia]	rejected - Ch 2 is the proper place for assessment of these papers and we prefer to minimize redundancy
10-1017	10	25	21			“averaged” seems to be needed before “over” (or I don't know what is meant) [William Ingram, United Kingdom]	accepted - wording changed
10-1018	10	25	24	25	25	Why "should"? And reference to Section 2.3 is wrong. Should be Section 2.5.5 [Albert Klein Tank, Netherlands]	"should" seems grammatically correct. Reference to Ch 2 changed
10-1019	10	25	24	25	32	Ch. 2 p45 l3-8 says ERA-iterim reveals an overall reduction in relative humidity since 2000 - is this consistent? [European Union]	Yes. Reduction in relative humidity is discussed.
10-1020	10	25	25	25	25	The section in Chapter 2 is now Section 2.5 and not 2.3. [Peter Thorne, United States of America]	accepted
10-1021	10	25	26	25	26	Change "atmospheric humidity" to "specific humidity". [J. Graham Cogley, Canada]	accepted
10-1022	10	25	27	25	29	Willett and co-authors undertook a subsequent intercomparison with models, published in ERL which should be characterized and cited here as it increases confidence in the initial findings of Willett et al., 2007 which compared just a single model to HadCRUH. [Peter Thorne, United States of America]	accepted Willett et al (2010) cited
10-1023	10	25	27			Chapter 7 (7.2.4) seems more relevant [William Ingram, United Kingdom]	Noted although in the revised text we don’t have the same sentence.
10-1024	10	25	29	25	33	Similar caveats pertain to the surface temperature issue. Short apparent trends at the end of a timeseries give an arresting visual cue interpretation but need not be significant harbingers of change. So, I find this discussion a hostage to fortune. As much as anything I am not sure what relevance it has to this chapter as it has nothing to do with models and attribution. I would strongly suggest this text get deleted and it be left to Chapter 2 to characterize this purely observational issue. Work submitted by Willett and colleagues after the IPCC cut-off suggests that the 'pause' has started to reverse. This cannot be included but does suggest this passage may be dated very quickly. [Peter Thorne, United States of America]	Taken into account - but we need to maintain consistency with respect to cutoff dates for publication.

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10-1025	10	25	29		33	Grammar lost [William Ingram, United Kingdom]	accepted - wording changed
10-1026	10	25	30			Capitalize “Interim” [William Ingram, United Kingdom]	accepted
10-1027	10	25	33	25	33	Change "temperatures. (Simmons et al. (2010)" to "temperatures (Simmons et al, 2010)." [Government of Canada]	accepted - this is a typo associated with the citation software
10-1028	10	25	33			Full stop misplaced [William Ingram, United Kingdom]	accepted
10-1029	10	25	40	25	41	It would be useful to state the conclusion that is drawn from this - presumably that the result is robust to the choice of the subset of models that is used to estimate the response to forcing. [Francis Zwiers, Canada]	accepted - this conclusion reiterated in the text
10-1030	10	25	45	25	45	Lanzante (2009) points out problems with radiosonde temperature, not water vapour, data. Rosenlof and Reid (2008) used these temperatures as part of a causal chain explaining stratospheric water vapour trends so their explanation, but not necessarily the actual stratospheric water vapour data, would have been affected by the poor radiosonde temperature data. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this paragraph now deleted
10-1031	10	25	45	25	48	I am not sure that you can assume that the reader is knowledgeable as to why the WV in the UTLS region is of particular importance. Is this discussed in any other chapter where the reader can be referred to? If it is not then do you need to expand the discussion here accordingly? [Peter Thorne, United States of America]	Taken into account - this paragraph now deleted
10-1032	10	25	50	25	51	Is "at the Earth's surface" needed as part of this medium confidence judgement? In main discussion, it is stated that the lower tropospheric is robust to anthropogenic forcing (across 22 models), but stratospheric water vapour variability is still not well explained". Consider also inserting a sentence explaining for readers what is meant between 'water vapour' and 'moisture content' as these are used interchangeably. [Government of Canada]	Accepted - wording is clarified
10-1033	10	25	50	25	51	"In summary, an anthropogenic contribution to increases in atmospheric moisture content at Earth's surface is found with medium confidence." This finding is unsupported by the 2012 NVAP-M global water vapor study. As discussed above in lines 28-40, this paper is highly significant, for it concludes, "Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data." Policy makers and non-specialist readers must be made aware of this finding; that it is based on upper air soundings, GPS data and multiple satellite surveys over land and sea; and that it is at odds with some earlier papers. Many cited papers in AR5 have yet to be published, but the first NVAP-M paper was published earlier this year and is definitely worthy of citation: Thomas H. Vonder Haar, Janice L. Bytheway and John M. Forsythe. Weather and climate analyses using improved global water vapor observations. GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L15802, 6 PP., 2012. doi:10.1029/2012GL052094. [Forrest Mims, United States of America]	Rejected - The paper cited in this comment does not carry out detection or attribution assessment, which is the focus of Ch 10. Development of new observaton-based data sets is the purview of Ch 2 of AR5.
10-1034	10	25	51	25	51	"a recent levelling-off of the long-term …". [J. Graham Cogley, Canada]	accepted - wording changed
10-1035	10	25	51	25	51	Why medium confidence and not a likelihood statement? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected - IPCC guidance suggests that likelihood statements should be reserved for higher-confidence assessments
10-1036	10	25	51	25	53	As per my earlier comment this seems to be an observational issue outside the purview of the chapter. [Peter Thorne, United States of America]	See response to comment 10-1024.
10-1037	10	25	53	24	53	"Detection and anthropogenic attribution studies of change in …". [J. Graham Cogley, Canada]	accepted - wording changed
10-1038	10	25	56	25	56	Section 10.3.2.2: Changes in Precipitation. There is a lack of discussion (none, as far as can be discerned) about the comparison of model simulations with observed precipitation changes in the Asian monsoon region e.g., papers by Ramanathan and colleagues, the study by Bollasina et al., 2011 (Anthropogenic aerosols and the weakening of the South Asian summer monsoon. Science, 334(6055), doi:10.1126/science.1204994). These, together with an even more recent study (Ganguly et al., JGR, 2012) provide convincing demonstration of the dominant aerosol influence on S. Asian precipitation. More generally, one of the effects due to Northern Hemisphere anthropogenic aerosol forcing has been shown to impact the tropical precipitation and the ITCZ, somewhat consistently across various models. Changes of opposite signs in precip on either side of the equator due to asymmetric hemispheric forcings is an effect that has come across from several model runs. This rather robust result from models, well explained physically (e.g., Ramaswamy and Chen, GRL, 24, 567,	Rejected - changes in monsoons are assessed and discussed in Chapter 14

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						1997; Lohmann and Rotstayn, 200x; Ming and Ramaswamy, J. Climate, 2009; Bollasina et al., Science , 2011) is worth pointing out. Aerosol forcing effects on the tropical precip may be one of the most important impacts caused by aerosols on the global hydrologic cycle. [Venkatachalam Ramaswamy, United States of America]	
10-1039	10	25				Section 10.3.2.2: in this section, it is very visible that the latitude dependence of the observed precipitation signal is not matched by the models' latitude dependence yet there is still a significant detection result. This does not seem to be reflected in the Summary statement at the end of this section where it states "medium confidence." The thinking behind this confidence statement could be clarified in the Summary or added previously in the section. [Chris Forest, United States of America]	Taken into account - we have clarified our justification of medium confidence. The newly cited Scheff & Friersen papers (comment 10-1041) bolster our justification of medium confidence.
10-1040	10	26	1	26	15	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - the published literature supports the assessment in the text
10-1041	10	26	1	27	35	IMO this should refer to Scheff & Frierson (2012a,b), who show in both CMIP3 & CMIP5 that while changes in P-E are very consistently dominated by a “wet-get-wetter” signal, changes in P are very consistently dominated by a poleward shift of the storm tracks. This does make much better physical sense of the changes simulated by the GCMs, so I do think it's worth saying here. [William Ingram, United Kingdom]	accepted - these papers now cited
10-1042	10	26	4	26	4	Balani Sarojini et al. is cited as "in preparation" - worth keeping an eye on. [Jochem Marotzke, Germany]	This paper is through the review process and has now been published.
10-1043	10	26	4	26	4	Please consider including Tapiador et al. 2010 to strenghten the illustration of the large observational uncertainties. The line would the read "(…) Polson et al. 2012, Tapiador 2010). The reference is: Tapiador, F.J., 2010. A Joint Estimate of the Precipitation Climate Signal in Europe using Eight Regional Models and Five Observational Datasets. Journal of Climate, 23, 7, 1719-1738. Table 2 quantifies those uncertainties and figure 2 provides a comparison between observations and model data. [FRANCISCO J. TAPIADOR, SPAIN]	Accepted
10-1044	10	26	4	26	6	The statements here about uncertainties are not supported by either Noake et al or chapter 2. Balani-Sarojini et al only identify large uncertainties 'in some parts of the world' - update. [European Union]	Accepted - wording modified here
10-1045	10	26	6	26	6	Chapter 2 reference is out of context. Chapter 2 does not state whether or not a dataset is reliable for D/A. [Albert Klein Tank, Netherlands]	Accepted - wording modified here
10-1046	10	26	8		9	A model can't indicate what humans expect. Drop “are expected to”, & maybe weaken “indicate” (or change it to e.g. “should be expected to”) [William Ingram, United Kingdom]	Accepted - wording modified here
10-1047	10	26	10	26	10	Unclear to me what "much more indistinct" means here. [Albert Klein Tank, Netherlands]	Accepted - wording modified here
10-1048	10	26	10	26	10	Suggest inserting "in many regions" after precipitation, since there are a few regions (notably Europe and the US) with relatively dense precipitation observing networks. You might also cite Wan et al (2012, JGR, submitted) in this context. Wan, H., X. Zhang, F.W. Zwiers, H. Shiogama, 2012: Effect of data coverage on the estimation of mean and variability of precipitation at global and regional scales. Journal of Geophysical Research, submitted. [Francis Zwiers, Canada]	Accepted - wording modified
10-1049	10	26	14	26	14	"that are reasonably robust to use of different observational datasets". Even more clear and free of jargon might be "observed in several different datasets". [J. Graham Cogley, Canada]	Accepted - wording modified here
10-1050	10	26	18	26	18	I think it should read "Global and zonal mean changes…" [Jochem Marotzke, Germany]	Accepted
10-1051	10	26	18	26	25	I am not sure that the t-test is suitable for this case. The t-test assumes a normal distribution, and annual mean precipitation is not necessarily normally-distributed. Some sort of explanation is required here apart from referencing the original paper. I reckon that a KS test would be more suitable for this case. [FRANCISCO J. TAPIADOR, SPAIN]	Rejected - The results and analysis technique shown in the figure have undergone peer review, which in this case we accept.
10-1052	10	26	19	26	19	What are the observations in Figure 10.9? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - wording added to caption. The observations are updated gridded values (derived from GHCN) as described by Zhang et al. (2007)

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10-1053	10	26	25			Misplaced bracket [William Ingram, United Kingdom]	Accepted - typo fixed
10-1054	10	26	27	26	27	Suggest replacing "carried out" with "attempted" - the scaling factors are a bit large to be more definitive, I think. [Francis Zwiers, Canada]	Accepted
10-1055	10	26	27	26	42	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - the published literature cited here support the assessment in the text
10-1056	10	26	30	26	31	"identify the fingerprint". Change "both over the period" to "over the periods", and end the sentence at "1950--1999". [J. Graham Cogley, Canada]	accepted - this sentence has been rewritten
10-1057	10	26	30		31	It reads as if this is with annual means, but this needs to be explicit [William Ingram, United Kingdom]	accepted - this sentence does indeed refer to annual means
10-1058	10	26	30			“fingerpring” ->“fingerprint” [William Ingram, United Kingdom]	accepted - typo fixed
10-1059	10	26	32	26	33	"during boreal spring … and during boreal winter". [J. Graham Cogley, Canada]	taken into account - wording modified in a slightly different way to make the suggested clarification
10-1060	10	26	36			“the” & “seasons” add nothing: cut [William Ingram, United Kingdom]	Accepted - wording changed
10-1061	10	26	38	26	38	Insert a comma after "considered". [Francis Zwiers, Canada]	accepted - typo fixed
10-1062	10	26	38			“results” ->“result” [William Ingram, United Kingdom]	accepted - typo fixed
10-1063	10	26	38			comma needed before “Polson” [William Ingram, United Kingdom]	accepted - typo fixed
10-1064	10	26	39			“fingerprint” ->“fingerprints” [William Ingram, United Kingdom]	accepted - typo fixed
10-1065	10	26	44	26	45	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - the published literature cited here support the assessment in the text
10-1066	10	26	45	26	46	"an attributable human influence". "observed". [J. Graham Cogley, Canada]	Accepted - wording changed
10-1067	10	26	55	26	55	Can the upper right panel be explained? Figure comes from a paper, but this panel seems to have lots of simulation acronyms. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account - figure modified
10-1068	10	26				Oreskaug et al. (Tellus A, 2010) got the opposite result for Norway. [Peter Guttorp, United States of America]	Rejected - the cited paper is a model evaluation study, and the comment does not refer to specific line(s) of text. We are unsure how this fits into the assessment in our chapter.
10-1069	10	27	14			Needs “blue and orange” before “vertical” like 17-8 [William Ingram, United Kingdom]	accepted - caption changed
10-1070	10	27	21	27	21	Be cautious here, precipitation has a high coefficient of variation in the subtropics- changes are hard to to detect [John Mitchell, United Kingdom]	Taken into account - the high coeff of variation is implicitly included in the assessments of drought.
10-1071	10	27	21	27	28	I think this set of analyses does include some model based attribution but as currently documented it is implied to be purely observational in nature and therefore outside chapter remit. If I am correct and they are attribution type studies the text needs rewriting to make this substantively clearer than is presently the case. [Peter Thorne, United States of America]	Taken into account [text to be modified in Sec 10.6.1]
10-1072	10	27	21	27	28	This paragraph on drying trends and drought would seem better placed in Section 10.6.1. Please also cross-reference to Chapter 2, and SREX Chapter 3. [Thomas Stocker/ WGI TSU, Switzerland]	accepted - paragraph removed from 10.3.2, material now incorporated into 10.6.1.3
10-1073	10	27	23	27	25	The Australian drought was not significantly different to the sustained drought across 1937-1946, a period whose conditions the IPCC blames more on natural forces than anthropogenic forces. Also in Eastern Australia, where this drought occurred, the average annual rainfall from 1901 to 1950 was less than the average rainfall since 1950. From 1922 to 1949 inclusive (i.e. 28 years) the average annual rainfall exceeded the 1961-90 average on just 9 occasions and on 3 of those times by less than 11mm. In contrast, on 11 of the 28 deficit years the shortfall exceeded 100mm and the minimum deficit in any year was 38mm. The drought	Taken into account [text to be modified in Sec 10.6.1]

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						from 2001 to 2006 was small by comparison. [John McLean, Australia]	
10-1074	10	27	23	27	26	Delete both "conditions". [J. Graham Cogley, Canada]	accepted
10-1075	10	27	24		30	Is the Australian drought been attributed to anthropogenic climate change - the text seems to imply this but i was not aware of any study that robustly does this [John Church, Australia]	Taken into account [text to be modified in Sec 10.6.1]
10-1076	10	27	28	27	28	Cross-refer to Section 10.6. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted
10-1077	10	27	30	27	35	It would be more reasonable to assign a “low to medium confidence” to the conclusion, considering the large uncertainties in observations and simulations. No significant long-term trend has been detected for global and sub-continental average precipitation series for both the past 100 years and 50 years. The observed multi-decadal variability in large scale precipitation, including that found for mainland China for the past century and 5 decades (Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q. and Zhang, J., 2007, Detection, causes and projection of climate change over China: an overview of recent progress, Advance in Atmospheric Sciences, 24 (6), 954-971), is obviously of nature. [Guoyu Ren, China]	Taken into account - our assessment is that "medium confidence" is justified based on the studies cited. For individual subcontinental regions the results might be different.
10-1078	10	27	35	27	35	Why "climatic" [Albert Klein Tank, Netherlands]	Rejected - we maintain "climatic changes" here for clarity, in accordance with the Glossary.
10-1079	10	27	37	28	25	It is unclear if human influence on hydrology includes hydraulic management such as damming and river rectification. Probably yes as it is detailed in chapter 13. Nevertheless this could be clearly mentioned (again) in this section. [European Union]	Accepted - A sentence from the third paragraph of this section is moved upward to the first paragraph.
10-1080	10	27	39	27	39	Suggest replacing "summarizes" with "assesses" to reinforce that this is an assessment and not a review. [Francis Zwiers, Canada]	accepted
10-1081	10	27	44	27	44	Replace "were not" with "was not". [Francis Zwiers, Canada]	Rejected - We consider detection and attribution to be separate activities and separate nouns in this sentence, hence they need a plural verb.
10-1082	10	27	48	27	48	Insert a comma after "short". [Francis Zwiers, Canada]	accepted
10-1083	10	27	49	27	49	According to page 8, lines 11-12 "definitive attribution" seems impossible (always less than 100%). [Albert Klein Tank, Netherlands]	accepted - wording changed
10-1084	10	27	52	27	53	Simple hydrology says that you've put the cart before the horse. Increased temperatures are not the cause of drier conditions; drier conditions are the cause of increased temperatures. The reason is that an absence of surface moisture means that little or no heat is taken by the process of evaporation and virtually all of the heat can go into warming the Earth's surface. [John McLean, Australia]	Taken into account - there may indeed be land-atmosphere feedbacks but forced warming is expected to change the surface water budget. The phrase "associated with increased temperature" is deleted, considering that the identified observational change is simply drier soils.
10-1085	10	27	53	27	56	This looks suspiciously like flawed reasoning. Increased temperatures are not the cause of drier conditions; drier conditions are the cause of increased temperatures. The absence of surface moisture means that little or no heat is taken by the process of evaporation and virtually all of the heat can go into warming the Earth's surface, ergo near-surface temperatures rise under sustained dry conditions. [John McLean, Australia]	See response to previous comment
10-1086	10	27	54	27	54	"identifies the effects of anthropogenic". [J. Graham Cogley, Canada]	accepted - typo fixed
10-1087	10	27	58	27	58	Even centuries for some rivers? [Albert Klein Tank, Netherlands]	Taken into account - this sentence is moved (cf comment 10-1079); the phrase mentioning time scales is no longer needed and has been removed
10-1088	10	27	58	27	58	Replace "are subject" with "are often subject" [Francis Zwiers, Canada]	accepted
10-1089	10	27	59	28	1	Change "assess climatic change" to "attribute detected hydrologic changes to climatic change". [J. Graham Cogley, Canada]	accepted
10-1090	10	28	3	28	3	PDO?? [European Union]	accepted - wording changed to remove this

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							abbreviation
10-1091	10	28	5	28	5	Unclear what this consistency means? Consistent with observed precipitation changes or model simulations or…? [Albert Klein Tank, Netherlands]	accepted - wording of this sentence changed
10-1092	10	28	7	28	7	Suggest replacing "could" with "should". Could suggests that this might be true under some (unspecified) set of circumstances, so using that word begs the question about what those circumstances might be. In contrast, "should" indicates an expectation that changes in precipitation received by high latitude basins will be reflected in changes in stream flow. [Francis Zwiers, Canada]	accepted
10-1093	10	28	7	28	9	This sentence appears to require a reference. [J. Graham Cogley, Canada]	accepted -- wording augmented
10-1094	10	28	7	28	9	This sentence needs a supporting reference. [European Union]	accepted -- wording augmented
10-1095	10	28	7	28	9	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	rejected - this assertion not supported by the refereed literature
10-1096	10	28	11	28	14	Barnett et all (2008) seems confused about basic hydrology. Increased temperatures are not the cause of drier conditions; drier conditions are the cause of increased temperatures. The absence or severe reduction in surface moisture means that little or no heat is taken by the process of evaporation and virtually all of the heat can go into warming the Earth's surface, which in turn means higher near-surface temperatures. [John McLean, Australia]	Rejected - Barnett et al imposed neither a drier surface, nor increased temperature, in any of the model simulations they assessed. Land-atmosphere feedbacks are included in their analysis.
10-1097	10	28	12	28	12	The description of the runoff term that is used in the Barnett et al (2008) study is not quite correct. The study used the "center of timing" of river flow for naturalized flow in three major rivers, where the "center of timing" (CT) is the day of the year when half the total water flow for the year has occurred. They don't say in the main paper, but I assume that CT is based on the water year, which in the Northern Hemisphere, is often defined as starting on Oct 1. [Francis Zwiers, Canada]	Accepted - wording changed.
10-1098	10	28	14	28	14	"studied". [J. Graham Cogley, Canada]	accepted
10-1099	10	28	14	28	14	typo: "studied" [Albert Klein Tank, Netherlands]	accepted
10-1100	10	28	14	28	19	It looks like the climate models are severely flawed if they do not include such fundamental hydrology principles. [John McLean, Australia]	Rejected - the climate models employed in this study include fundamental hydroclimatic principles
10-1101	10	28	14	28	19	Barnett et al (2008) has ignored the following: (a) Loik, M.E. et el (2004) - A Multi-scale perspective of water pulses in dryland ecosystems: climatology and ecohyrdology of the western USA, (b) Jin, J et al (2006) - Relationship between atmospheric circulation and snowpack in the Western USA, and (c) Cayan,D.R et al l(1999) - "ENSO and Hydrological Extremes in the Western United States. All three emphasise the impact of the ENSO on the hydroology of the western USA. Barnett mentions the ENSO just once and tries to dismiss it as short-term, apparently or willfully blind to the Pacific Climate Shift of 1976, after which the El Nino side of absolutely neutral (ie. SOI=0) dominated the ENSO (and prior to the shift the La Nina side was dominant.) [John McLean, Australia]	Rejected - our assessment is that Barnett et al were cognizant of ENSO effects and included such effects in their estimates of natural variability
10-1102	10	28	14			“stdied” [William Ingram, United Kingdom]	accepted - typo fixed
10-1103	10	28	22	28	23	Re streamflow and evapotranspiration: where? [Dáithí Stone, United States of America]	accepted - wording changed
10-1104	10	28	22	28	25	Also, it should be assigned a confidence “low to medium confidence”. [Guoyu Ren, China]	Rejected - our assessment is that medium confidence is justified. The text is augmented to bolster the justification.
10-1105	10	28	23	28	23	Need to say where this assessment holds - many regions in northern mid- and high latitudes? [Francis Zwiers, Canada]	accepted - wording changed
10-1106	10	28	24	28	25	These subsection summaries keep on ending with a sentence or two excusing why you cannot (if you cannot) state anything confident. Why do you need to excuse yourselves? This sort of statement is only appropriate if you are trying to say “we haven't noticed something, but don't think that that necessarily means it isn't changing a lot” or “there is a crucial thing that we are missing that if we could have in 5-10 years would change	accepted - wording changed and paragraph shortened. No excuses are intended to be implied.

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						this statement". But you never indicate either so it hangs. [Dáithí Stone, United States of America]	
10-1107	10	28	29	28	29	This opening sentence feels like a gross over-simplification of what is a complicated set of factors that drive the particulars of the circulation including the land/dsea configuration, orographic barriers etc. This just seems like an invitation for critics. [Peter Thorne, United States of America]	Accepted. We included factors such as land-sea contrast and orography. Here, atmospheric circulations mean planetary-scale circulations, rather than small scales.
10-1108	10	28	31	28	31	Add a comma after "climate system". [Francis Zwiers, Canada]	Accepted. A comma was added.
10-1109	10	28	32	28	34	But don't they count as an external forcing on the local climate? [Dáithí Stone, United States of America]	Noted. External forcing was included in the sentence.
10-1110	10	28	34	28	34	Suggest replacing "reviewed" with "assessed" (presumably Chapter 2 does an assessment). [Francis Zwiers, Canada]	Accepted. "reviewed" was replaced with "assessed".
10-1111	10	28	35	28	35	Section is now 2.7 [Peter Thorne, United States of America]	Accepted. It was changed to " section 2.7.5"
10-1112	10	28	35			There is an excuse again, implying that you (we) have failed. But that is presupposing there is something to detect. [Dáithí Stone, United States of America]	The statement here just tells the fact. It does not imply a pre-assumption of existence of regional climate trends.
10-1113	10	28	37			What do you mean by "significant"? [Dáithí Stone, United States of America]	Line 37 is blank. We could not find the word "significant".
10-1114	10	28	39	28	39	What aspects of tropical circulation? [Francis Zwiers, Canada]	"width of the tropical circulation" was added.
10-1115	10	28	44	29	30	Section 10.3.3.1; Overall, it seems that papers from Hu et al, including one that is still in submitted phase, are quite prominent in the discussion. Not sure that this reflects a balanced view of the field and additional references are suggested in further comments. [Government of Australia]	Accepted. These suggestted papers in comments below were cited. Hu et al. is published.
10-1116	10	28	46	28	46	Awkward: "…determined based on…suggest that…" [Jochem Marotzke, Germany]	Accepted. "determined" will be deleted.
10-1117	10	28	46	28	49	Is there a discussion of tropical circulation change in Chapter 2 that could be cross-linked here, and is a consistent conclusion given here? [Francis Zwiers, Canada]	Accepted. We have cross-referred section 2.6 (should be section 2.7.5 now) at line 35 in the same page. We cross-refereed it again here. The conclusion given is consistent with chapter 2.
10-1118	10	28	51	28	52	Do you mean to say that anthropogenic forcing contributes to the widening, or specifically that 'changes in anthropogenic forcing' cause the widening (which is what it says as written)? These are subtly different concepts. If the latter, can more explanation be provided? [Government of Australia]	Accepted. "changes" was removed.
10-1119	10	28	51	28	56	Johanson and Fu (2009) were first to note the apparent shortcoming of GCMs in simulating width of tropics. Citation: Johanson, C. M. and Q. Fu, 2009: Hadley cell widening: Model simulations versus observations. J. Climate, 22, 2713-2725. [Government of Australia]	Accepted. The paper was cited.
10-1120	10	28	51	28	56	The possibility/likelihood of errors and inhomogeneities in the reanalyses themselves cannot be discounted, which may very well be result in an overestimate of the rate of tropical expansion. Multiple studies have indicated that the reanalyses are probably not suitable for the calculation of long-term/climate trends (e.g. Thorne and Vose 2010). Citation: Thorne, P. W. and R. S. Vose, 2010: Reanalyses suitable for characterizing long-term trends. Bull. Amer. Meteor. Soc., 91, 353-361. doi:10.1175/2009BAMS2858.1 [Government of Australia]	Acepted: The paper was cited and the text is modified.
10-1121	10	28	51	28	56	The observed widening of the Hadley Circulation is in complete accord with the dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. zero) since mid-1976. A large body of literature finds that the strengthening of the Hadley Circulation (and corresponding weakening of Walker Circulation) corresponds to the El Nino side of the scale. Either Hu et al is ignorant of the body of literature and the observational data pertaining to ENSO, or it was poorly simulated in the models used by that paper. Refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures". [John McLean, Australia]	Rejected. El nino strength/amplitude peaked at 1998 and declined since then, while the poleward expension of the Hadley circulation continues even after 1998. In Southern Hemisphere, poleward expension of the Hadley circulation shows seasonality, with signficant largest trend in DJF. It is consistent with the seasonality of Antarctic stratospheric cooling due to severe ozone depletion.

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							This is confirmed by CMIP3 and CMIP5 simulations.
10-1122	10	28	52	28	56	This doesn't quite give the same sense as the paragraphthat begins at line 46. Here models are judged by whether they reproduce observed changes, while in the paragraph just above, we are told that those changes are very uncertain - which implies to me it would be inappropriate to conclude that models systematically underestimate observed changes. Could it be that those "observations" are confounded with the effects of observing system changes in reanalyses? [Francis Zwiers, Canada]	Accepted: the text is modified to be more constistent.
10-1123	10	28	55	28	56	Lu et al. [2009] noted role of volcanic eruptions on tropical expansion calculations using tropopause methodologies. They also looked at relative roles of radiative forcing and changing SST. Lucas et al. [2012] estimated the magnitude of this effect as well as role of ENSO. These were generally interannual effects, but timing of starting point can result in apparent trend. Nguyen et al [2012] examined interactions of the Hadley Cell with ENSO and the annular modes. Citations: Lu et al. [2009] cited earlier in document. Other citations: Lucas, C., H. Nguyen and B. Timbal, 2012: An observational analysis of Southern Hemisphere tropical expansion. J. Geophys. Res., 117, D17112; Nguyen, H., B. Timbal, I. Smith, A. Evans and C. Lucas, 2012: The Hadley circulation in reanalyses: climatology, variability and expansion. Submitted, J. Climate [Government of Australia]	Accepted. These papers were cited.
10-1124	10	28				Section 10.3.3: nothing is mentioned about changes in the zonal tropical circulation despite many published papers (for instance the recent Tokinaga, H., S -P. Xie, C. Deser, Y. Kosaka, and Y. M. Okumura, 2012: Slowdown of the Walker circulation driven by tropical Indo-Pacific warming. Nature, 491, 439-443, doi: 10.1038/nature11576. [Laurent Terray, France]	There was a subsection on the Walker circulation in ZOD. However, studies showed inconsistent results of trends in the Walker circulation. Thus, the subsection was removed in FOD and SOD.
10-1125	10	29	1	29	16	This section should reference Staten et al (2011), who use systematic GCM 'time slice' experiments to examine the forcings behind extratropical jet shift, Hadley cell expansion, etc. They conclude that SST is a significant driver of these changes in both the past and the future. The paper is included in the reference list, but was not seen as a citation in the text (it may have been overlooked). [Government of Australia]	Accepted. The paper is now cited
10-1126	10	29	1	29	16	The observed widening of the Hadley Circulation is in complete accord with the dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. zero) since mid-1976. A large body of literature finds that the strengthening of the Hadley Circulation (and corresponding weakening of Walker Circulation) corresponds to the El Nino side of the scale. I conclude that either the papers that you cite are ignorant of the body of literature and the observational data pertaining to ENSO, or the ENSO was poorly simulated in the models that were used. Refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures". [John McLean, Australia]	Rejected. El nino strength/amplitude peaked at 1998 and showed declining since then, while the poleward expension of the Hadley circulation continues even after 1998. In Southern Hemisphere, poleward expension of the Hadley circulation shows seasonality, with signficant trend in DJF. It is consistent with the seasonality of Antarctic stratospheric cooling due to severe ozone depletion. This is confirmed by CMIP3 and CMIP5 simulations.
10-1127	10	29	1	29	28	Two statements concerning the role of Antarctic ozone depletion in poleward expansion of the southern Hadley cell say that 'There is robust evidence that Antarctic ozone depletion is a major factor in causing poleward expansion of the southern Hadley cell during austral summer' (at lines 1-2) and 'Based on modelling studies there is medium confidence that stratospheric ozone depletion has contributed to the observed poleward shift of the southern Hadley cell border during austral summer.' (at lines 26-28). I doubt that wordings 'robust evidence' and 'medium confidence' are consistent with each other. Please double check if these statements get along with each other. [Alexey Karpechko, Finland]	Accepted. "robust" is rmoved and text is modified.
10-1128	10	29	5	29	5	Replace "suggest" with "suggests". [Francis Zwiers, Canada]	Accepted.
10-1129	10	29	11	29	14	Perhaps related to static stability, but changing properties of baroclinic waves have also been noted as a mechanism for tropical expansion. See Chen et al (2008), Lu et al (2008). Citations: Chen, G., J. Lu and D. M. W. Frierson, 2008: Phase speed spectra and the latitude of surface westerlies: Interannual variability and the global warming trend. J. Climate, 21, 5942-5959.; Lu, J. G. Chen and D. M. W. Frierson, 2008: Response of the zonal mean atmospheric circulation to El Niño versus global warming. J. Climate, 21, 5835-5851. doi:10.1175/2008JCLI2200.1 [Government of Australia]	Accepted. Most of these papers were cited in FOD. Because the assessment here focuses more on forcing (natural or anthropogenic), rather than on mechanisms, these papers were cut off in SOD due to the length requirement. They were cited.
10-1130	10	29	14	29	16	There are also papers that state that SST plays little role in tropical expansion eg. Lu et al (2009). Sobel and Carmago (2011) use CMIP3 runs to suggest that SST changes are opposite to what should be expected from	Accpted. But Sobel and Carmago (2011) actually showed that SST changes are a consequence of

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						tropical expansion. Citation: Sobel, A. H. and S. J. Camargo, 2011: Projected future seasonal changes in tropical summer climate. J. Climate, 24, 473-487. [Government of Australia]	Hadley circulation changes. It does not mean ooposite effect of SSTs on the Hadley cell.
10-1131	10	29	15	29	15	Replace "SST" with "SSTs". [Francis Zwiers, Canada]	Accepted.
10-1132	10	29	19	29	19	"...December-February mean change of the southern border of the Hadley cell." [James Renwick, New Zealand]	Accepted.
10-1133	10	29	19			"Hadley" should be "the Hadley cell". [Adrian Simmons, United Kingdom]	Accepted.
10-1134	10	29	20	29	21	ERA-Interim data have been available from 1979 onwards for well over a year. It would be preferable for Figure 10.11 to use ERA-Interim data for the whole 1979-2005 period, and not to show results from the earlier-generation ERA-40 reanalysis. [Adrian Simmons, United Kingdom]	Accepted. ERA-40 is deleted. We now show trends based on several modern reanalysis products for the period from 1979 to 2005.
10-1135	10	29	26	29	30	The observed widening of the Hadley Circulation is in complete accord with the dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. zero) since mid-1976. A large body of literature finds that the strengthening of the Hadley Circulation (and corresponding weakening of Walker Circulation) corresponds to the El Nino side of the scale. The widening of the Hadley Circulation can therefore logically be attributed to ENSO and in particular the Pacific Climate Shift of 1976. There is no need to bring stratopheric ozone depletion or anthropogenic forcing into the picture. Refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures". [John McLean, Australia]	Rejected. We do not agree with the comment. El nino strength/amplitude peaked at 1998 and showed declining since then, while the poleward expension of the Hadley circulation continues even after 1998. In Southern Hemisphere, poleward expension of the Hadley circulation shows seasonality, with signficant trend in DJF. It is consistent with the seasonality of Antarctic stratospheric cooling due to severe ozone depletion. This is confirmed by CMIP3 and CMIP5 simulations.
10-1136	10	29	38	29	38	Replace "show" with "shows". [Francis Zwiers, Canada]	Accepted-text revised
10-1137	10	29	43	29	53	Caption of Figure 10.12. The 20th century Reanalysis 20CR currently finishes in 2010 so not all the trends can go up to 2011. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure cpation has been corrected.
10-1138	10	29	55	29	55	Replace "demostrate" with "demonstrates that". [Francis Zwiers, Canada]	Accepted-text revised
10-1139	10	29	55	29	57	After trying to understand this passage several times I gave up. I suspect that there is an error in here somewhere but regardless work to improve clarity would be adviseable. [Peter Thorne, United States of America]	Accepted-text revised. See comment 10-1138.
10-1140	10	30	9	30	29	When comparing simulation with the observations the large uncertainties of observed SAM trends should be taken into account(Fogt et al., 2009, J Climate; Marshall 2003, J Climate). Otherwise, the detected and attributed SAM trends may be biasedly estimated. This differs from NAM. I suggest to emphasize the uncertainties when making conclusion statement. [Daoyi Gong, China]	Teken into account. Fogt paper is cited and the discussion of the assessment in Table 10.1 discusses observational uncertainties.
10-1141	10	30	9	30	29	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected. Reviewer does not provide grounds for assertion.
10-1142	10	30	11	30	11	Should "Figure 10.11" be "Figure 10.12"? See also line 26. [Francis Zwiers, Canada]	Accepted-this is now Fig. 10.13
10-1143	10	30	11	30	15	This sentence needs to be rewritten to reflect the following facts: In the Southern Hemisphere there are two jets – the subtropical and the eddy-driven polar jets – not just a mid-latitude jet. While SAM may be associated with changes in the polar jet, it is less correlated with changes in the subtropical jet over Australia – which is by far the most important for Australia during winter, when some of the largest and robust changes in rainfall have occurred over southern Australia both in observations and projections (Frederiksen and Frederiksen 2007; Frederiksen et al. 2010, 2011). [Jorgen Frederiksen, Australia]	Accepted-text revised as suggested
10-1144	10	30	11			"Figure 10.11" shoulb be "Figure 10.12" [David Bromwich, United States of America]	Accepted-this is now Fig. 10.13
10-1145	10	30	15	30	23	Section 10.6 Extremes: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted.

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10-1146	10	30	16	30	16	Capitalize "A" in "Austral" to be consistent with line 31. [Government of Canada]	Accepted-text revised
10-1147	10	30	25	30	29	Section 10.6.1 Attribution of Changes in Frequency/Occurrence and Intensity of Extremes: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted.
10-1148	10	30	26			Figure 10.11 shoulb be "Figure 10.12" [David Bromwich, United States of America]	Accepted-this is now Fig. 10.13
10-1149	10	30	31	30	32	The statement 'the positive trend in the SAM…due in part to O3 depletion'. Misleading that GHGs are not important. Based upon the context as summaried, it is clear that the GHGs are at least as important as O3, if not the dominant drivers for SAM. [Daoyi Gong, China]	Accepted-text modified.
10-1150	10	30	35			this is about the right level of detail and verbiage. It is perhaps worth emphasizing that the large 'bump' that has generated numerous papers has virtually no impact on D&A analysis results. These are determined by the large trend [tim barnett, United States of America]	Noted.
10-1151	10	30	41	30	41	Unclear: "…, including to annular mode trends." [Jochem Marotzke, Germany]	Accepted-text modified.
10-1152	10	30	45	35	31	Need an overall summary for section 10.4 [European Union]	Rejected. Summary statements are made at appropriate place where variable being discussed and are additionally gathered together, with evidence, in the synthesis table 10.1.
10-1153	10	30	47	39	49	Here (or earlier), an explanation is missing why the oceanic analysis is limited to the quantities listed here. It is mentioned for salinity that only few formal ocean detection studies are existing, but this statement is quite hidden (p. 33, line 27/28). [European Union]	Takein into account Introduction to 10.4 now includes a rationale for the variables being looked at.
10-1154	10	30	48	30	49	This sentence doesn't belong a report about climate because ocean acidity is neither a cause or consequence of climate change. It belongf in something like a UNEP report of the consequences of increased atmospheric CO2. [John McLean, Australia]	Rejected -the acidity of oceans is an important factor for ocean bio-geochemisitry with climate implications.
10-1155	10	30	53	30	53	A reference to Box 13.1 would be appropriate after the first sentence [Gunnar Myhre, Norway]	Accepted - Text Revised
10-1156	10	30	53	31	2	No map of regions of heating were provided but it seems highly likely that the ENSO is a major factor both through direct ocean warming and through the strengthened Hadley Circulation causing a reduction in cloud cover, the consequence of which is greater solar irradiance and hence an increase in heat energy going into the ocean. [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1157	10	30	54	30	54	Release "is increasiing" with "increased". [Francis Zwiers, Canada]	Accepted - Text Revised
10-1158	10	30	56	30	56	Suggest deleting "Significantly". [Francis Zwiers, Canada]	Accepted - Text Revised
10-1159	10	30		32		Such methodologies are also sensitive to systematic errors in the underying model or models used. [Government of United States of America]	Taken into account - covered in Section 10.2 and Chapter 9
10-1160	10	31	7	31	8	I had a similar comment on Chapter 3. Characterizing it as a decadal variability is somewhat mis-leading. Firstly, people will conflate this with the better known and earlier documented surface temperature warming decade discussion. Second, without a context as to when and how it varied its significance and therefore what discussion follows becomes less accessible. I would suggest more explicitly characterizing this issue in both time and character to aid the reader here. [Peter Thorne, United States of America]	Accepted - Text Revised
10-1161	10	31	7	31	9	It is not quite clear from this whether that two things mentioned are linked (i.e., that the non-climate observing biases gave rise to an apparently decadal scale discrepancy between models and observations), but I assume that is the case. Perhaps that could be made a bit clearer. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1162	10	31	21	31	27	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1163	10	31	22	31	24	How do is see, from Figure 10.13a, that models with ANT and NAT forcing correspond more closely with observations? The figure does not seem to distinguish between ANT only simulations and ALL forcings	Accepted - Figure caption Revised

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						simulations. Also, the caption doesn't describe the grey shading, which I assume is uncertainty on the observations. [Francis Zwiers, Canada]	
10-1164	10	31	33	31	33	Saying that the eruptions "have caused a multi-decadal cooling" doesn't leave much room for uncertainty. Should this be qualified in some way, perhaps with a confidence statement (say medium, or high?). [Francis Zwiers, Canada]	Accepted - Text Revised to indicate high confidence in the statement.
10-1165	10	31	38	31	50	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1166	10	31	48	31	48	The remark in parentheses begs the question as to how the observed decadal variability can be known so precisely, and how it is that the range between CMIP3 models is so small. Perhaps I'm missing something - but I find Fig 10.13b rather obscure. Differences in S/N ratios across the different datasets, even discounting the Levitus datasets, suggest observational uncertainty in the magitude and/or pattern of change. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1167	10	31	53	31	53	Pierce et al., 2012 used 20 (not 12) CMIP5 models in their analysis [Paul Durack, United States]	Accepted - Text Revised
10-1168	10	31	55	31	57	Again these claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1169	10	32	4	32	5	Agreement among models is not any kind of proof even if models are 100% accurate. Do not imply that this is the case. [John McLean, Australia]	Taken into account - combined with other comment (10-1169)
10-1170	10	32	5	32	6	This sounds a bit over-confident. [Francis Zwiers, Canada]	Taken into account - test revised to indicate that the spurious decadal variability question is resolved.
10-1171	10	32	6	32	6	Should it read "Attribution to anthropogenic warming" - attribution is always done by humans. [Jochem Marotzke, Germany]	Accepted - Text Revised
10-1172	10	32	8	32	12	The comment as to the anthropogenic attribution of sea level rise would seem to necessitate a figure showing that attribution over time a major new result from AR4. [Government of United States of America]	Rejected - This would be a duplication as thermosteric sea level rise is a direct consequence of increased ocean temperatures for which a figure already exists.
10-1173	10	32	8	32	12	The wording implies that it is 'extremely certain' that all the increase in global ocean heat content in the upper 700m over 1951-2000 can be attributed to anthropogenic forcing. Surely that isn't right? I suggest using wording similar to that in the related paragraph about sea level rises (page 34, lines 42-46), which refers to extreme certainty that the 1951-200 increases have a substantial contribution from anthropogenic forcing. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accepted - Text Revised
10-1174	10	32	8	32	12	This claim is a fantasy unless you can demonstrate that all climate forces are simulated with 100% accuracy, which I am certain you cannot. An alternative plausible explanation is that the domination of ENSO conditions on the El Nino side of absolutely neutral (ie. zero) since mid 1976 had a significant impact on ocean temperature, which would imply that anthropogenic emissions of CO2 played a negligible part. (References for that shift - Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures" and o fcourse IPCC 4AR chapter 3). Figure 3.1(a) indicates mid-latitude warming of the ocean to 700m, which is consistent with reductions in cloud cover caused by the Hadley Circulation. (Solar radiation is very likely the cause of ocean warming; Downwelling radiation from CO2 cannot penetrate th eocean more than a few microns.) [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1175	10	32	10	32	10	"likely" instead of "certain"? [Albert Klein Tank, Netherlands]	Accepted - Text Revised
10-1176	10	32	10	32	10	certain --> likely (?) [Jochem Marotzke, Germany]	Taken into account - combined with other comment (10-1175)
10-1177	10	32	10	32	10	Extremely certain isn't a formally acknowledged AR5 category and sounds more certain than "virtually certain (≥99% probability)" even though you have defined it as >95% probability. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - combined with other comment (10-1175)

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10-1178	10	32	10	32	10	"extremely certain" should read "extremely likely" if this is meant to convey >95% probability (cf Mastrandrea et al Uncertainty Guidance Note) [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	Taken into account - combined with other comment (10-1175)
10-1179	10	32	10	32	10	This is the first use of 'extremely certain' I have come across. Is it consistent with uncertainty guidance? It also grates with the inner language pedant in me as I do not see it as good English. [Peter Thorne, United States of America]	Taken into account - combined with other comment (10-1175)
10-1180	10	32	10	32	10	"Extremely certain" is not a term used in the IPCC uncertainty guidance document. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - combined with other comment (10-1175)
10-1181	10	32	10	32	12	In several places, "extremely certain" should be replaced with "extremely likely" to be consistent with the uncertainties guidance paper. Is it possible to quantify the amount of the increase that is attributable to anthropogenic forcing? As written, you might infer that all of the increase was caused in this way; is it the intent to say that, or should there be a qualifier, such as "most" (which would imply, at least half), [Francis Zwiers, Canada]	Taken into account - combined with other comment (10-1175)
10-1182	10	32	11	32	12	State precisely when the increase in heat content began. Do not make vague comments like "latter half of the twentieth century" because the precise start of increase may be significant. [John McLean, Australia]	Accepted - Text Revised
10-1183	10	32	15	32	15	Delete "have". [Francis Zwiers, Canada]	Editorial – copyedit to be completed prior to publication
10-1184	10	32	18	32	18	"Their" means whose? [Jonathan Gregory, United Kingdom]	Editorial – copyedit to be completed prior to publication
10-1185	10	32	18	32	18	"Their" - whose? [Jochem Marotzke, Germany]	Editorial – copyedit to be completed prior to publication
10-1186	10	32	22	32	22	Allowed who to make the attribution? Is "attribute" used here in the same way as it is in other parts of the chapter (i.e., to quantify the contribution that a response to a forcing has made to an observed change, or at least, to state, at some specified likelihood level, the cause of an observed change)? [Francis Zwiers, Canada]	Accepted - Text Revised
10-1187	10	32	24	32	25	Replace "95% confidence level" with "5% significance level". This is a very common error of statistical language. The thing that the analyst controls when doing a test of significance is the significance level, which is the probability of incorrectly rejecting the null hypothesis when it is true. Tests of a parameter (such as the mean or variance) are sometimes performed by constructing an interval estimate (confidence interval) of the parameter, and then determining whether the value specified in the null hypothesis is included in the confidence interval. If not, the null hypothesis is rejected at a significance level that is 100% minus the coverage of the confidence interval (typically 90% or 95%). It is not rejected at the corresponding "confidence level". Using that term suggests confidence that the alternative hypothesis is correct - which is something that cannot be inferred from standard "frequentist" tests; you would have to be a Bayesian statistician to determin whether the alternative hypothesis is more likely than the null hypothesis. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1188	10	32	25	32	30	Is there a reference where this is done? Also, this is a somewhat confusing statement - presumably a space-time study that considered ocean heat content increases in all basins simultaneously would be very compelling, but I can't quite tell from the words whether this is what is meant here, or whether what is really meant is individual basin scale studies all detecting the influence of ANT forcing. A full space-time study would account for things like the expected differences in heat uptake over time in each of the basins and differences in internal variability in a single diagnostic. As with findings for surface air temperature, such space-time studies are considerably more compelling than studies of the global mean (which would be based on time-evolution only) or individual regional studies that ignore change outside the specific region of interest (continental scale or smaller). The latter benefit less from reductions of internal variability and have a less ability to separate the responses to different forcings. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1189	10	32	35	32	41	The caption needs to clarify when the start year is for the segment of length L. [Chris Forest, United States of America]	Accepted - Figure caption Revised
10-1190	10	32	35	32	41	The figure appears to use a different colour scheme than described in the caption. [Francis Zwiers, Canada]	Accepted - Figure caption Revised

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10-1191	10	32	38	32	39	V and no V models appear to be black solid and grey dashed lines. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - combined with other comment (10-1190)
10-1192	10	32	47	32	47	Delete "in the oceans" (observed ocean salinity change would not take place elsewhere :)). [Francis Zwiers, Canada]	Accepted - as suugested
10-1193	10	32	49			Ocean salinity data are neither consistent or inconsistent with changes the hydrological cycle but simply irrelevant because they result from local or regional changes in P-E and do not imply changes in either global precipation or global evaporation. [Government of France]	Taken into account, removed phrase about acceleration of hysdrological cycle
10-1194	10	32	52	32	53	"and in the interior". "supporting". [J. Graham Cogley, Canada]	Accepted and as suggested
10-1195	10	32	53	32	53	Suggest inserting "analysis" or "interpretation" after "broadly supports". [Francis Zwiers, Canada]	Taken into account
10-1196	10	32	55	32	55	Change "follow an enhancement of" to "enhance" (or clarify the meaning). [J. Graham Cogley, Canada]	Rewritten…. Patterns of subsurface salinity changes largely follow the existing mean salinity pattern at the surface and 18 within the ocean. For…
10-1197	10	32				Section 10.4.2: Update reference Terray et al. 2011 to Terray L., Corre L., Cravatte S., Delcroix T., Reverdin G., Ribes A., 2012 : Near-surface salinity as Nature's rain gauge to detect human influence on the tropical water cycle. J. Climate, 25, 958-977. [Laurent Terray, France]	Accepted bibliography updates
10-1198	10	33	3			Change the study of Hosoda et al. (2009) to Kobayashi et al. (2012). Hosoda's study discussed the salinity on the sea surface mainly and not mentioned specific water masses as AAIW. Kobayashi's study discussed long-term changes of water masses in the South Indian Ocean and concluded that AAIW has decreased its salinity significantly (95% confidence level) for the recent 50 years. Kobayashi, T., K. Mizuno, and T. Suga, 2012: Long-term variations of surface and intermediate waters in the southern Indian Ocean along 32°S, Journal of Oceanography, 68, 243-265, DOI: 10.1007/s10872-011-0093-5. [Taiyo Kobayashi, Japan]	Accepted - new reference was added
10-1199	10	33	7	33	7	Delete "also"? It is not clear what additional evidence is referred to by "also". Overall, this sentence could do with some additional word smithing - the initial statement doesn't seem to be associated with a reference, but then various bits of "other" evidence are cited, some of which sound like they are the same evidence as described in the initial statement. [Francis Zwiers, Canada]	Accepted - the shallow salinity maximum now also has explicit references.
10-1200	10	33	7	33	8	Delete "has occurred", and clean up the remainder of the sentence, which is garbled. [J. Graham Cogley, Canada]	Accepted -
10-1201	10	33	7		24	Observed ocean salinity do not suggest anything regarding the global water cycle because" (i) ocean salinity is governed P-E not total precipitation nor evaporation, (ii) observed regional variations (either over land or the ocean) may be caused by changes in global atmospheric transport of water vapor (i.e. for example a shift of the latitude of convergence zones) irrespective of the total amount of water stored in the atmosphere. If this was not so, very large differences in total precipitation would be observed between winter and summer as the summer hemisphere atmosphere contains 2-3 times more water vapor than the winter atmosphere. Also there is no oceanic water cycle because, by definition, the cycle involves long-range atmospheric transport through open lateral boundaries. The reference to the Clausius-Clapeyron is irrelevant, no matter what the results of CMIP3 may be. [Government of France]	Taken into account. We are careful to now discuss global water cycle in terms of the global patterns of surface changes in surface freshwater flux
10-1202	10	33	7			I would delete the word "global" here. The salinity results are indicative of an increased hydrological cycle of precipitation and evaporation over the ocean, but they do not necessarily imply changes to the hydrology of continental interiors. The water that is made available to the land is a small residual of the precipitation and evaporation over the ocean. Similar comments have been made in reference to Chapter 3. Later on this page, in line 46 to 48, there is a sentence that is germane to this point. [Adrian Simmons, United Kingdom]	Taken into account. We are careful to now discuss global water cycle in terms of the global patterns of surface changes in surface freshwater flux, see comment 1202
10-1203	10	33	10	33	10	Cross-reference to Figure 3.4a should be to the whole of Figure 3.4. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - editorial and taken into account in proofing of chapter
10-1204	10	33	18	33	19	Change "observations of" to "observed", and clarify what is meant by "global temperature increase per degree surface warming". [J. Graham Cogley, Canada]	Accepted -

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10-1205	10	33	20	33	23	"When the water flux amplification (that is precipitation minus evaporation) is examined in CMIP3 models, they show an amplification of the oceanic hydrological cycle to be about 8+-5%.." This statement is not supported by the Durack et al., 2012 (Science) publication. the 8+-5% number is the INFERRED OBSERVED E-P change obtained by determining the CMIP3 E-P and SSS amplification relationship, and then scaling the observed SSS (16+-10%) by this relationship. The CMIP3 models suggest that modelled E-P responds at a rate of 4.5% per degree (Durack et al., 2012; Figure 2C and text p 457) [Paul Durack, United States]	Accepted - the text has been changed, and reference has been made to the Chapter 9 as well, where the Clausius Claperyon equation is referred to for the tropics. Clausius Claperyon is not referred to anymore in this section.
10-1206	10	33	20	33	24	"The amplification of the water flux (precipitation minus evaporation) in CMIP3 models is about 8 ± 5%, consistent with …". The "The implication is" sentence appears to be disconnected from its predecessor, and should perhaps follow the next earlier sentence. If this is not right, a fuller explanation is needed. [J. Graham Cogley, Canada]	Accepted - the text has been changed, and reference has been made to the Chapter 9 as well, where the Clausius Claperyon equation is referred to for the tropics. Clausius Claperyon is not referred to anymore.
10-1207	10	33	22	33	23	I'm struggling with this, perhaps because I lack expertise and don't understand how surface salinitity amplification is determined. Therefore, I also don't know what kind of amplication would be consistent with Clausius-Claperyron (or whether such an expectation can be reasonably calculated given that P increases substantially more slowly than the CC relation, and the complexities of water vapour transport). Perhaps a few additional words explaining the concept of amplication would help - and perhaps it is also worth considering whether an allusion to Clausious Clayeron is indeed helpful (should the last bit of the sentence beginning with "and is consistent with ..." be deleted?). [Francis Zwiers, Canada]	Accepted - Clausisu Claperyon is now removed, and discussed in more detail in Chapter 9 (for the tropics and at a scale more relevant to this equation). The paragraph has been shortened.
10-1208	10	33	23	33	24	"The implication is that the CMIP3 ocean models mix surface salinity (and heat) too strongly.." This statement is more an inference than a proven fact and is not really supported (as a direct implication) by the Durack et al., 2012 (Science) publication. While the results suggest this conclusion is likely, I am uncertain such a strong statement is supported by the cited literature [Paul Durack, United States]	Accepted - the strength of this assertion has been softened
10-1209	10	33	23	33	24	The reader will rightly ask what implications this has for the findings and discussion in 10.4.1. You cannot conclude both that models and observations ocean heat content changes are right AND that models mix far too much heat down into the ocean. These two findings at least as stated are in gross contradiction as far as I can tell. [Peter Thorne, United States of America]	Rejected - the statement about mixing of is about the surface layers, and the depth integral from surface to 700 metre is affected by the result here. Hwever we have clarified the text to make sure the text is all about the surface mixed layer.
10-1210	10	33	26	33	55	This whole passage is replete with grammatical errors that make it entirely inaccessible. It needs to be completely rewritten for clarity and carefully proof-read for internal consistency. [Peter Thorne, United States of America]	Taken into account
10-1211	10	33	27	33	28	"relatively few studies that attribute these changes formally to". [J. Graham Cogley, Canada]	Accepted and taken into account
10-1212	10	33	31	33	31	Defining 30S-50N as an equatorial band is extremely odd. [Peter Thorne, United States of America]	Accepted and changed
10-1213	10	33	31	33	32	"'in the equatorial … patterns have changed significantly ...". [J. Graham Cogley, Canada]	Rewritten… On a larger spatial scale, the surface salinity 46 patterns in the band from 30°S–50°N show anthropogenic contributions that are larger than the 5–95% 47 uncertainty range (Terray et al., 2012).
10-1214	10	33	31	33	33	Although Terray et al. (2012) claim detection of an anthropogenic influence on the 33-yr Sea Surface Salinity (SSS) record, "roughly half" the historical CMIP3 model runs used in their study contain natural external forcings. As a result statements about attribution to "anthropogenic forcing" based on this particular work should be measured to bear this limitation in mind. i.e. detection results actually demonstrate that external forcing has had an influence on observed SSS changes. [Oliver David Andrews, United Kingdom]	Accepted and taken into account
10-1215	10	33	31	33	33	Change sentence to: "On a larger spatial scale, the band from 30°S– 50°N shows significant changes in surface salinity patterns at the 5–95% confidence level compared with internal variability, formally attributed to anthropogenic forcing (Terray et al., 2011). [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted and taken into account
10-1216	10	33	32	33	32	The reference here to the "5-95% confidence level" is totally confusing. I think you are saying that changes are significant at the 10% significance level, based on a two sided test, but I'm not absolutely certain. Note that refering to "confidence levels" when conducting a test of significance is a very common error of statistical	Taken into account - the sentence referred to here now does not mix confidence level, significance anymore.

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						language. The thing that the analyst controls when doing a test of significance is the significance level, which is the probability of incorrectly rejecting the null hypothesis when it is true. Tests of a parameter (such as the mean or variance) are sometimes performed by constructing an interval estimate (confidence interval) of the parameter, and then determining whether the value specified in the null hypothesis is included in the confidence interval. If not, the null hypothesis is rejected at a significance level that is 100% minus the coverage of the confidence interval (typically 90% or 95%). It is not rejected at the corresponding "confidence level". Using that term suggests confidence that the alternative hypothesis is correct - which is something that cannot be inferred from standard "frequentist" tests; you would have to be a Bayesian statistician to determin whether the alternative hypothesis is more likely than the null hypothesis. [Francis Zwiers, Canada]	
10-1217	10	33	34	33	34	Delete "east-west". [J. Graham Cogley, Canada]	Accepted -
10-1218	10	33	36	33	37	"changes (1955--2004) over the upper 250 m of the water column cannot be explained …". [J. Graham Cogley, Canada]	Accepted as suggested
10-1219	10	33	41	33	41	What are "historicalNat simulations"? [Albert Klein Tank, Netherlands]	Taken into account and text now refers to simulations "with just volcanic and solar variations"
10-1220	10	33	41	33	42	Expand or explain "historicalNat". Delete "are". [J. Graham Cogley, Canada]	Taken into account -HistoricalNat has been expanded into words
10-1221	10	33	41			… historicalNat (typo) [Government of France]	Accepted -
10-1222	10	33	42	33	42	typo: "are" [Albert Klein Tank, Netherlands]	Accepted -
10-1223	10	33	45			Please replace Allan and Soden (2008) with Allan et al. (2010) Environmental Research Letters since this is more appropriate for comparing with the salinity changes. [Richard Allan, United Kingdom]	Accepted - in full
10-1224	10	33	46	33	48	"differ in amplitude from". Change "result" to "discrepancy". [J. Graham Cogley, Canada]	Accepted.
10-1225	10	33	50	33	50	Delete "expert". [J. Graham Cogley, Canada]	Accepted.
10-1226	10	33	50			There is no oceanic water cycle (see comment 10.3). DELETE …"and the amplification of the oceanic water cycle". [Government of France]	Accepted.
10-1227	10	33	51	33	51	Terray et al., 2011 should be Terray et al., 2012 [Paul Durack, United States]	Accepted - editorial and corrected with revisions of bibliography
10-1228	10	33	53	33	53	typo: "a" [Albert Klein Tank, Netherlands]	Editorial
10-1229	10	33	53	33	53	Delete one "a" before "detectable". [Christian-D. Schoenwiese, Germany]	Editorial
10-1230	10	33	53	33	53	Replace "shows a a detectable" with "shows a detectable". [Francis Zwiers, Canada]	Editorial - accepted
10-1231	10	33	53	33	55	", show a detectable signal part of which is likely to be due to anthropogenic forcing." Delete the final sentence. [J. Graham Cogley, Canada]	Accepted - taken into account.
10-1232	10	33	54	33	54	The quantification here is pretty weak ("likely that SOME of the observed changes … are attributable"), so it might be better to avoid that kind of language, and rather use language indicating that an expected response to anthropogenic forcing has been detected in observations - e.g., "it is likely that the influence of anthropogenic forcing is discernable in ....". Presumably it would be hard to respond to the question, how much change is some of the observed change? [Francis Zwiers, Canada]	Accepted - taken in account and now rephrased the last two sentences to give clear statements about the confidence in the assessment, and also the likelihood of anthropogenic forcing.
10-1233	10	34	1	34	16	Figure 10.14b uses a categorical colour scheme for a continuous variable (correlation), which makes it very hard to interpret the colours intuitively. [Jochem Marotzke, Germany]	Accepted - the revised figure has removed the correlation colour bar an the points are shaded in just one colour.
10-1234	10	34	4	34	5	"Ocean surface salinity pattern amplification has an 8% increase.." should be "Ocean surface salinity pattern amplification has an 8% per degree increase.." [Paul Durack, United States]	Accepted and text changes as suggested
10-1235	10	34	16	34	16	Terray et al., 2011 should be Terray et al., 2012 [Paul Durack, United States]	Accepted

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10-1236	10	34	18		59	I thought this section was weak and could be strengthened. Suggest moving it to after the cryosphere section, and draw more heavily on the budget closure papers of Church et al. 2011 (GRL) and Gregory et al. accepted (J Climate) [John Church, Australia]	Taken into account - text revised to include results from section 10.5.2 (Cryosphere section).
10-1237	10	34	25	34	25	"reasonably well" is not a scientific expression. Please quantify exactly how well those simulations matched observations. [John McLean, Australia]	Rejected - this is referring to wording from the AR4 as it was in the published version.
10-1238	10	34	29	34	29	This statement is false. The corrections are no more than estimates, especially as to which data should be modified, and the reason for that is that there is no record of which method of SST monitoring was used on each ship. I think there's good reason to suspect that the "adjustments" were made with a deliberate intent of closing a gap that should not have existed, and quite obviously those making the adjustments knew which way the adjustments had to be made. [John McLean, Australia]	Rejected - outside the scope of the chapter (topic covered in Chapter 3)
10-1239	10	34	29	34	29	Suggest deleting "significantly", to avoid confounding with the statistical concept of significance. If an adjective is needed, you could use a synonym, such as "considerably". [Francis Zwiers, Canada]	Accepted - Text Revised
10-1240	10	34	32	34	33	The cited reference fails to explain how heat can be transported 700m (and further) into the ocean and failed to recognise that downwelling radiation from CO2 cannot penetrate more than a few microns. [John McLean, Australia]	Rejected - this is contrary to the physical understanding of the surface ocean and text does not need changing.
10-1241	10	34	32	34	33	The sentence on the Global Energy budget seems out of place here, and not relevant. It only adds confusion, given the paragraph is about the sea level budget. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - Text Revised
10-1242	10	34	34	34	35	Saying "... capture these contributions to a fair degree." is vague and unprofessional. Quantify exactly how well they do capture them. [John McLean, Australia]	Accepted - Text Revised
10-1243	10	34	35	34	36	"...to a fair degree..." is not very quantitative. Can alternative quantitative language be used here? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - Text Revised
10-1244	10	34	37	34	40	Figure 3.12 (chapter 3) showed considerable variation in the contemporaneous sea level at six locations with long term records but neither that chapter or this chapter shows maps that illustrate the change in sea level at different locations. It seems very likely that the ENSO is a major influence on sea level because it of the change in thermocline that it brings. During La Nina conditions it is not unusual to find temperatures at over 100m depth in the western Pacific at around 28C compared to 21C or 2C on the surface of the eastern Pacific. During El Nino events the sea surface temperatures are warm over a greater area. [John McLean, Australia]	Rejected - outside the scope of the chapter (topic covered in Chapter 13)
10-1245	10	34	43	34	43	Delete comma after "that". [J. Graham Cogley, Canada]	Editorial – copyedit to be completed prior to publication
10-1246	10	34	44	34	44	"virtually certain that the increase". [J. Graham Cogley, Canada]	Taken into account - combined with other comment (10-1248)
10-1247	10	34	44	34	44	Should the phrase be "extremely likely" rather than "extremely certain"? [Jonathan Gregory, United Kingdom]	Accepted - Text Revised
10-1248	10	34	44	34	44	certain --> likely (?) [Jochem Marotzke, Germany]	Taken into account - combined with other comment (10-1248)
10-1249	10	34	44	34	44	"Extremely certain": see comment on page 32 line 20. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - combined with other comment (10-1248)
10-1250	10	34	44	34	44	"extremely certain" should read "extremely likely" if this is meant to convey >95% probability (cf Mastrandrea et al Uncertainty Guidance Note) [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	Taken into account - combined with other comment (10-1248)
10-1251	10	34	44	34	44	"Extremely certain" is not a term used in the IPCC uncertainty guidance document. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - combined with other comment (10-1248)
10-1252	10	34	44	34	44	Replace "extremely certain" with "extremely likely". [Francis Zwiers, Canada]	Taken into account - combined with other comment (10-1248)
10-1253	10	34	44	34	46	This statement has no validity until you can demonstrate that the ENSO had negligible impact, which I can't	Rejected - the scientific evidence points to changes

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						see that you do. [John McLean, Australia]	occuring on time-scales not relevant to ENSO.
10-1254	10	34	45	34	45	How does one quanitify "substantial"? Is this more or less than "most" (i.e., more than half)? Someone will ask. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1255	10	34	48	34	48	Deleted "do". [Francis Zwiers, Canada]	Editorial – copyedit to be completed prior to publication
10-1256	10	34	48	34	50	These studies have no validity whatsoever unless you can demonstrate that the models used completely and accurately include all natural climate forces. (Of course peer-review of the papers should have noted this huge qualification on the results and arguably should have rejected the paper for the failing, which begs the question as to why should the IPCC cite papers where the peer-review is very questionable?) [John McLean, Australia]	Rejected - outside the scope of the chapter (topic covered in Chapter 9)
10-1257	10	34	50	34	50	Regarding Figure 10.20, you shouldn't confuse a simple comparison of time series with "attribution", which requires substantially more, including a rigourous evaluation of whether a proposed signal is present in observations, whether it is there with the right amplitude, and whether there are confounding influences on the observations that could mimick the signal. [Francis Zwiers, Canada]	Accepted - Text Revised
10-1258	10	34	55	34	55	Insert an "is" near the end of the line - "and IS not well quantified …". [Francis Zwiers, Canada]	Editorial – copyedit to be completed prior to publication
10-1259	10	34	57	34	59	The statement here, that anthropogenic contributions are "relatively small" contradicts lines 45 and 46 where it is claimed that the anthropgenic contribution is "substantial". One (or both) are very likely incorrect. [John McLean, Australia]	Rejected - the regional changes are small compared to variability at those scales unlike with global mean changes.
10-1260	10	34	57	34	59	I believe the sea level rise at Venice is very clear and it is established that it is anthropogenic in nature (through aquifer depletion). [Dáithí Stone, United States of America]	Rejected - not directly physical climate related. May be more appropriate for WG-II.
10-1261	10	34	58	34	58	Replace the subjective words "relatively small" with an exact quantification. [John McLean, Australia]	Rejected - not supported by the peer-reviewed published literature
10-1262	10	35	2	34	31	Is this subsection needed? Its conclusions are not very strong; if cuts are needed, this could be a target. [Jochem Marotzke, Germany]	Taken into account - we have shortened the section, and included a new detection and attribution study. Note comment 1266 below.
10-1263	10	35	2	35	31	Part of uncertainty might be due to the low resolution of the biogeochemical module which does probably not include the feedback of oxygen depletion on nutrients (elimination of N and release of P and Fe) [European Union]	Taken into account - we have noted that model uncertainty to include bio-geochemical component of simulations and also as discussed in box 6.5
10-1264	10	35	3			Change Wong et al. (1999a) to Wong et al (1999) [Taiyo Kobayashi, Japan]	Taken into account - editorial
10-1265	10	35	4	35	5	Should refer to section 6.4.5. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - and corrected in revised text
10-1266	10	35	4	35	31	This was an intersting section. Should it link back to Ch 2? Also between Ch2 and 10 there is the issue of measurements of oxygen in the atmosphere which show oxygen reducing slightly - roughly in accord with the amount of carbon burnt. This is another way to show that fossil fuel is being burnt to increase CO2 rather than being mysteriously released by underwater volcanoes. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - link to chapter section 6.1.3.2 (rather chapter 2) where the decline in oxygen conentration in the atmosphere is discussed..
10-1267	10	35	4	35	31	One may also mention the study by Frölicher et al., GBC, 2009 and by Cocco et al., BGD, 2012 as cited in chap 6. The latter study shows results from a range of ESMs [Fortunat Joos, Switzerland]	Taken into account. Rather than include additional references here, the cross reference is made to chapter 6.
10-1268	10	35	4	35	31	I found this section extremely hard to follow. It jumps around a lot and there are several grammatical errors that significantly detract from readability. A rewrite to simplify, improve flow and better proof reading would all help here. [Peter Thorne, United States of America]	Taken into account - text has been change to improve flow and readability
10-1269	10	35	4	35	31	Overall, this paragraph needs some additional word smithing… [Francis Zwiers, Canada]	Taken into account - text has been change to improve flow and readability

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10-1270	10	35	6	35	7	Could make explicit mention of secular historical deoxygenation evident in long-term time series measurements from the OMZs (Stramma et al. 2008) and subarctic North Pacific (Whitney et al., 2007). [Oliver David Andrews, United Kingdom]	Taken into account. To keep to length constraints reference is made to chapter 6 for further discussion.
10-1271	10	35	6	35	8	Considering geographical balance, some studies on the North Pacific should be removed and new ones discussing features in the other areas should be added. Aoki et al (2005): Southern Ocean, Indian Sector Bindoff and McDougall (2000): South Indian Ocean ('1962'-1987) Emerson et al (2004): North Pacific Keeling and Garcia (2002): Global Mecking et al (2006): North Pacific Nakanowatari et al (2007): the whole North Pacific Ono et al (2001): North Pacific (around Japan) The following would be the candidates to be added. Kobayashi et al (2012): South Indian Ocean (1960-2010) McDonagh et al. (2005): South Indian Ocean (1987-2002) Murata et al. (2007): South Pacific Stendardo and Gruber (2012): North Atlantic Stramma et al (2008): the whole tropical Kobayashi, T., K. Mizuno, and T. Suga, 2012: Long-term variations of surface and intermediate waters in the southern Indian Ocean along 32°S, Journal of Oceanography, 68, 243-265, DOI: 10.1007/s10872-011-0093-5. McDonagh, E. L., H. L. Bryden, B. A. King, R. J. Sanders, S. A. Cunningham, and R. Marsh, 2005: Decadal changes in the south Indian Ocean thermocline. J. Clim., 18, 1575–1590, doi:10.1175/JCLI3350.1. Murata, A., Y. Kumamoto, S. Watanabe, and M. Fukasawa, 2007: Decadal increases of anthropogenic CO2 in the South Pacific subtropical ocean along 32°S, J. Geophys. Res., 112, C05033, doi:10.1029/2005JC003405. Stendardo, I., and N. Gruber (2012): Oxygen trends over five decades in the North Atlantic, J. Geophys. Res., 117, C11004, doi:10.1029/2012JC007909 Stramma, L., G. C. Johnson, J. Sprintall, and V. Mohrholz, 2008: Expanding oxygen-minimum zones in the tropical oceans, Science, 320(5876), 655–658, doi:10.1126/science.1153847. [Taiyo Kobayashi, Japan]	Taken into account -updated the references where appropriate. Not all new references suggestions were accepted and only some were removed.
10-1272	10	35	8	35	8	Insert "a" before "pattern". [Francis Zwiers, Canada]	Accepted
10-1273	10	35	8			Change Wong et al. (1999b) to Wong et al. (1999) [Taiyo Kobayashi, Japan]	Accepted
10-1274	10	35	9	35	9	Insert "Recent" at the beginning of the sentence (to contrast with "these earlier results" mentioned on line 10). [Francis Zwiers, Canada]	Accepted
10-1275	10	35	10	35	10	Replace "extends" with "extend" (analyses, on line 9, is plural). [Francis Zwiers, Canada]	Taken into account. Analyses is actuall an analysis, and sentence has been changed to reflect it being singular rather than plural.
10-1276	10	35	18	35	21	"The observed decrease –0.55 ± 0.13 × 1014 mol yr–1 (Helm et al., 2011) is the same magnitude as the decrease estimated from rising oxygen concentrations in the atmosphere (Manning and Keeling, 2006)". With other words: since the ocean is not such a stronk sink for atmospheric O2 atmospheric O2 levels increase, Is this correct? [European Union]	Accepted - now refer to the simulations that cover this period from Section 6.4.5 and Andrews et al 2013 (see below)
10-1277	10	35	20	35	23	This sentence needs to be modified for clarity (perhaps putting the positive detection result for observed zonal mean oxygen changes in response to external forcing globally [and for the Pacific basin] first), and to include citation: Andrews, O. D., Bindoff, N. L., Halloran, P. R., Ilyina, T., and Le Quéré, C.: Detecting an external influence on recent changes in oceanic oxygen using an optimal fingerprinting method, Biogeosciences Discuss., 9, 12469-12504, doi:10.5194/bgd-9-12469-2012, 2012. [Oliver David Andrews, United Kingdom]	Accepted - and changed too improve clarity.
10-1278	10	35	20	35	23	reference?(Its a slightly awkward sentence) [John Mitchell, United Kingdom]	Reference added….The attribution study of oxygen decreases using two Earth System Models concluded that observed changes for the Atlantic Ocean are

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							"indistinguishable from natural internal variability" however the changes of the global zonal mean to external forcing (all forcings including greenhouse gases) has a detectable influence at the 10% significance level (Andrews et al 2012).
10-1279	10	35	22	35	23	Change to ...variability" but that for global zonal means the external forcing (all forcings including greenhouse gases) has a detectable influence at the 90% confidence level. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - and changed to imporve clarity
10-1280	10	35	25	35	28	Besides the biological pump also O2 production by phyto-plankton should play a significant role of sea water O2 levels. I do not see this mentioned [European Union]	Rejected - the open ocean work is typically out of the depth of light penetration, and so this term is considerd smaller, eg Deutsch et al 2006
10-1281	10	35	26	35	26	Reference Brandt et al. 2010 missing. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - editorial and taken into account in proofing of chapter
10-1282	10	35	26			Reference to Brandt et al., 2010 to be added in the references list (JPO, vol.40). [BERNARD BOURLES, France]	Accepted - editorial and taken into account in proofing of chapter
10-1283	10	35	28	35	28	typo: "to" [Albert Klein Tank, Netherlands]	Accepted -
10-1284	10	35	29	35	31	It might be appropriate to give a confidence assessment in this case - either in place of the "about as likely as not" likelihood assessment, or to support that assessment. The implication of making a likelihood assessment is that there is high, or very high, confidence in the body of evidence - but the uncertainties that are described here suggest a lower level of confidence. The uncertainties guidance indicates that a likelihood assessment can still be made in the case of medium confidence, but in that case, the confidence assessment should also be given. [Francis Zwiers, Canada]	Accepted - Taken into account, confidence level added.
10-1285	10	35	32	35	32	There is at least one formal detection study of an integrated oceanic variable (Atlantic meridional overturning circulation), based on AR4 simulations (Baehr, 2011, DSR II). [European Union]	Noted. However the chapter does not consider detection and attribution of the AMOC.
10-1286	10	35	33	41	13	Likewise no summary for 10.5 [European Union]	Rejected. Summary statements are provided at the end of each subsection of the chapter, and are then gathered together in table 10.1 Therefore it would be unnecessary duplication to repeat then in an additional summary section.
10-1287	10	35	37			We trust this section on Sea Ice will be revised to take into account any published studies relating to the record breaking 2012 Arctic sea ice extent. [Thomas Stocker/ WGI TSU, Switzerland]	Teken into account. See comment 1288
10-1288	10	35	39			Section 10.5.1.1 : Please include the recent research findings on the possible impacts of the decline of Arctic Sea Ice on the atmospheric circulation, weather pattern and extreme weather events in the mid-latitudes. For examples : - Xiangdong Zhang, Asgeir Sorteberg, Jing Zhang, Ru¨diger Gerdes, and Josefino C. Comiso, 2008 : Recent radical shifts of atmospheric circulations and rapid changes in Arctic climate system, Geophysical Research Letters, 35, L22701, doi:10.1029/2008GL035607 - Jennifer A. Francis, Weihan Chan, Daniel J. Leathers, James R. Miller,and Dana E. Veron, 2009 : Winter Northern Hemisphere weather patterns remember summer Arctic sea-ice extent, 36, L07503, Geophysical Research Letters doi:10.1029/2009GL037274 - Ian Simmonds and Kevin Keay, 2009 : Extraordinary September Arctic sea ice reductions and their relationships with storm behavior over 1979–2008, Geophysical Research Letters , 36, L19715, doi:10.1029/2009GL039810 - Vladimir Petoukhov and Vladimir A. Semenov, 2010 : A link between reduced Barents-Kara sea ice and cold winter extremes over northern continents, J. of Geophysical Research, 115, D21111, doi:10.1029/2009JD013568 - Meiji Honda, Jun Inoue, and Shozo Yamane, 2009 : Influence of low Arctic sea-ice minima on anomalously cold Eurasian winters, Geophysical Research Letters, 36, L08707, doi:10.1029/2008GL037079 - Francis, J. A., and S. J. Vavrus 2012: Evidence Linking Arctic Amplification to Extreme Weather in Mid-Latitudes. Geophys. Res. Lett., 39, L06801, doi:06810.01029/02012GL051000 [Sai Ming Lee, Hong Kong,	Taken into account. Will an an update to include 2012 sea ice loss. But impact of sea ice loss on atmospheric circulation is beyond the scope of chapter 10..

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						China]	
10-1289	10	35	43			Note: Box 5.1 should now be cited as Box 5.2. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
10-1290	10	35	46	35	46	The following two papers also examined the rapid declining of summer and/or winter sea ice in the first decade of the 21st century. They may be mentioned here. [Xiangdong Zhang, United States of America]	accepted papers added
10-1291	10	35	46	35	46	Zhang, X., A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso, 2008: Recent radical shifts in atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi:10.1029/2008GL035607. [Xiangdong Zhang, United States of America]	accepted
10-1292	10	35	46	35	46	Overland, J. E., and M. Wang, 2010: Large-scale atmospheric circulation changes are associated with the recent loss of Arctic sea ice Tellus A, 62, 1–9, doi:10.1111/j.1600-0870.2009.00421.x. [Xiangdong Zhang, United States of America]	noted but see comment 10-1288
10-1293	10	35	46	35	57	Reference should be made to the all-time minimum in Arctic summer sea-ice extent in September 2012 [David Bromwich, United States of America]	see 1288
10-1294	10	35	46	35	57	The record minimum in 2012 should be discussed in this para. [Thierry Fichefet, Belgium]	see 1288
10-1295	10	35	46	35	57	This section will need revision to account for the new September 2012 minimum. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	see 1288
10-1296	10	35	46	35	57	Please report the information on the new record low in Arctic Sea Ice extent occurred in September 2012. Reference : http://nsidc.org/arcticseaicenews/2012/10/poles-apart-a-record-breaking-summer-and-winter/ [Sai Ming Lee, Hong Kong, China]	see 1288
10-1297	10	35	46	35	57	Update to 2012 will change the text substantially. Cross-references to Figures 4.11, 4.13 and 4.14 should probably be to Figure 4.7. [David Parker, United Kingdom of Great Britain & Northern Ireland]	see 1288. Will cross chech with chapter 4
10-1298	10	35	46	35	57	The text of this paragraph will presumably be updated to refer to the record low coverage of ice observed in September 2012. [Adrian Simmons, United Kingdom]	see 1288
10-1299	10	35	46	35	57	It is not clear to us why this paragraph focuses on changes in Arctic Sea ice over the first decade of the 21st century, when the decline shown in Chapter 4 based figures has been occurring over a longer time period than this - "the average decadal extent of Arctic Sea ice has decreased in every season, and in every successive decade since 1979". Please also note that the citations given to the Chapter 4 figures here are inaccurate and need to be corrected. The wording of the final sentence in particular seems to imply that some threshold or sudden transition occurred in 2007, but this is not the impression given from the figures and assessment given in Chapter 4. Consider rewording, or clarifying why the focus here is on the past 10 years, and particularly the period since 2007. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. there has been a long trend since 1979, but a steeper trend in multiyear ice since mid 2000s
10-1300	10	35	48	35	57	NASA press statements declared the loss of Arctic ice in 2007 to be due to storms and wave conditions breaking the ice, and wind pushing it into the paths of warm currents. It is entirely to be expected that new ice in the regions that lost an abnormal amount of ice will be thin and easily broken. These are natiural conditions, which climate models don't seem to handle very well. [John McLean, Australia]	taken into account we note a commbination of forced and internal variability
10-1301	10	35	50	35	50	given 2012, this statement needs to be revised [Axel Schweiger, United States of America]	see 1288
10-1302	10	35	51	35	51	Replace "2011" with "2012", and insert "had" after "has". [Francis Zwiers, Canada]	see 1288
10-1303	10	35	52	35	52	"...2011 being second lowest compared with 2007." Presumably you can update with statistics o fthe 2012 record minimum. [James Renwick, New Zealand]	see 1288
10-1304	10	35	54	35	54	Total Arctic sea ice volume in September decreased by 80% from 1979 -2012 based on PIOMAS sea ice reanlysis (data updated from Schweiger et al. 2011) [Axel Schweiger, United States of America]	accepted will add POMAS results
10-1305	10	35	55	35	55	Insert "also" after "Sea ice has". [Francis Zwiers, Canada]	noted
10-1306	10	35	56	35	56	Replace "five years" with "six years"? [Francis Zwiers, Canada]	Accepted - new text has been written here and now

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							redundant.
10-1307	10	35		38		The role of dynamics and thermodynamics in forcing the changes in sea ice needs to be discussed more. Specifically, Perovich et al. 2008 on the role of ice-albedo feedback, and bottom versus top melt of sea ice should be discussed. And Rigor and Wallace 2004, on the role of advection of younger/thinner ice into the coastal zones in accentuating summer melt should be included. [New reference: Rigor, I.G. and J.M. Wallace, Variations in the Age of Sea Ice and Summer Sea Ice Extent, Geophys. Res. Lett., v. 31, doi:10.1029/2004GL019492, 2004.] [Ignatius Rigor, United States of America]	taken into account. There is not room or mandate for a full dicussion of ice dymamics in chapter 10. but we will add some comments and more references
10-1308	10	36	2	36	11	There has also been work done indicating that the Atlantic Meridional Overturning Circulation/Atlantic Multidecadal Oscillation has a discernable impact on september sea ice extent and should be mentioned in the context of sea ice extent variability. e.g. Mahajan, Salil, Rong Zhang, Thomas L. Delworth, 2011: Impact of the Atlantic Meridional Overturning Circulation (AMOC) on Arctic Surface Air Temperature and Sea Ice Variability. J. Climate, 24, 6573–6581.doi: http://dx.doi.org/10.1175/2011JCLI4002.1 and Day, J. J., Hargreaves, J. C., Annan, J. D., Abe-Ouchi, A. (2012) Sources of multi-decadal variability in Arctic sea ice extent. Environmental Research Letters, 7 (3). 034011. ISSN 1748-9326 doi: 10.1088/1748-9326/7/3/034011 [Jonathan Day, United Kingdom of Great Britain & Northern Ireland]	taken into account. Will add a comment on AMO
10-1309	10	36	2	36	11	This very general paragraph is oddly placed within a section on Sea Ice. It rather integrates across several different Arctic climate variables (incl. tundra shrubbiness, permafrost, forest fires etc) and therefore seems better placed elsewhere as a concluding paragraph, perhaps in 10.9.2. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account. Multiple lines of evidence for arctic change support the sea ice conclusions
10-1310	10	36	2	36	43	This as far as I can tell is not about detection and attribution and therefore out of chapter scope. [Peter Thorne, United States of America]	see comment 1309
10-1311	10	36	4	36	10	"Persistent trends in many Arctic variables, including sea ice extent,……., can no longer be associated solely with the dominant climate variability patterns such as the Arctic Oscillation". In light of the Notz and Marotzki (2012) and Day et al (2012), there is little evidence that there was ever a significant link between pan-Arctic ice extent and the AO. Suggest the text is changed to reflect these studies. [Jonathan Day, United Kingdom of Great Britain & Northern Ireland]	taken into account.. It is still important to note both internal and forced contributions even if the relative percentage contributions are not rigorously known
10-1312	10	36	5	36	5	Should changes in permafrost actually be spelt out here, its both a reduction in area and an increase in permafrost temperatures? [Peter Thorne, United States of America]	taken into account ie two permafrost indices
10-1313	10	36	9	36	9	One reference can be inserted as Oza et al, 2011b after Overland, 2009 [Government of India]	accepted
10-1314	10	36	10	36	10	Calling for recognition of sounds like advocacy. Surely something more like concluding that there was compelling evidence of would be better here? [Peter Thorne, United States of America]	accepted. Will note data in paper rather than advocacy
10-1315	10	36	10	36	11	What does "abrupt climate change" mean in the context of this chapter? This phrase has not been used before. [Albert Klein Tank, Netherlands]	accepted
10-1316	10	36	11	36	12	Does the chapter have a view on the Duarte et al (2012) suggestion? [Francis Zwiers, Canada]	see 1234
10-1317	10	36	13	36	13	"decreases in sea ice changes" -> so there is less change then? Wording here needs attention. [Thomas Stocker/ WGI TSU, Switzerland]	noted
10-1318	10	36	20	36	20	One reference can be added as Srivastav et al, 2011 after Gascard et al 2008 [Government of India]	accepted
10-1319	10	36	20	36	25	These feedbacks were first noted by Manabe and Wetherald, 1975,Manabe and Stouffer 1980 [John Mitchell, United Kingdom]	accepted
10-1320	10	36	27	36	28	what is 'gradual global warming'? This is the only location in chapter 10 where this term is used. Are feedbacks not part of this gradual warming? Please clarify and use more specific wording, consistent with the terminology used elsewhere in the chapter. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account. Used better wording
10-1321	10	36	27	36	43	Observational (Chylek et al., 2009) and modeling studies (Mahajan et al., 2011, Day et al., 2012)) suggest that Arctic sea-ice is influenced by the Atlantic Multidecadal variability. These and other studies should be	Accepted - mentioned AMO

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						mentioned here, rather than just saying internal variability on line 28. References:Chylek, P., C. K. Folland, G. Lesins, M. K. Dubey, and M. Wang, 2009: Arctic air temperature change amplification and the Atlantic multidecadal oscillation. Geophys. Res. Lett., 36, L14801, doi:10.1029/2009GL038777. Day, J. J., Hargreaves, J. C., Annan, J. D. and Abe-Ouchi, A. (2012) Sources of multi-decadal variability in Arctic sea ice extent. Environmental Research Letters, 7 (3). 034011. ISSN 1748-9326 doi: 10.1088/1748-9326/7/3/034011 Mahajan, Salil, Rong Zhang, Thomas L. Delworth, 2011: Impact of the Atlantic Meridional Overturning Circulation (AMOC) on Arctic Surface Air Temperature and Sea Ice Variability. J. Climate, 24, 6573–6581. [Government of United States of America]	
10-1322	10	36	30	36	33	Quantify your statements. It's not clear if the wind from the same direction as before was stronger than, equal to or weaker than similar wind patterns in the past but this information would make a lot of difference to interpretations. [John McLean, Australia]	taken into account. Wording changed
10-1323	10	36	31	36	31	Southerly wind pattern makes little sense here. I think what you mean is windflow from the Pacific to Atlantic sectors here. If so, say so. The wind at the north pole is always Southerly! [Peter Thorne, United States of America]	taken into account
10-1324	10	36	33	36	35	The following two papers also examined North Atlantic warm water intrusion through Fram Strait and the Barents Sea and could be mentioned here: [Xiangdong Zhang, United States of America]	accepted
10-1325	10	36	33	36	35	Polyakov, I. V., et al., 2005: One more step toward a warmer Arcitc. Geophys. Res. Lett., 32, L17605, doi: 10.1029/2005GL023740. [Xiangdong Zhang, United States of America]	see 1324
10-1326	10	36	33	36	35	Zhang, X., A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso, 2008: Recent radical shifts in atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi:10.1029/2008GL035607. [Xiangdong Zhang, United States of America]	see 1324
10-1327	10	36	34	36	34	Replace "play" with "have played". [Francis Zwiers, Canada]	noted
10-1328	10	36	35	36	35	Does "likely" represent a calibrated assessment using the IPCC uncertainties language? If not, another term should be used. If it is, I wonder whether a confidence assessment might be better suited to this situation, since the assessment concerns a judgement of which processes are in play. One could say, "Based on this evidence, there is (medium, high, very high?) confidence that these Arctic amplification mechanisms ....". [Francis Zwiers, Canada]	taken into account. References in paragraph support uncertainty language
10-1329	10	36	41	36	43	How does "likely" here reconcile with "very likely" used on page 38, line 3?. We appear to have FOUR different statements on arctic sea ice: 1) The ES says 'likely' anthropogenic forcing has contributed to arctic sea ice retreat; 2) here we have a combination of internal variability and human emissions are 'likely' responsible for recent decreases; 3) on page 38 we have a much stronger statement that it is 'very likely' that anthropogenic forcing is a major contributor to the sea ice decrease; 4) and Table 10.1 states an even stronger statement that "very likely' anthropogenic contribution is the cause of MOST of the sea ice retreat. This inconsistency on such a key topic is a concern and needs to be carefully addressed. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Will make uncertianly language consitsent throughout section 10.5 and with the rest of chapter 10.
10-1330	10	36	41	37	15	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the statements should be removed or seriously qualified. [John McLean, Australia]	Rejected. There is enough model evidence to suggest that timing of sea ice loss is uncertain
10-1331	10	36	41			Day, et al. Sources of multi-decadal variability in Arctic sea ice extent. Environmental Research Letters, 7(3), 2012 also looks at the causes of the sea ice decline, and argues for a modest contribution from internal variability. [James Annan, Japan]	accepted
10-1332	10	36	41			the lack of good data and modeling results makes this a weak chapter. The data often appear at odds with each other and this is reflected in the loose text.. Can this section be cut down to only include formal D&A results? [tim barnett, United States of America]	taken into account. Both data and models support d & a conclusions, even if they show wid variation in timing of loss.
10-1333	10	36	45	37	1	Day et al. 2012 find that between 5-30% of the decline in september sea ice extent is due to the AMO.	combined with 1321

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						Roughly in agreement with the results from Key et al, adding weight to this statement. [Jonathan Day, United Kingdom of Great Britain & Northern Ireland]	
10-1334	10	36	45	37	15	This seems to cross-remit with later chapters where much of the discussion more naturally belongs. This chapter should avoid trying to reach beyond its stated remit. Such over-reach simply serves to invite critics to split the differences between chapters when they discuss the same thing sufficiently differently - which is inevitable. [Peter Thorne, United States of America]	taken into account. Model results are a major diagnostic for chapter 10
10-1335	10	36	53	36	53	Schweiger et al 2011 found that the 1979 - 20010 Artic sea ice volume change from the PIOMAS sea ice reanalysis agreed well with the CCSM3 integrations and than attribution of the ice volume loss was not sensitive to the errors in the ice volume reanalysis [Axel Schweiger, United States of America]	accepted
10-1336	10	36	57	36	57	This study computed the fraction of natural variabilty deviding the trend in the anthropogenically forced ensemble by the observed trend. This assumes a "perfect" model and discounts the possibilty that CCSM4 may not have sufficient sensitivity. In the limit, a model with constant sea ice would yield a 100% natural variability by this measure. The 50% natural variability assignment needs to be caveated as such. [Axel Schweiger, United States of America]	see 13235
10-1337	10	37	1	37	1	Impact of multi-decadal variabilty on long term trend should cite Day et. al 2012 who found a significant but small contribution of the overall trend due to natural variabilty (AMO,AMOC). The 50% number from Kay is really out there. I think it would be ok if clearly identified as being very sensitive to the "perfect" model assumption [Axel Schweiger, United States of America]	see 13335
10-1338	10	37	1	37	1	"Detection of anthropogenic forcing" -> should this be "attribution of"? [Thomas Stocker/ WGI TSU, Switzerland]	noted
10-1339	10	37	1	37	4	I don't think the sentence describes the evidence in Figure 10.15a very well - the key is the comparison between models and observations, which is not made very apparent in the words. A confusing aspect is the reference to RCP4.5 and RCP8.5, which I am guessing miscommunicates what is shown in the diagram. I assume that what is shown from forced model runs in Figure 10.15a is mainly from the historical forcing runs. RCP forcing scenario runs would have to be used to extend the historical runs onwards from 2005, but that is, presumably, not the key part of the diagram for this discussion. For this chapter, the projection into the near term should not be a concern (that becomes a Chapter 11 topic). [Francis Zwiers, Canada]	taken into account. We are simplifying figure 10.15. it seems reasonable to extend the model runs to 2010 to make the necessary case for chapter 10 without overlapping chapter 11
10-1340	10	37	6	37	6	Figure 10.15 gives the impression that it is MIROC-ESM (panel D) that remains close to its control run, not HadGEM2ES (panel C). [David Parker, United Kingdom of Great Britain & Northern Ireland]	see 1339
10-1341	10	37	6	37	15	It is not all that clear that the discussion of projections, which beging at line 6, really contributes to our understanding of the causes of the detected historical change. [Francis Zwiers, Canada]	taken into account. Chapter 12 shows a continuation of the trend in chapter 10 , necessary for our d & a conclusions
10-1342	10	37	8	37	8	Delete the unnecessary "anthropogenic forcing and rising". [J. Graham Cogley, Canada]	see next comment
10-1343	10	37	8			It says here that the sea ice cover would stop shrinking if anthropogenic forcing stopped increasing. This is not consistent with the findings, however, in at least some of the references cited on this line. For example, the results of Armour et al. (2011) directly imply that if anthropogenic forcing stops increasing, the sea ice cover will continue to shrink for a long time due to the "memory" of the system (although the sea ice cover evolves in step with the hemisphere-mean temperature and would not continue to decrease if the hemispheric-mean temperature stopped rising). [Ian Eisenman, United States of America]	taken into account. Will note that this point is contriversial. Will consider dropping the point here as covered in chapter 12 see 1347
10-1344	10	37	9			Mahlstein and Knutti is now accepted in JGR [Irina Mahlstein, Switzerland]	noted
10-1345	10	37	10	37	10	Please state the reason for no tipping points. [Government of United States of America]	see 1343
10-1346	10	37	10	37	10	Consider including this message in the E.S. for the chapter. [Albert Klein Tank, Netherlands]	see 1343. evidience is not stron enough for ES
10-1347	10	37	10	37	12	Tipping points/thresholds, including in relation to arctic sea ice are addressed in Chapter 12, and don't fall within the scope of Chapter 10. This sentence could be removed here. [Thomas Stocker/ WGI TSU, Switzerland]	see1343

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10-1348	10	37	11	37	11	Insert commas before "once" and after "threshold", and delete "amount that". [J. Graham Cogley, Canada]	noted
10-1349	10	37	11			I don't know how to make sense of this definition of a "tipping point". In the simplest linear picture of the climate system, global-mean temperature would continue to rise for thousands of years if CO2 were held fixed at today's value (e.g., Held et al. 2010, dpi:10.1175/2009JCLI3466.1). Presumably one would not call such a linear relaxation a "tipping point", but it would meet the criteria of this definition. [Ian Eisenman, United States of America]	see 1343
10-1350	10	37	13	37	13	Change "an increased presence of external" to "increased". [J. Graham Cogley, Canada]	noted
10-1351	10	37	22	37	22	We note that Hadley ISST_ice is not one of the datasets assessed in chapter 4. This is a concern that a key observational dataset used in the D&A chapter is not assessed in the corresponding observation chapter. Some cross-chapter discussion with Chapter 4 on this would be essential, and ideally, Hadley ISST_ice could be included in their assessment. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account. Will shift to the chapter 4 timeseries
10-1352	10	37	29	37	38	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the statements should be removed or seriously qualified. [John McLean, Australia]	taken into account. There is a difference between model ensemble members means and data.
10-1353	10	37	29	37	38	This paragraph seems to overlap with the model evaluation given in Chapter 9, and could probably be removed here. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account. Will note chapter 9 results here and reduce text
10-1354	10	37	29	38	4	How about the direct effects of aerosols especially black carbons on the melting of sea ice in addition to the regional climate warming? There are publications already, and they should be assessed here. A recent paper related the accelerated melting of sea ice to the direct influence of CO2 itself on crystal structure of ice (???). These are anthropogenic agents, but they are different from the CO2-induced arcitic warming. [Guoyu Ren, China]	taken into account. We should mention black carbon but cannot consider it as the main driver
10-1355	10	37	31	37	33	It is worth mentioning here that the CMIP5 multi-model mean significantly underestimates the magnitude of the trend in September Arctic sea ice extent from 2005 to 2012. [Thierry Fichefet, Belgium]	taken into account see 1353
10-1356	10	37	32	37	33	A comparison between models and Artic sea ice change is provided for 1980-2000, which misses the large decreases observed in the past 10 years. Please provide up to date information on this comparison. [European Union]	see 1288
10-1357	10	37	32	37	33	A comparison between models and Artic sea ice change is provided for 1980-2000, which misses the large decreases observed in the past 10 years. Please provide up to date information on this comparison. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	see 1288
10-1358	10	37	33	37	33	Add Chapter 9 at the end of the sentence. [Thierry Fichefet, Belgium]	see 1353
10-1359	10	37	33		38	Discussion of dust radiative forcing should include an estimate from: P. Chylek and U. Lohmann, Aerosol radiative forcing and climate sensitivity deduced from the Last Glacial Maximum to Holocene transition, GEOPHYSICAL RESEARCH LETTERS, VOL. 35, L04804, doi:10.1029/2007GL032759, 2008 [Petr Chylek, United States of America]	accepted
10-1360	10	37	40	37	57	Needs an addition to mention that the 1930s ice loss was reported from ships and ground level and questions remain about the extent of that loss in the areas that were not observed. [John McLean, Australia]	taken into account. But there were Alaskan Observations which showed not entremes
10-1361	10	37	40	37	57	This again seems only very marginally related to attribution and risks substantive cross-talk issues with Chapter 4. This chapter and in particular this section needs to say what has happened in the sphere of its charge - detection and attribution. There are dedicated chapters to discussing observations, model processes and model projections and it is duplicative and inappropriate to discuss these areas within this chapter. [Peter Thorne, United States of America]	taken into account. The 1930s is often used as an argument against recent anthropogenic change. It is important to comment on 1930s versus 2000s.
10-1362	10	37	42	37	45	The Arctic Ocean warming and sea ice retreat in the early 1990s were also examiend by the following studies through both statistical analysis and model simulations. These two studies may be mentioend here: [Xiangdong Zhang, United States of America]	accepted

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10-1363	10	37	42	37	45	Rigor, I. G., J. M. Wallace, and R. L. Colony, 2002: Response of sea ice to the Arctic Oscillation, J. Clim., 15, 2648–2668 [Xiangdong Zhang, United States of America]	see 1362
10-1364	10	37	42	37	45	Zhang, X., M. Ikeda, and J. E. Walsh, 2003: Arctic sea-ice and freshwater changes driven by the atmospheric leading mode in a coupled sea ice-ocean model, J. Clim., 16, 2159–2177. [Xiangdong Zhang, United States of America]	see 1362
10-1365	10	37	47	37	49	It appears at least in mainland China as mentioned above. [Guoyu Ren, China]	taken into account
10-1366	10	37	56	37	56	Would it be helpful to insert "episodic" ahead of "regional increases" when describing the changes in the early 20th century? Were those regional increases sustained over the entire period being discussed? [Francis Zwiers, Canada]	accepted
10-1367	10	37	56	37	56	Is this a calibrated "unlikely"? If so, perhaps confidence language would be better suited here as well - see my comment concering page 36, line 35. [Francis Zwiers, Canada]	taken into account. will add confidence language
10-1368	10	37				Figure 10.15c: Why is the historical ice extent in the MIROC-ESMC increasing through the 1950s-1970s? [Jonathan Day, United Kingdom of Great Britain & Northern Ireland]	see 1339
10-1369	10	38	1	38	4	As in Point 10; explicit mention of the fact that it is 'very likely that anthropogenic forcing a major contributor to the observed decreases in Arctic sea ice' in the Executive Summary should be made. This is a potentially high impact statement for policy. [Oliver David Andrews, United Kingdom]	this is now consisten wit ES
10-1370	10	38	1	38	4	explicit mention of the fact that it is 'very likely that anthropogenic forcing a major contributor to the observed decreases in Arctic sea ice' in the Executive Summary should be made. This is a potentially high impact statement for policy. [European Union]	see 1369
10-1371	10	38	1	38	4	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the statements ned to be removed or seriously qualified. [John McLean, Australia]	taken into account
10-1372	10	38	3	38	3	As noted previously, this "very likely" statement on sea ice retreat is inconsistent with the ES, and elsewhere in chapter 10. [Thomas Stocker/ WGI TSU, Switzerland]	see 1369
10-1373	10	38	3	38	3	See previous comments concerning the choice between likelihood and confidence language (e.g., page 36, line 35, and page 37, line 56). Again, the judgement that is made here is perhaps better expressed using confidence language - ".... there is (high, very high) confidence that ....". A question that could arise, particularly with a likelihood assessment, is whether the size of the contribution can be quantified. Based on CMIP5, one might actually try that (although I'm not aware that this has been done yet). Based on CMIP3, one would be a bit hesitant given the large discrepancy between observed and simulated changes in Arctic sea ice extent (e.g., Min et al., 2008). [Francis Zwiers, Canada]	accepted see earlier notes
10-1374	10	38	6	38	12	Add data from recent years in this estimate. [Thierry Fichefet, Belgium]	see 1288
10-1375	10	38	9	38	9	One reference can be added as Oza et al, 2011a after Comiso and Nishio, 2008b [Government of India]	accepted
10-1376	10	38	10	38	12	It must be stated here that the models do not reproduce well the internal variability of the system in the Southern Ocean, the majority of them overestimating the standard deviation of the sea ice extent over the last 30 years compared to observations (see Zunz et al. 2012, cited in Chapter 9). The observed trend in Antarctic Sea ice may thus appear not significant compared to simulated internal variability because models overestimate it while the observed trend is significant as discussed in Chapter 4 (for instance page 35-36). [Hugues Goosse, Belgium]	accepted and included. This is the basis of our conclusion
10-1377	10	38	10	38	14	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the statements should be removed or seriously qualified. [John McLean, Australia]	see 1376
10-1378	10	38	11			Rather than stating that observed trends in Antarctic sea ice are "inconsistent with CMIP3 simulations including historical forcings (Hegerl. Et al., 2007)", the text could be updated to state that these trends are "inconsistent with CMIP5 simulations including historical forcings (Turner et al., 2013)". Reference: Turner, J., Bracegirdle, T., Phillips, T., Marshall, G. J. and Hosking, J. S., 2013. An Initial Assessment of Antarctic Sea	accepted

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						Ice Extent in the CMIP5 Models. Journal of Climate, in press (doi:10.1175/JCLI-D-12-00068.1). [David Bromwich, United States of America]	
10-1379	10	38	12	38	12	Citing Hegerl et al. (2007b) appears a bit circular here and new observations and model results have been obtained since AR4. [Hugues Goosse, Belgium]	accepted
10-1380	10	38	16	38	35	Consider assessing the important papers by Zunz et al. (2012; cited in Chapters 9, 11 and 12) and Bitz and Polvani (GRL, 2012, doi:10.1029/2012GL053393) in this para. [Thierry Fichefet, Belgium]	accepted
10-1381	10	38	19	38	19	SAM not defined. We recommend that it is defined and added to the glossary. [Government of United States of America]	it is defined earlier inchapter 10
10-1382	10	38	20	38	20	Is this a calibrated "likely"? [Francis Zwiers, Canada]	not enough evidence for probabalistic languge
10-1383	10	38	20	38	21	The conclusion to this sentence is mere speculation. [John McLean, Australia]	accpted
10-1384	10	38	25			The robustness of the results by Liu and Curry (2010) is highly questionable. The changes in SST and precipitation over the Southern Ocean during 1958-1999 reported in this paper are based 1) on SST data in which Southern Ocean observations are almost non-existent prior to 1982; and 2) on precipitation data estimated from the ERA-40 reanalysis, which is known to have a precipitation jump in high southern latitudes around 1979 when satellite observations are massively introduced. I suggest removing the reference to this paper here. [David Bromwich, United States of America]	accepted
10-1385	10	38	33	38	35	The observed trend in sea ice extent is said to be significant in Chapter 4 (page 4-3, lines 35-36 for instance). Here it is said that it is within the bounds of internal variability. This is compatible although this deserves probably some additional explanations. Furthermore, as this estimate of the magnitude of internal variability is highly uncertain because of large model biases and short observation time series in the region, such a conclusion is also highly uncertain. [Hugues Goosse, Belgium]	Noted. In the revised chapter we stress the large uncertainties in internal variability.
10-1386	10	38	41	38	44	They can also inhibit amplification- if the surface is melting, then the thermal inertia of the ice sheet holds temperature down to the melt temperature (cf Arctic sea-ice in summer) [John Mitchell, United Kingdom]	taken into account as one mechanism
10-1387	10	38	44	38	44	Why this Ref? This is on global T trends. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	accpted will check the right refeence
10-1388	10	38	44	38	44	Hansen and Lebedeff 1987 do not discuss ice sheets. [David Parker, United Kingdom of Great Britain & Northern Ireland]	see 1387
10-1389	10	38	44	38	44	I don't think Hansen and Lebedeff (1987) would be the right reference in this context! [Francis Zwiers, Canada]	see 1387
10-1390	10	38	44	38	46	I suggest not including a precise fraction (2/3) unless it is made exactly consistent, including the uncertainties, with the assessment of the sea level budget in 13.3.6. I don't think a number is needed to make the point. [Jonathan Gregory, United Kingdom]	accepted
10-1391	10	38	45	38	45	"2/3 of the contributions from all ice covered regions" -> Better to write this out in full as "two out of the three possible contributions from ice covered regions", if this is what you are meaning, although it is something of an obvious statement. [Thomas Stocker/ WGI TSU, Switzerland]	see 1390
10-1392	10	38	45			This statement is not balanced. The ice sheets have been small contribtors to sea lvel rise for most of the 20th century. It is only in the last few years that they have taken such a significant contribution [John Church, Australia]	taken into account. Will note for balance
10-1393	10	38	47	38	48	"is discussed". "attribution of warming over Antarctica to human influence". [J. Graham Cogley, Canada]	noted
10-1394	10	38	48			Sections 13.3 and 13.4 [John Church, Australia]	will cross refence
10-1395	10	38	54	38	56	I suggest omitting this sentence here, because it is not relevant to attribution. The non-linearity becomes apparent in the much larger changes projected for the future. [Jonathan Gregory, United Kingdom]	taken into account. It is important to mention potential non linearity
10-1396	10	39	1	39	15	This entire paragraph has nothing to do with attribution and therefore is out of scope. [Peter Thorne, United States of America]	taken into account. But it does relate to detection

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1397	10	39	1	39	29	It would be very helpful if this discussion of changes in the Greenland ice sheet could be framed using detection and attribution concepts, along the lines of those that are laid out in the good practice guidance paper on detection and attribution. What I learn from these paragraphs is the last decade, in particular, has seen historically large changes (i.e., this is something that has been detected), and that some studies have linked these changes to warming in the region. However, there do not appear to be any formal detection and attribution studies that link mass loss directly to forcing, and thus the discussion in these paragraphs has the flavour of representing one step in a multi-step attribution process (as described in the good practice guidance paper). I think that including some of this framing would help in determing and defending a level of confidence. On line 27, I again wonder if confidence language would be more suitable than likelihood language. [Francis Zwiers, Canada]	taken into account. Reviewer lays out current condition that detaection signal is evident and there are some mechanistic connections to attribution but only one quasi formal attribtuion study
10-1398	10	39	4	39	4	Greenland temperature records began much earlier than 1873. This is when DMI began monitoring them. Look at this paper. Vinther, B.M., Andersen, K.K., Jones, P.D., Briffa, K.R. and Cappelen, J., 2006: Extending Greenland temperature records into the late-18th century. J. Geophys. Res. 111, D11105, doi:10.1029/2005JD006810. The last deacde was the warmest in a much longer period as well. The year 2010 was amazing warm and over 1 deg C warmer than all other years in the SW Greenland average. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	accepted
10-1399	10	39	4	39	4	Fettweis et al. 2011a don't discuss 2010 or 2011. [David Parker, United Kingdom of Great Britain & Northern Ireland]	taken into account will update
10-1400	10	39	15	39	15	Garbled, incomplete sentence. [J. Graham Cogley, Canada]	noted
10-1401	10	39	15	39	15	This sentence has been truncated. [Jonathan Gregory, United Kingdom]	noted
10-1402	10	39	15	39	15	typo: "Mass loss and melt is Glacier" [Albert Klein Tank, Netherlands]	noted
10-1403	10	39	15	39	15	Please check the incomplete statement “Mass loss and melt is Glacier (Holland et al., 2008; Walker et al., 2009)”. [Sai Ming Lee, Hong Kong, China]	noted
10-1404	10	39	15	39	15	Incomplete sentence [John Mitchell, United Kingdom]	noted
10-1405	10	39	15	39	15	Sentence "Mass loss…" has got corrupted. [David Parker, United Kingdom of Great Britain & Northern Ireland]	noted
10-1406	10	39	15	39	15	"Mass loss and melt is Glacier". Not clear, some words missing? [Christian-D. Schoenwiese, Germany]	noted
10-1407	10	39	15			Unfinished sentence [David Bromwich, United States of America]	noted
10-1408	10	39	15			Sentence appears to be incomplete [Thomas Stocker/ WGI TSU, Switzerland]	noted
10-1409	10	39	17	39	19	Is this a statistically robust attribution in the usual sense of this chapter? For instance, was a level of significant correlation assessed from AOGCM control runs, and was the possible response to natural forcings considered? [Jonathan Gregory, United Kingdom]	taken into account will add refence
10-1410	10	39	21	39	22	Where does this information come from, since there were no observations of melting before the satellite era? On the other hand, retreat of Greenland outlet glaciers in the early 20th century was also widespread. [Jonathan Gregory, United Kingdom]	see 1410
10-1411	10	39	24	39	25	This is wrong. Greenland has many long records - see above. You are referring to the interior of Greenland. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	accepted
10-1412	10	39	24	39	27	I am not clear exactly what "story" we have confidence in, and what level of confidence (in AR5 terms) we have. I would be happier to read some more specific statements, such as in the next sentence. [Jonathan Gregory, United Kingdom]	accepted
10-1413	10	39	31	39	31	"Antarctica". [J. Graham Cogley, Canada]	noted
10-1414	10	39	31	39	31	One reference can be added as Oza et al, 2011c after greatest losses are at the edges [Government of India]	accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1415	10	39	31	39	31	It would be more precise to say "Antarctic ice sheet mass" than "ice mass in the Antarctic". [Jonathan Gregory, United Kingdom]	accepted
10-1416	10	39	31	39	31	"at the edges" is rather vague; it would be better to be precise and consistent with 4.4.2.3 (with a reference to that section), which says that the thinning is concentrated on the ice-streams. [Jonathan Gregory, United Kingdom]	accepted
10-1417	10	39	32	39	32	Ice-shelves are always floating, by definition. Please could you identify the ice-shelf concerned. [Jonathan Gregory, United Kingdom]	accepted
10-1418	10	39	32	39	34	I think that it can be concluded only that the thinning is due to greater heat transport into the sub-ice-shelf cavity. That might be due either to ocean warming or to altered transports, but have both of these definitely been implicated, as this sentence says? A reference to 4.4.3.1.2 would be helpful, and it would be good if this para and that section could cite the same publications regarding the Southern Ocean, to make it clear they have the same assessment. [Jonathan Gregory, United Kingdom]	accepted will quote chapter 4
10-1419	10	39	34	39	40	Don't the cited papers show that the primary cause of faster melting is the bringing into increased contact with Antarctic ice shelves of Circumpolar Deep Water that was already quite warm enough to melt the ice? That water will have seen its temperature increase only marginally, if at all, due to anthropogenic warming of the ocean. On that basis, the underlying cause of the increased melt does not depend on whether anthropogenic forcing is a contributor to ocean warming in the Southern Ocean, and it is irrelevant that Section 10.4.1 concludes that it is extremely certain that the anthropogenic forcing is a significant contributor to warming of the ocean. The two sentences should be reworded accordingly. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	taken into account. The sentence that this comment referes to has changed and is now expressed as " Antarctica has regionally dependent decadal variability in surface temperature with variations in these trends depending on the strength of the Southern Annular Mode climate pattern. Recent warming in continental west Antarctica has been linked to sea surface temperature changes in the tropical Pacific (Ding 49 et al., 2011). As with Antarctic sea ice, changes in Antarctic ice sheets have complex causes (Section 4.4.3). The observational record of Antarctic mass loss is short and the internal variability of the ice sheet is poorly understood. These factors combined with incomplete models in Antarctic ice sheet mass loss result in low confidence in scientific understanding, and attribution of the mass balance of Antarctica to human influence is premature"
10-1420	10	39	37	39	38	"contributor to ocean warming". "virtually certain". [J. Graham Cogley, Canada]	see 1419
10-1421	10	39	38	39	38	Should the phrase be "extremely likely" rather than "extremely certain"? [Jonathan Gregory, United Kingdom]	taken into account will check
10-1422	10	39	38	39	38	"Extremely certain": see comment on page 32 line 20. [David Parker, United Kingdom of Great Britain & Northern Ireland]	see 1421
10-1423	10	39	38	39	38	"extremely certain" should read "extremely likely" if this is meant to convey >95% probability (cf Mastrandrea et al Uncertainty Guidance Note) [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	see 1421
10-1424	10	39	38	39	38	Replace "extremely certain" with "extremely likely". [Francis Zwiers, Canada]	see 1421
10-1425	10	39	38	39	40	It is not clear what the reader is intended to infer from these statements. It would be more helpful if you gave a definite assessment of the level of confidence and/or likelihood that changes in the ocean affecting the Antarctic ice-shelf have an anthropogenic contribution. The earlier attribution statement concerns warming of the ocean in the global mean, and does not necessarily apply to the Southern Ocean. [Jonathan Gregory, United Kingdom]	see 1421
10-1426	10	39	38			"Extremely certain" is not a term used in the IPCC uncertainty guidance document. Suggest to change to the formal confidence/likelihood language if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	see 1421
10-1427	10	39	42	39	43	"regionally variable decadal variability". Delete "climate pattern". [J. Graham Cogley, Canada]	accepted
10-1428	10	39	42	39	44	Evidence for Antarctic surface temperature trends being attributable to ozone forcing is not supported by Steig et al. 2009 as cited. In general, the influence of ozone on surface temperature is limited to the summer	taken into account. Will update text and references. But major uncertainties remain and is the conclusion

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						season only, as discussed in Thompson et al., 2011). In all the other seasons, sea ice changes have played a significant role (Steig et al., 2009; Schneider et al., 2011; Turner et al., 2005). Attribution to trends in the tropical Pacific by(Ding et al., 2011 is also supported by Schneider et al., 2011, 2012 and Bromwich et al., in press).. References:1 Bromwich, D. H. et al. Central West Antarctica among most rapidly warming regions on Earth. Nat. Geosci. in press (2012). 2 Schneider, D. P., Deser, C. & Okumura, Y. An assessment and interpretation of the observed warming of West Antarctica in the austral spring. Clim. Dyn. 38, 323-347, doi:10.1007/s00382-010-0985-x (2011). 3 Schneider, D. P., Okumura, Y. & Deser, C. Observed Antarctic Interannual Climate Variability and Tropical Linkages. J. Climate 25, 4048-4066, doi:10.1175/jcli-d-11-00273.1 (2012). 4 Steig, E. J. et al. Warming of the Antarctic ice-sheet surface since the 1957 International Geophysical Year. Nature 457, 459-462, doi:doi:10.1038/nature07669 (2009). 5 Turner, J. et al. Antarctic climate change during the last 50 years. IJCli 25, 279-294 (2005). [Eric Steig, United States of America]	of the section
10-1429	10	39	42	39	48	The relevance of Antarctic temperature change to the ice-sheets is not obvious; probably this discussion belongs elsewhere. Antarctica is too cold for surface melting to have affected its mass. On the other hand, an assessment of precipitation change would be relevant here. [Jonathan Gregory, United Kingdom]	see 1428
10-1430	10	39	42	39	52	I suggest the following changes to the text: "In continental West Antarctica, on the other hand, recent warming has been linked to sea surface temperature changes in the tropical and subtropical Pacific (Bromwich et al., 2012; Ding et al., 2011; Schneider et al., 2012)". References: Bromwich, D.H., Nicolas, J. P., Monaghan, A. J., Lazzara, M. A., Keller, L.M., Weidner, G.A. and Wilson, A.B., 2012: Central West Antarctica among the most rapidly warming regions on Earth. Nature Geoscience, in press. /// Schneider, D., Deser, C. and Okumura, Y., 2012. An assessment and interpretation of the observed warming of West Antarctica in the austral spring. Climate Dynamics, 38:323- 347. [David Bromwich, United States of America]	see 1428
10-1431	10	39	42	39	52	This para seems repetitive from earlier. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	see 1428
10-1432	10	39	48	39	49	The comparison with Antarctic sea-ice is unhelpful. They are both complex, but that does not make them similar. [Jonathan Gregory, United Kingdom]	accepted
10-1433	10	39	50	39	52	This final statement is important but it is unclear how the foregoing leads to this conclusion. I think this statement should be linked to separate assessments of what we know and can attribute concerning sub-ice-shelf melting and dynamical response on the one hand, and accumulation change on the other. What is meant by "models in Antarctic ice sheet mass loss"? [Jonathan Gregory, United Kingdom]	accepted. Will take suggestion on tightening language.
10-1434	10	39	56	40	2	What period does "historically" refer to, and how does that period compare with the "longer time periods" referred to at the top of page 40? [Francis Zwiers, Canada]	Taken into account - Text was revised to "In the 20th century" (all studies cited mainly use 20th century data). The "longer time periods" were set in relation to the "interannual and decadal variability" of the previous sentence. To make this clearer, "longer time periods" was replaced by the more specific "On time periods longer than years and decades".
10-1435	10	39	59			"Reliable evidence" -> would be better to use one of the terms from the uncertainty guidance document for describing the quality of evidence, egg, "medium evidence", "robust evidence". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised to "robust"
10-1436	10	40	7	40	7	Change "short glacier lengths" to "glacial retreat". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected - Referring to glacier length is more specific than to "retreat", which may be understood as volume, lenght, or area reductions. The study cited is referring its conclusions to length reductions.
10-1437	10	40	14	40	15	This could be made more clear. Is it the climatic drivers whose variability exceeds that in earlier records, or the observed (or modelled?) responses of the glaciers? Perhaps all that is needed is to insert "responses to" before "inferred". [J. Graham Cogley, Canada]	Taken into account - Yes, it is referred to the inferred climatic drivers. Since these are mentioned in the previous sentence, it should be clear, but we extended the text to "the inferred" to strengthen the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							reference.
10-1438	10	40	15	40	15	One reference can be added as Bolch et al, 2012 after "climate records(" [Government of India]	Rejected - The mentioned paper does not extract climatic drivers or related parameters, and thus it cannot be cited here.
10-1439	10	40	15	40	16	This statement is diffuse and the references even more: what exactly is the "internal variability of the earlier records*, for instance, in the papers by Huss and Bauder or Huss et al., which - to my knowledge - do not treat questions of internal variability and earlier records but apply a heavily tuned degree-day model to observations of the (mainly late) 20th century with large possible errors . Huss and Bauder even use uncalibrated point observations with large and possibly even cumulative uncertainties. How can climate change detection and attribution be made with such approaches and such problematic data? The "unadapted" direct meteorological record itself would be a much safer evidence than a rather intransparent mixture of climate and ice data. IPCC should not provide the impression that uncalibrated point observations on glaciers or heavily tuned degree-day models can realistically be used for climate change detection and attribution. [Wilfried Haeberli, Switzerland]	There are three points to consider in this comment: (1) "internal variability of the records" - Taken into account - Text was revised to "exceed the variability of the earlier parts of the records"; (2) "degree-day models" - Taken into account - It is indeed not intended to suggest that these models are good for D&A. What we refer to here is the century-long time series of the obtained melt factors and solid/liquid precipitation ratio (Huss and Bauder, 2009). In the text we therefore deleted the misleading term "degree-day factors" and replaced it by the specific variables. (3) "uncalibrated point observations" - Rejected - These data are from peer-reviewed literature.
10-1440	10	40	18	40	20	Anthropogenic land cover change as a driver of glacier change is a new subject in the literature. If this statement is maintained in the report, it must be based in a published study. Can a confidence statement be added? (I guess, considering the small number of studies (1), low confidence should be assigned). [Christian Huggel, Switzerland]	Taken into account - Since it is clearly stated that there is only one study that has addressed the issue, we think that providing a confidence assessment is not necessary. However, to avoid confusion we now indicate that the study cited looked at local-scale land cover changes only (and these have limited impact potential).
10-1441	10	40	20	40	23	It is not clear to what the high confidence statement refers. Confidence results from evidence, so confidence in evidence is probably not an accurate wording. In principle I agree with the likelihood statement and I'm convinced it should be stated but just to mention that a quantitative statement could be difficult to sustain because of the (unfortunately) very small number of papers that really have addressed this issue (at least formally). However, such a statement could be justified considering the vast literature on glaciers and climate (change), and the conclusions that can be drawn from this in terms of anthropogenic influence. [Christian Huggel, Switzerland]	Taken into account - Text was revised to indicate more clearly what we understand as robust evidence, and what as the resultant high confidence. We agree that there is vast literature on glaciers and climate, but there are only few studies that can build on detailed century-scale data (and these are the ones we intend to cite in the text).
10-1442	10	40	21	40	21	"the confidence we have in estimates". [J. Graham Cogley, Canada]	Accepted - text revised accordingly
10-1443	10	40	23	40	23	Certainly glaciers have lost significant mass, but it would be more conservative to say "that a substantial part of the mass loss of glaciers …". [J. Graham Cogley, Canada]	Accepted - text revised accordingly
10-1444	10	40	23	40	23	The wording suggests that all of the mass loss can be attributed human influence - is that the intent? I could imagine that natural (multi-)decadal scale variability could have some influence on mass balance changes in some places. [Francis Zwiers, Canada]	Taken into account - text revised to "that a substantial part of the mass loss of glaciers …"
10-1445	10	40	25			(section 10.5.3) The observed reduction in snow cover is consistent with the strengthening of the Hadley Circulation that comes with the El Nino side of absolutely neutral (ie. SOI = zero) dominating the ENSO. Warm air is carried to the mid latitudes and pushes Arctic circulation further north. This dominance has occurred since 1976, see Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures" and IPCC 4AR chapter 3, [John McLean, Australia]	Rejected. Not supported by the peer-reviewed published literature
10-1446	10	40	27	40	27	Add "(SCE)" after "snow cover extent" (because this abbreviation is used lateron). [Christian-D. Schoenwiese, Germany]	Editorial
10-1447	10	40	27	40	42	This entire passage has nothing to do with attribution and therefore is out of scope. Discussion belongs in Chapters 4 and 9. [Peter Thorne, United States of America]	Taken into account. Text has been removed.

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10-1448	10	40	29	40	29	Delete "and a small increase in winter" which isn't clearly supported by Figure 4.21. Possibly change "summer and spring" to "spring and early summer" as the analyses cited in Chapter 4 are mostly March through June.. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been removed.
10-1449	10	40	30	40	30	Introduce "SCE" [Albert Klein Tank, Netherlands]	Editorial
10-1450	10	40	30	40	30	Define SCE. [Francis Zwiers, Canada]	Editorial
10-1451	10	40	30	40	31	To be unambigious, it would be better if it read "(7 Mkm^2 lower)". [David Rupp, United States of America]	Taken into account. Text has been removed.
10-1452	10	40	30	40	31	This value of 7 Mkm^2 is not supported by Fig. 4.19. In that figure, it appears to be only about 2 Mkm^2 lower. [David Rupp, United States of America]	Taken into account. Text has been removed.
10-1453	10	40	31	40	31	Insert "in" after "anomalies" [Francis Zwiers, Canada]	Editorial
10-1454	10	40	32	40	33	"This seasonality…" To which "seasonality" does this refer to? There is mention prior to N.H. and N. America, satellite and in-situ, and summer, spring, winter, March, April, May and June. Needs clarification. [David Rupp, United States of America]	Taken into account. Text has been removed.
10-1455	10	40	32	40	33	This reads that major negative N. American anomalies are "consistent" with in-situ measurements in N. Eurasia. How so? And should they be? Is the point to compare N. America with N. Eurasia, or satellite with in-situ? If the latter, why not compare equivalent geographic areas? If the former, is there a reason they may not be similar? Derksen and Brown (2012) show "inconsistency" in April SCE between N. America and Eurasia. [David Rupp, United States of America]	Taken into account. Text has been removed.
10-1456	10	40	35	40	35	Replace "over NH shortened" with "over that NH has shortened". [Francis Zwiers, Canada]	Taken into account. Text has been removed.
10-1457	10	40	44	40	53	Section 10.5.3: Derksen and Brown (2012) should be mentioned in this section as a detection study. Derksen, C., Brown, R. (2012), Spring snow cover extent reductions in the 2008-2012 period exceeding climate model projections, Geophys. Res. Lett., 39, L19504. [David Rupp, United States of America]	Rejected. The comparison between observations and model simulations by Derksen and Brown (2012) is rather qualitative.
10-1458	10	40	48	40	50	To be more precise, Rupp et al. (2012a) showed that while some CMIP5 simulations with all forcing could explain the observed decrease, in general the CMIP5 simulations with all forcings could only explain half of the magnitude of decrease. [David Rupp, United States of America]	Accepted. Text modified.
10-1459	10	40	49	40	49	Insert "spring" before "snow cover"? [Francis Zwiers, Canada]	Accepted. Text modified.
10-1460	10	40	50	40	53	Should this be a bit more specific (e.g., does the statement apply to all seasons?). [Francis Zwiers, Canada]	Taken into account, text is modified.
10-1461	10	40	50	40	53	The assessment appears to be constructed in reverse order here (the statement is, in effect, we have medium confidence that something is likely). Confidence language should not be used to qualify likelihood statements in this way. Rather, the confidence statement should assess the evidence basis, and the likelihood of some outcome should then be assessed on the basis of that evidence. Normally likelihood would only be assessed if there is high, or very high, confidence in the evidence basis. A likelihood assessment is still possible if there is only medium confidence in the evidence basis, but some additional discussion would be required (e.g., to point out assumptions concerning the evidence that are required to make the likelihood assessment). [Francis Zwiers, Canada]	Taken into accopunt. Evidence reassessed. There is a high confidence in the observations and in modelling studies that allow the use of likelihood language.
10-1462	10	40	51	40	51	Here it says "medium confidence", but Table 10.1 says "high confidence" [David Rupp, United States of America]	Taken into account. Text is adjusted to "high confidence" to be consistent with the table.
10-1463	10	40	51	40	53	This summary is too cautious an not consistent with table 10.1 row 22 which indicates high confidence. [European Union]	Taken into account. The new summary is now consistent with Table 10.1
10-1464	10	40	51	40	53	Here you indicate 'medium confidence' in the evidence, but Table 10.1 gives "high confidence". Please check carefully. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. The new summary is now consistent with Table 10.1
10-1465	10	40	52	40	52	For someone from outside it seems obvious that it is "likely to be caused by all forcings". This is D/A community language. [Albert Klein Tank, Netherlands]	Taken into account. The word "external" added to be specific.

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10-1466	10	40	55	41	13	This is out of scope. Except for the final sentence which is all that it is within this Chapter's direct purview to say on the matter. [Peter Thorne, United States of America]	Accepted. Text modified.
10-1467	10	40	56	40	57	This sentence doesn't make sense. How can a trend per decade increase over a single period? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. But this is not relevant anymore as the text has been removed.
10-1468	10	41	1	41	2	This very local detail for a global assessment. [Albert Klein Tank, Netherlands]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1469	10	41	2	41	4	Here is an error in the reference: “Pavlov et al. (2007)” should be changed on “Pavlova et al. (2007)”. [Petr Sporyshev, Russian Federation]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1470	10	41	7	41	13	How consistent is the mentioned trend towards earlier snow fall in autumn across regions? I think there is also evidence for later snow fall in autumn, and it could be mentioned that such a trend leads to cooling of permafrost, something that has been observed. Possibly it could be mentioned that retreat of glaciers can result in formation of new permafrost (if ambient temperature conditions are given, see e.g. Kneisel C, Kääb A. 2007. Mountain permafrost dynamics within a recently exposed glacier forefield inferred by a combined geomorphological, geophysical and photogrammetrical approach. Earth Surface Processes and Landforms 32 : 1797–1810.) [Christian Huggel, Switzerland]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1471	10	41	12	41	12	typo "or" [Albert Klein Tank, Netherlands]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1472	10	41	12	41	12	…little or no change (or, typo). [Christian-D. Schoenwiese, Germany]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1473	10	41	13	41	13	Again last sentence is inconsistent with table 10.1. [European Union]	Taken into account. But this comment is not relevant anymore, the text has been removed.
10-1474	10	41	15	48	54	Following the request of the chapter authors, I have reviewed the text of this section. I congratulate the authors on a comprehensive and clear assessment regarding the attribution of changes in extremes. I agree with most of the assessment, but have some comments/revisions on the subsection dealing with the attribution of drought changes, and one comment on section 10.6.2 (see next points). [Sonia Seneviratne, Switzerland]	Noted.
10-1475	10	41	17	41	23	Is “extremes” a defined term in this report? Otherwise you might want to point out that “extremely normal” is not what you are considering. And what time scales? Is a very wet year an extreme heavy precipitation event? [Dáithí Stone, United States of America]	Taken into account. "Extremes" is now more specific where needed.
10-1476	10	41	17	41	23	There are a couple of typos in this paragraph - including is spelled incorrectly (line 19), and an "a" is missing on line 22 ("as A starting point"). [Francis Zwiers, Canada]	Editorial
10-1477	10	41	19	41	19	"inncluding" should be "including" [Lisa Alexander, Australia]	Editorial
10-1478	10	41	19	41	19	"including". [J. Graham Cogley, Canada]	Editorial
10-1479	10	41	19	41	19	misprinted "inncluding" [Jiemjai Kreasuwun, Thailand]	Editorial
10-1480	10	41	19	41	23	In most of the other chapters I've seen "IPCC SREX" is simply referred to as "SREX". A consistent terminology amongst chapters would seem favorable. [Lisa Alexander, Australia]	Taken into account. We now use SREX
10-1481	10	41	21	41	23	Surely it would make more sense to start from the newer SREX report as the basis here. [Peter Thorne, United States of America]	Taken into account. SREX was considered interim assessment between AR4 and AR5, it was decided that AR5 starts from AR4 assessment.
10-1482	10	41	28	41	28	Could cross reference Box 2.4 for a wider discussion of 'moderate' extremes. [Lisa Alexander, Australia]	Taken into account. It is now cross referenced.
10-1483	10	41	28	41	28	It would be useful to give some examples here, or brief further explanation of what you mean by "moderate events". The idea of "moderate events" being extreme is something of an oxymoron without further explanation. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. A brief explanation is provided. It is also cross referenced to Box 2.4.

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10-1484	10	41	28	41	29	Moderate events' – this does not sound extreme. In fact it sounds contradictory. Do you mean 'moderately extreme'? Also can 'specific' events be attributed? [Government of United States of America]	Taken into account. There is now a clear definition of "moderate events" and cross reference to Box 2.4. We meant to attribute the changes in risk of specific events. Text is now modified to reflect this.
10-1485	10	41	28			This sentence is contradictory! The point really is that the word “extreme” is used in the climate change literature to mean things that aren't really extremes at all (because that's as far as there's statistical reliability, or because the one-in-ten case is thought useful to study & there's no better word), so this does have to be made, but made clearly! [William Ingram, United Kingdom]	Taken into account. This is now made very clear (with cross reference to Box 2.4 for definitions).
10-1486	10	41	34	41	36	The observations are consistent with the strengthening of the Hadley Circulation that comes with the El Nino side of absolutely neutral (ie. SOI=zero) dominating the ENSO, which has been the case since 1976 (although maybe it changed in 2010). Warm air is carried to the mid latitudes and pushes Arctic circulation further north. References - Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures" and IPCC 4AR chapter 3. [John McLean, Australia]	Rejected. Not supported by the peer-reviewed published literature
10-1487	10	41	36	41	37	This sentence seems superfluous. Hopefully readers don't really need that degree of hand holding. [Peter Thorne, United States of America]	Taken into account. The text is for clarity.
10-1488	10	41	39	41	39	Expected' according to what measure? Past observations? If so, over what period? [Government of United States of America]	Taken into account. Text has been modified
10-1489	10	41	39	41	41	The observations are consistent with the strengthening of the Hadley Circulation that comes with the El Nino side of absolutely neutral (ie. SOI=zero) dominating the ENSO, as has been the case since 1976, although perhaps it altered in 2010. Warm air is carried to the mid latitudes and pushes Arctic circulation further north. This accounts for general warming and any heatwaves can be attributed to stationary or quasi-stationary pressure cellls directing warm air ro a specific location (refer discussion of 2003 Eurpoean heatwave in chapter 3 of IPCC 4AR). [John McLean, Australia]	Rejected. Not supported by the peer-reviewed published literature
10-1490	10	41	39	41	42	Few typos in this paragraph - please correct. [Lisa Alexander, Australia]	Editorial
10-1491	10	41	39	41	42	This section should cite Hansen et al PNAS, 2012. Also it should be explicitly stated that it is very likely that seasonal extremes increased in frequency and that this is very likely attributable to human influence. (based on the work of: Hansen et al PNAS 2012; Jones et al, 2008; Stott et al, 2011) [Dim Coumou, Germany]	Taken into account. Reference added. Hansen et al. by its own does not establish the link between human influence and seasonal extreme temperature increase.
10-1492	10	41	39	41	42	Recently, Hu et al. (2011) demonstrated that long-term trends in addition to NAO and ENSO are the major factors resulting in the record high SST in the Atlantic Main Hurricance Development Region (MDR) in 2010 summer. Hu, Z.-Z., A. Kumar, B. Huang, Y. Xue, W. Wang, and B. Jha, 2011: Persistent atmospheric and oceanic anomalies in the North Atlantic from Summer 2009 to Summer 2010. J. Climate, 24(22), 5812-5830, DOI: 10.1175/2011JCLI4213.1. [Zeng-Zhen Hu, United States of America]	Taken into account. But the discussion is not about individual event.
10-1493	10	41	39			Doesn't quite read right: replace “rare” by “warm”? [William Ingram, United Kingdom]	Accepted. Text modified.
10-1494	10	41	40			Space after closing bracket [William Ingram, United Kingdom]	Editorial
10-1495	10	41	44	42	54	Temperature extremes caused by other human-activities such as soil moisture, vegetation change, and urbanization seems ignored [Daoyi Gong, China]	Talen into account. What have been discussed here are of large scale.
10-1496	10	41	44	42	54	There are new studies which provide weaker evidence for influence of the increased CO2 concentration on changes in extreme temperature events. The new studies show, for the first time, that urban effects not only affect the large scale mean temperature trends, but also the trends of minimum temperature and the extreme temperature indices in mainland China during the last 50 years (Zhou YQ, Ren GY. 2009. The effect of urbanization on maximum, minimum temperature and daily temperature range in North China. Plateau Meteorol 28(5): 1158-1166 (in Chinese); Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature	Taken into account. A new reference added in this section.

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						events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053; Zhang L, Ren GY, Liu J et al. 2011. Urban effect on trends of extreme temperature indices at Beijing Station. Chin J Geophys. 54: 1150-1159 (in Chinese)), indicating that the previously estimated regional trends of some extreme temperature indices may be too large. This, in combination with the observed slowdown of global and regional climate warming in the last 15 years, may not encourage an adjustment to the conclusions of the AR4 and SREX assessments. [Guoyu Ren, China]	
10-1497	10	41	46	41	46	Section 2.6 [Peter Thorne, United States of America]	noted, but there is no speciifc comment here
10-1498	10	41	47	41	48	Section 2.6 [Peter Thorne, United States of America]	noted, but there is no specific comment here
10-1499	10	41	48	41	48	It might suggest a human influence but it also suggests other mechanisms (see my comment about lines 39 to 41 on this same page) [John McLean, Australia]	Taken into account, but the comment does not providespecific literatures.
10-1500	10	41	48	41	53	Make clear that this is based on CMIP3. Can this be updated with CMIP5 results? [Francis Zwiers, Canada]	Taken into acocunt. Text modified.
10-1501	10	41	48			section 10.6.2, while interesting,seems out of place here. Suggest it be moved to appendix or deleted. Also since there are so many 'non detectable items, the rest of the chapter could be cut down, with maybe a paragraph on each side area [tim barnett, United States of America]	Rejected. It is important to assess the science of attribution of individual events since this leads to an assessment statement on heatwaves as this science is being uesd to put recent extreme events in to the context of climate variability and change.
10-1502	10	41	48			“suggests human influence” seems too strong to me, in that it suggests evidence independent of that already reviewed for change in mean temperature. I don't think much, if any, is. “is consistent with the evidence above for human influence on mean temperature”? In fact, it would be interesting to know if all this is consistent with the (modelled & observed) extremes simply shifting as much as the means (plus noise, as always). I assume from the fact that this is never mentioned that it has not been examined – but explicitly mentioning & regretting this omission might be helpful. [William Ingram, United Kingdom]	Taken into account. In many places, the changes in extreme temperature would be consistent with shift in means (though there is evidence for changes in variability in some regions such as very dry region). Changes in extremes due to shift in mean is considered as changes in extreme.
10-1503	10	41	49	41	50	The claims here are contradicted by the Australian Bureau of Meteorology whose media statements clearly state that stationary or near-stationary pressure cells were to blame for heatwaves. The BoM descriptions have at different times been applied to heatwaves in south-western Australia, south Australia, south-eastern Australia and the central east coast. For example: "Perhaps the most important synoptic feature during the month, and the one responsible for Victoria's heatwave, was a blocking high pressure system in the Tasman Sea which developed from about the 26th. The system directed a northerly air flow over the southeast of Australia, moving an extremely hot air mass over the southeast of the continent." (Monthly Weather Review, Victoria, Jan 2009) and "As the high pressure system entered the Tasman Sea it slowed dramatically and became nearly stationary during the next week, 7th to 14th, bringing heatwave conditions over the vast majority of South Australia." (Monthly Weather Review, South Australia, Nov 2009) (both available from http://www.bom.gov.au/climate/mwr/). And of course "The 2003 heat wave was associated with a very robust and persistent blocking high-pressure system that may be a manifestation of an exceptional northward extension of the Hadley Cell (Black et al., 2004; Fink et al., 2004)." IPCC 2007 report, WGI contribution, Chapter 3, Section 3.8.4 (Box 3.6) [John McLean, Australia]	Taken into account. Specific events such as these mentioned here are covered in Section 6.2.
10-1504	10	41	50	41	50	Are there any regions over the globe where the trends are found to be inconsistent? [Government of United States of America]	Taken into account. Text modified.
10-1505	10	41	51	41	52	The observations are consistent with the strengthening of the Hadley Circulation that comes with the El Nino side of absolutely neutral (ie. SOI=zero) dominating the ENSO conditions since mid 1976. (References Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures") If ENSO modelling was better this might be better recognised. [John McLean, Australia]	Rejected. Not supported by the peer-reviewed published literature
10-1506	10	41	51	42	6	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected. It is not the modelling of nature forces is poor. It is nature forcing weak.

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10-1507	10	41	52	41	52	remove "the" before heatwave. In fact both of the studies referenced here assess heatwave duration rather than heatwave intensity. I am unaware of studies that have looked at heatwave intensity globally but there may be some that I am unaware of. [Lisa Alexander, Australia]	Taken into account. Text modified.
10-1508	10	41	52			I assume this “growing season length” takes account only of temperature. Since soil moisture may be, or become under climate change, the limiting factor, this should be made explicit. [William Ingram, United Kingdom]	Taken into account. Growing season length is typically defined according to temperature.
10-1509	10	41	56	42	56	"shows". [J. Graham Cogley, Canada]	Editorial
10-1510	10	42	5	42	5	Typo - space required after ")". [Lisa Alexander, Australia]	Editorial
10-1511	10	42	8	42	20	These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. As well as this, research has shown that daily maximum temperatures are closely linked to solar irradiance and the moisture content of the Earth's surface (less moisture meaning less heat used in the process of evaporation). [John McLean, Australia]	Rejected. Anthropogenic forcings influence amount of solar irradiance absorbed by the atmosphere.
10-1512	10	42	8	42	21	Care is needed here as Chapter 2 concludes only low to medium confidence in DTR changes because of increased evidence that there are distinct inhomogeneities in max and min temperatures. As these analyses are looking at at best Qced and not homogenized data all they might be doing is looking at non-climatic influences rather than any kind oif emerging signal. There seems to be a degree of disconnect between how sections 2.4 and 2.6 are discussing the observations and how they are being used here that needs to be reconciled. This comment applies to most of 10.6.1.1 but is particularly acute here. [Peter Thorne, United States of America]	Taken into account. The discussion is not about DTR, the factors that impacted DTR may not impact temperature extremes.
10-1513	10	42	12	42	16	This sentence is quite unclear especially "location parameters as linear functions of signals..". This could do with some rewording for clarity. [Lisa Alexander, Australia]	Taken into account. Text is modified.
10-1514	10	42	13	42	13	I think this needs to be a bit more tutorial - would the reader understand what a location parameter is? [Francis Zwiers, Canada]	Taken into acocunt. Text modified.
10-1515	10	42	13		15	While arguably literally true as written, this is seriously misleading in that it will give the impression to any innocent reader that the same applies to mean/total precipitation. Either qualify “all other things being equal (which they are not for total precipitation, which is energetically constrained: ref or x-ref).” or have an explicit step in the logic about extreme precipitation typically scaling with total column moisture as it rains out all there is in the atmosphere. [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1516	10	42	16	42	16	"complementary". [J. Graham Cogley, Canada]	Editorial
10-1517	10	42	16	42	16	… complementary (e missing, typo). [Christian-D. Schoenwiese, Germany]	Editorial
10-1518	10	42	16	42	16	Complementary is spelled incorrectly. [Francis Zwiers, Canada]	Editorial
10-1519	10	42	19	42	21	More typos and minor grammar suggestions. Line 19, replace "are able" with "were able". Line 20, replace "model" with "models". Line 21, replace "overestimates" with "overestimate". [Francis Zwiers, Canada]	Editorial
10-1520	10	42	21	42	21	"overestimates" should be "overestimate" [Lisa Alexander, Australia]	Editorial
10-1521	10	42	24			The horizontal lines in the figure at 0 and 1 are invisible. They could be thicker or darker to make the graphs easier to read. [Chris Forest, United States of America]	Editorial
10-1522	10	42	24			Add s to “side” or “show” [William Ingram, United Kingdom]	Editorial
10-1523	10	42	30	42	30	Space between "thefrequency". [Lisa Alexander, Australia]	Editorial
10-1524	10	42	30			“thefrequency” [William Ingram, United Kingdom]	Editorial
10-1525	10	42	34		35	Grammar, or at least punctuation, adrift [William Ingram, United Kingdom]	Editorial
10-1526	10	42	38	42	39	"Changes in annual extremes of daily temperatures may also be attributed to human influence …". [J. Graham Cogley, Canada]	Taken into account. What we meant in the text was that human influence was detected.

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10-1527	10	42	38	42	53	Speaking logically, these claims about anthropogenic and natural forcing cannot be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected. Natural forcing is week when compared with other forcings.
10-1528	10	42	41	42	43	Sentence is confusing. Please clarify if possible. [Government of Canada]	Taken into account. The sentence has been rewritten.
10-1529	10	42	41			Doesn't quite make sense - “the anthropogenic-only signal can be detected” (assuming that's all that's meant: if not it really needs clarifying [William Ingram, United Kingdom]	Taken into account. Rephased.
10-1530	10	42	42	38	45	This is almost the first reference to the influence of land cover change on climate (temperature or rainfall). I feel this needs a more in-depth discussion. [European Union]	Taken into account. A new reference added in this section.
10-1531	10	42	43	42	44	Does this need more discussion? On the face of it, I would expect this vegetation change to affect winter extremes in the way discussed (due to the increase in surface albedo associated with snow masking of the grass). In summer, it seems less obvious what the effect on extremes might be - the vegetation change might result in a change in the balance between latent and sensible heat production, which could either increase or decrease extremes depending upon the direction of the change in the balance. [Francis Zwiers, Canada]	Taken into account. The nature of Christidis et al. study does not provide detailed account of physics involved. We have modified text to reflect that this is simulated by ONE earth system model.
10-1532	10	42	47	42	54	Few typos in this paragraph - change to "for the detection of a human influence", "and at some continental..". Please correct. [Lisa Alexander, Australia]	Editorial
10-1533	10	42	48	42	51	Should there not also be mention in this sentence of the detectable changes in extremely warm nights (TN90), given the results presented of Morak et al. (2011a)? [Government of United States of America]	Taken into account. Text has been modified to highlight new evidence.
10-1534	10	42	48	42	51	Is the new evidence more studies, bigger changes, more widespread changes or what ? [John Mitchell, United Kingdom]	Taken into account. The text is modified to highlight new evidence.
10-1535	10	42	53	42	54	This review comment relates to my prior review comment #18. Revise to use the same language as in the Executive Summary, where in the latter "anthropogenic forcing" is used, whereas here in section 10.6.1.1 the phrase "human influence" is used. [Martin Hoerling, United States of America]	Taken into account. Anthropogenic forcing is used here.
10-1536	10	42	53	42	54	This statement cannot be sustained. You don't seem to understand the cause of heatwaves and have failed to take into account hydrological variations. [John McLean, Australia]	Rejected. The comment does not provide logical reasoning why the statement cannot be sustained..
10-1537	10	42	56	44	2	Section 10.6.1.2 Precipitation Extremes: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted.
10-1538	10	42	56			Please see Shiu, C.-J., S. C. Liu, C. Fu, A. Dai, and Y. Sun (2012), How much do precipitation extremes change in a warming climate?, Geophys. Res. Lett., 39, L17707, doi:10.1029/2012GL052762.Their major conclusions are: (1) Large changes in the precipitation extremes derived from the Global Precipitation Climatology Project (GPCP) data, about 100% increase for the annual top 10% heavy precipitation and about 20% decrease for the light and moderate precipitation for one degree warming in the global temperature, are in agreement with results derived from reanalyses of the European Centre for Medium-Range Weather Forecasts (ECMWF) and the National Centers for Environmental Prediction (NCEP). (2) In comparison, coupled climate models are capable of simulating the shape of the change in precipitation intensity, but underestimate the magnitude of the change by about one order of magnitude. The most likely reason of the underestimation is that the typical spatial resolution of climate models is too coarse to resolve atmospheric convection. [Shaw Liu, China]	Taken into account. However, it does not ssem to be physically plausible to have 100% increase for one degree increase in temperature.
10-1539	10	42	56			(section 10.6.1.2) These claims about anthropogenic and natural forcing cannot logically be sustained while the modelling of natural forces is poor. [John McLean, Australia]	Rejected. It is naturalforcing that is weak.
10-1540	10	42	56			This section is about heavy precipitation extremes, not precipitation extremes generally. [Dáithí Stone, United States of America]	Taken into account. It is now more specific where ever possible.
10-1541	10	43	1	43	2	Reference for 'appear to be consistent'? And specify on what spatial/temporal scales? [Government of United States of America]	Taken into account. Text modified but no reference added as this is a summary of AR4 findings.

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10-1542	10	43	1	44	2	About the human-caused precipitation extremes, impacts of aerosols on triggering/enhance the deep convective thunderstorm which may bring heavy rainfall, seems ignored. This is important issue even now its spatial scale and role in long-term changes are still not clear.(Tao et al., 2012, Rev. Geophy.) [Daoyi Gong, China]	Taken into account. Chapter 7 covers these aspects.
10-1543	10	43	1	44	2	The regions of extreme rainfall should be clearly stated. It seems likely that the variations in precipitation will be consistent with the ENSO. [John McLean, Australia]	Taken into account. The changes in extreme precipitation that have been assessed are at very large scale, as such, it is difficult to specify specific regions (except the UK study).
10-1544	10	43	8	43	28	This paragraph could do with some better coordination with Ch 7 (7.5.5) and Ch 12 (12.4.5.5). In particular consideration of future projections belongs in Ch 12 and not Ch 10. Also there are several typos including "moist-adiabate temperature rapes rate"! [Lisa Alexander, Australia]	Taken into account. Text is modified to reduce duplication with Ch7 and Ch12.
10-1545	10	43	8	43	28	Mahajan et al. (2012) find that positive trends in monthly heavy precipitation (return period of one year) in the US in two datasets with one of them being statistically significant at the 95% confidence level based on a bootstrapping method. This study could be cited here. References: Mahajan S., G. R. North, R. Saravanan, M. G. Genton (2011): Statistical Significance of the Trends in Monthly Heavy Precipitation over the US, Climate Dynamics, 38, 1375-1387 [Government of United States of America]	Taken into account. But the objective of this paragraph was to highlight expected changes in extreme precipitation due to warming.
10-1546	10	43	8	43	28	These paragraphs refer to “extreme precipitation” when I think it means “extreme heavy precipitation”. They also seem focused on ~daily time-scales: what about longer time scales? [Dáithí Stone, United States of America]	Taken into account. However longer-time scale is not assessed due to limited availability of literature.
10-1547	10	43	8	43	28	This paragraph seems to be more the purview of other chapters as it does not directly discuss attribution. [Peter Thorne, United States of America]	Taken into account. The text has been modified to highlight main points of expected changes. The discussion about c-c relation etc. is removed as it is covered in Section 7.6.5
10-1548	10	43	12	43	18	Clausius Claperon has been shown to not hold for hourly extreme precipitation at temperatures of 10C and higher, see for example: Pall, P., M. Allen and D. Stone, 2007. Testing the Clausius-Capeyron constraint on changes in extreme precipitation under CO2 warming. Clim. Dyn., 28, 351-363. O'Gorman, P. A. and T. Schneider, 2009. The physical basis for increases in precipitation extremes in simulations of 21st-century climate change. Proc. Nat. Acad. Sciences, 106, 14773-14777. Lenderink, G. and E. van Meijgaard, 2008. Increase in hourly precipitation extremes beyond expectations from temperature changes. Nature Geosci., 1, 511-514. Haerter, J. O. and P. Berg, 2009. Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geosci., 2, 372-373. Lenderink, G. and E. van Meijgaard, 2009. Reply to: Unexpected rise in extreme precipitation caused by a shift in rain type? Nature Geosci., 2, 373. Lenderink, G., Mok, H. Y., Lee, T. C., and van Oldenborgh, G. J.: Scaling and trends of hourly precipitation extremes in two different climate zones – Hong Kong and the Netherlands, Hydrol. Earth Syst. Sci. Discuss., 8, 4701-4719, doi:10.5194/hessd-8-4701-2011, 2011. [Emma Daniels, Netherlands]	Taken into account. As this is also assessed in Chapter 7, we have removed the relevant text, with a reference to Section 7.6.5.
10-1549	10	43	13	43	13	Please replace 'change' with 'warming', since 'more moisture' only accompanies warming, not cooling. [Government of United States of America]	Taken into account. Buit this comment is not relevant anymore as the text has been removed.
10-1550	10	43	13	43	28	The argument is too simplistic, and the notion of "all things being equal" is a non-starter for serious consideration of how increasing GHG forcing may affect extreme precipitation. One revision is to follow the statement that "A higher moisture content in the atmopshere would be expected to lead to stronger extreme precipiation", immediately with the statement "However, other factors affecting the intensity of extreme precipitation such as atmospheric circulation, thermodynamic stability, and intensity of vertical motions may also change thereby affecting the probable intensity of extreme precipitation". This section reminds one of early overly simplistic discussions on how GHG forcing would affect Atlantic hurricanes, with the argument initially offered that there should be an increase given the strong correlation between Atlantic hurricanes and MDR SSTs, with the latter projected to rise. Thus, "all things being equal", Atlantic hurricanes were surmised	Taken into acocunt. Chapter 7 has a Section on this. As a result, this part is removed from Ch10. We now reference to Section 7.6.5

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						to increase in a warming world. But all things are not projected to stay equal, and this may have significant implications for changes in extreme precipitation, just as we have learned regarding Atlantic hurricanes. [Martin Hoerling, United States of America]	
10-1551	10	43	17	43	17	"kelvin". The name of the unit is not capitalized. [J. Graham Cogley, Canada]	Editorial
10-1552	10	43	19			“season” adds nothing [William Ingram, United Kingdom]	Taken into account. "season" removed.
10-1553	10	43	21			I am either too lazy or too close to the deadline to check, but how global is global here? Antarctica? Africa? [Dáithí Stone, United States of America]	Taken into acocunt. We added "with sufficient data"
10-1554	10	43	22			“on average globally” doesn't quite make sense: I think it just needs “fractional” at the start of the line [William Ingram, United Kingdom]	Taken into account. "on average: removed.
10-1555	10	43	24	43	24	"moist-adiabatic temperature lapse rate, or in the vertical velocity, may …". [J. Graham Cogley, Canada]	Taken into account. However, the comment is not relevant anymore as the text is removed. This is now assessed in Ch7
10-1556	10	43	24	43	24	typo: "lapse" [Albert Klein Tank, Netherlands]	Editorial
10-1557	10	43	24	43	24	"...the moist-adiabate temperature rapes rate…" I considered a number of wise cracks, but decided really nothing more needed to be said. [David Rupp, United States of America]	Editorial
10-1558	10	43	25	43	25	Typo: "temperature" [Jochem Marotzke, Germany]	Editorial
10-1559	10	43	25	43	27	The following paper should also be added to this list of references: Lenderink, G. & Van Meijgaard, E. Increase in hourly precipitation extremes beyond expectations from temperature changes. Nature Geosci. 1, 511–514 (2008). [Government of United States of America]	Taken into account. However, the comment is not relevant anymore as the text is removed. This is now assessed in Ch7.
10-1560	10	43	27			“in areas” adds nothing: delete [William Ingram, United Kingdom]	Taken into account. However, the comment is not relevant anymore as the text is removed. This is now assessed in Ch7.
10-1561	10	43	30	43	31	Revise sentence to begin with "There is only a modest body of direct evidence….." [Martin Hoerling, United States of America]	Accepted. Text modified.
10-1562	10	43	30		50	General comment: Perhaps more care is needed in using phrases like 'changes in mean lead to changes in [Sucharita Ghosh, Switzerland]	Taken into account. Text modified.
10-1563	10	43	32	43	36	Remove these speculative sentences, which lack evidence and have no reference. [Martin Hoerling, United States of America]	Accepted. Text modified.
10-1564	10	43	34	43	34	I did not understand "if the probability of precipitation remains similar". [Jochem Marotzke, Germany]	Taken into account. But this comment is not relevant anymore as text is removed.
10-1565	10	43	34		36	Now, this sentence is untrue. I can't see any valid point that is worth making along these lines. I suggest replacing both sentences by something like “However, mean precipitation is expected to increase less than extreme precipitation because of energy constraints (ref(s) &/or x-ref(s)). So this does not imply that changes in extreme precipitation cannot possibly be detectable either, though sampling uncertainty will obviously be larger, and data is limited (Alexander & al, 2006).” [William Ingram, United Kingdom]	Accepted. Text modified.
10-1566	10	43	34			While again this is literally true as written, it is nonsense in that with extreme precipitation increasing more than the mean, the probability of precipitation is expected to decrease! [William Ingram, United Kingdom]	Taken into account. But this comment is not relevant anymore as text is removed.
10-1567	10	43	43		44	While this sentence is true as written, it makes it sound as if the anthropogenic-only signal is plausible, which it is not – the main difference between the 2 signals is volcanic eruptions whose occurrence & effects on rainfall indisputably happened in the real world, and the signals are nothing like orthogonal (the case in which it might make sense to detect one but not the combination). There is very poor coverage (as Min & al do make clear in their Fig 1) & even the somewhat sketchy results they do get require careful selection of the number of EOFs retained, raising serious questions about test multiplicity. (They use 4 temporal EOFs specifically to	Taken into account. The reference to Min et al. has been rephased.

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						maximize detectability and reduce "short-term increases at both ends of the record". This is not the classic D&A situation of increasing the number of degrees of freedom included until one reaches scales too small to be well-simulated in the GCM(s), & for that matter I am not aware of any justification of using temporal EOFs in D&A – it certainly does not seem intuitively right to me.) Min & al find ranges of beta typically extending far above 1, & while obviously no fluctuation-dissipation theorem formally applies, simple commonsense suggests that if the GCMs do so massively underestimate the mean signal, they may similarly underestimate its variability - but even best-estimate betas range far above the square root of two which is as much as they scale standard deviation up by (Zwiers, pers. comm.): scaling it up by just by the best-estimate of beta looks about enough to lose all their detections. Finally, it would be dubious to claim the 10% significance they do with only 4 degrees of freedom, but even more so given their defence of the use of the contrafactual signal as having larger trend, effectively reducing the test to a simple test on sign, which has only a 50% significance. I hope to submit a comment to Nature in the next few days: it will be available to IPCC reviewers from the AOPP ftp site (I think at ftp://ftp.atm.ox.ac.uk/pub/ingram, but I cannot be certain as the system has crashed for the weekend). I am grateful to the authors of Min & al for thorough discussions. [William Ingram, United Kingdom]	
10-1568	10	43	45	43	45	"precipitation amounts". [J. Graham Cogley, Canada]	Taken into account. What we meant in the text was extreme precipitation, not extreme precipitation amount.
10-1569	10	43	46	43	47	Suggest replacing "by using a transform … to spatial scales" with "by independently transforming annual precipitation extremes in models and observations onto a dimensionless scale that may be more comparable between the two". [Francis Zwiers, Canada]	Accepted. Text modified.
10-1570	10	43	49	43	50	"be a 50% chance". "likelihood of detecting it". [J. Graham Cogley, Canada]	Taken into account. Text modified.
10-1571	10	43	49		50	Make “chance” & “likelihood” the same [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1572	10	43	50			“to detect” → “of detecting” [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1573	10	43	52	44	1	A clearer statement is required whether the AR5 has strengthened the conclusion of the AR4 regarding anthropogenic influence on global trend towards increases in the frequency of heavy pcpn events. One reads on pg 10-43, line 8, that new studies since the AR4 have "strengthened the expectation of increase in extreme precipitation", but it appears from reading this conclusing sentence that such evidence has not carried over to stronger evidence for detection of attribution. Clarity is needed here. [Martin Hoerling, United States of America]	Taken into account. Our assessment is the same as SREX. It is however difficult to make direct comparison between AR4 and AR5 assessments because of the use of different uncertainty guidance.
10-1574	10	43	55	43	56	Presumably "medium confidence" should be italicised. [Lisa Alexander, Australia]	Editorial
10-1575	10	43	56	44	1	What does “at the global scale” mean? As I said for SPM-4, line 2, when I asked the experienced colleagues who happened to be in the office with me, they had completely different ideas. Replace with whatever is meant [William Ingram, United Kingdom]	Taken into account. The wording is now more specific.
10-1576	10	43				variance' etc. (this paragraph; also see elsewhere). The phrase "should lead to" should be replaced by [Sucharita Ghosh, Switzerland]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1577	10	43				"should be accompanied by". There can be examples of time series data [Sucharita Ghosh, Switzerland]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1578	10	43				where the mean (trend) stays unchanged over time but the range changes. So, if one is only searching for [Sucharita Ghosh, Switzerland]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1579	10	43				changes in the mean, other aspects of distributional changes may be overlooked. [Sucharita Ghosh, Switzerland]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1580	10	44	1	44	2	Important to note earlier, because this applies to the whole chapter. What part of the message in the E.S. is only due to the changed uncertainty language applied? [Albert Klein Tank, Netherlands]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1581	10	44	1	44	2	My suggestion is to delete the sentence that begins with "The use of …". Basically, I disagree with the	Accepted. Text removed.

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						contention that comparison is not possible. The SREX assessment, which you repeat here, is a less confident assessment than the AR4 assessment, which did consciously make a likelihood assessment. Making a less confident assessment is fine, and that decision has already been defended in the SREX report - so I think it is sufficient to say here that the assessment of the WG1 AR5 report is consistent with that of SREX. [Francis Zwiers, Canada]	
10-1582	10	44	1			I assume “uncertainty guidance” means “ways of specifying uncertainty” [William Ingram, United Kingdom]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1583	10	44	2			Insert “the” after “in” for consistency with the rest of this section anyway [William Ingram, United Kingdom]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1584	10	44	4	44	42	Section 10.6.1.3 Drought: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	noted.
10-1585	10	44	4			This should be “Meteorological drought” because that is what you discuss. [Dáithí Stone, United States of America]	Taken into account. The assessment in SREX was not just meteorological drought.
10-1586	10	44	6	44	42	These statements are reasonable but have failed to mention the vital point that drought causes, rather than is caused by, elevated temperatures. Less heat energy is required when the evaporation process is reduced by the reduction of surface moisture, which means that the more of the arriving heat energy will heat the Earth's surface. [John McLean, Australia]	Taken into account. There is a positive feedback between temperature and (soil) moisture
10-1587	10	44	10	44	12	Add after "precipitation and temperature changes": "(noting the fact that temperature can only be indictectly related to drought trends, see Box 3.3 of that IPCC SREX chapter). In addition it assessed that there was low confidence in the assessment of changes in drought at the level of single regions". [Sonia Seneviratne, Switzerland]	Taken into account. Text is modified.
10-1588	10	44	12	44	12	What is "its"? I think you mean anthropogenic influence, but I can't be sure. To be clear, perhaps replace "based on its attributed impact" with "based on the attributed impact of anthropogenic influence". [Francis Zwiers, Canada]	Taken into account. Text is modified.
10-1589	10	44	14	44	15	"predominantly". "wind speed and solar radiation". [J. Graham Cogley, Canada]	Editorial
10-1590	10	44	14	44	15	You could add the following references for this statement: Sheffield et al. 2012, Seneviratne 2012. References: 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575; 2) Seneviratne, S.I, Nature, 491, 338-339. [Sonia Seneviratne, Switzerland]	Taken into account. New references added.
10-1591	10	44	14	44	25	This needs to refer to Ch 2. It also repeats a lot from Ch 2. This is another section that could be dramtically reduced. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Description of past changes is removed, with reference to Ch2.
10-1592	10	44	14			It is also a complex term. You did not discuss precipitation drought in the previous subsection and don't here either. But the importance of precipitation depends largely on what type of drought you are considering. I can think of types of droughts in which precipitation is irrelevant. And under climate change trends in many types of droughts will probably be driven by temperature rise rather than any precipitation change. [Dáithí Stone, United States of America]	noted.
10-1593	10	44	21	44	21	drought indices': Including the PDSI noted in the previous paragraph? [Government of United States of America]	Taken into account. Reference changed.
10-1594	10	44	21	44	24	Two published studies address the question of conflicting indications for changes in drought, which should be cited here. Hoerling, M., J. Eischeid, X. Quan, H. Diaz, R. Webb, R. Dole, and D. Easterling, 2012: Is a Transition to Semi-Permanent Drought Conditions Imminent in the U.S. Great Plains?. J. Climate. doi:10.1175/JCLI-D-12-00449.1, in press. Sheffiel, J.,, Eric F. Wood & Michael L. Roderick, 2012: Little change in global drought over the past 60 years Nature 491, 435–438 (15 November 2012) doi:10.1038/nature11575. [Martin Hoerling, United States of America]	Taken into account. References added.
10-1595	10	44	22	44	22	Add Dai 2012 and Sheffield et al. 2012 in the parenthesis [currently "(Dai, 2011; Sheffield and Wood, 2008)"]. References: 1) Dai, A. Nature Clim. Change http://dx.doi.org/10.1038/NCLIMATE1633 (2012); 2) Sheffield, J.,	Taken into account. References added

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						E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575. [Sonia Seneviratne, Switzerland]	
10-1596	10	44	22	44	23	"due to the examination of different time periods … as well as uncertainties in the output of land surface models (Pitman et al. …". It will be necessary here to consider Sheffield, J., E.F. Wood and M.L. Roderick, 2012, Little change in global drought over the past 60 years, Nature, 491, 435-438. [J. Graham Cogley, Canada]	Taken into account. Text modified. Reference added.
10-1597	10	44	22			Delete last comma [William Ingram, United Kingdom]	Editorial
10-1598	10	44	24			Omit “land use and” as its effects are included in land cover [William Ingram, United Kingdom]	Editorial
10-1599	10	44	25	44	25	Add the following sentence after "... (see also Deo et al. 2009)": "In a recent study, Sheffield et al. (2012) identify the representation of potential evaporation as solely dependent on temperature (using the Thornthwaite-based formulation) as a possible explanation for the tendency of common PDSI-based estimates to overestimate historical drought trends compared to other estimates. This stands in partial contradiction with previous assessments that suggested that using a more sophisticated formulation (Penman-Monteith) for potential evaporation did not affect the results of respective PDSI trends (Dai 2011, van der Schrier et al. 2011). Sheffield et al. (2012) argue that issues with the treatment of spurious trends in atmospheric forcing datasets and/or the choice of calibration periods explain these conflicting results.". References: 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature; 2) Dai, A. Wiley Interdisc. Rev. Clim. Change 2, 45–65 (2011); 3) van der Schrier, G., Jones, P. D. & Briffa, K. R. J. Geophys. Res. 116, D03106 (2011). [Sonia Seneviratne, Switzerland]	Accepted. Suggested text added.
10-1600	10	44	25	44	26	A couple of typos - insert "a" before significant (line 25), and replace "decade" with "decadal" (line 26). [Francis Zwiers, Canada]	Editorial
10-1601	10	44	25		26	Omit “low-frequency” as a more precise specification follows immediately [William Ingram, United Kingdom]	Taken into account. This comment is not relevant anymore. Text has been removed.
10-1602	10	44	26			The current text omits what one eventually realizes is the main point – add “due to internal variability” after “deficits” [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1603	10	44	27	44	28	What does "assessed in terms of attribution studies" mean? [J. Graham Cogley, Canada]	Taken into acocunt. Text modified.
10-1604	10	44	30		31	This reads like a general statement but of course it can't be – one couldn't have anomalies or droughts all the time as they are defined only by differing from reference conditions. I guess “under climate change” is the key point that's been omitted, but it needs clarifying whatever it means [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1605	10	44	37	44	42	The willingness, when adequately supported, to reverse the conclusions of prior assessments, adds credibility to the overall enterprise. Nicely done. I recommend that this new assessment statement also appear in the Executive Summary (see my comment #21). [Martin Hoerling, United States of America]	Noted.
10-1606	10	44	37	44	42	Very good! Thanks for the objective and precise evaluation! Actually, I disagreed strongly to the conclusion in reviewing this subsection of the AR4 draft, but unfortunately the authors did not accept our suggestion at that time. [Guoyu Ren, China]	Noted.
10-1607	10	44	38	44	38	Suddenly the text speaks about "hydrological droughts", before just about "droughts". If the "hydrological" carries weight, it needs to be introduced. [Jochem Marotzke, Germany]	Taken into account. "Hydrological" removed, to be consistent with previous text
10-1608	10	44	38	44	40	I agree with this assessment to the extent that it refers to global-scale trends in drought (which are not meaningful since there are drought trends of differing sign depending on the region). Although one can state that there is low confidence in most regional drought trends, it is important to note that most of the published studies agree on some regional changes. For instance, the regional drought changes highlighted in the SREX (since 1950: drying in southern Europe and West Africa and wetting in central North America and northwestern Australia) are robust even when assessed with the more recent study of Sheffield et al. (2012, Nature). See also comment of Seneviratne (2012, Nature). 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575; 2) Seneviratne, S.I, Nature, 491, 338-339. [Sonia Seneviratne, Switzerland]	Taken into account. The assessment now mention regional difference.

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10-1609	10	44	38			Explain “hydrological droughts”? [William Ingram, United Kingdom]	Taken into account. "Hydrological" removed, to be consistent with previous text
10-1610	10	44	39	44	40	"confidence about increasing trends … uncertainties and variable results from region to region". [J. Graham Cogley, Canada]	Taken into account. Text modified.
10-1611	10	44	41	44	41	Typo: "distinguishing" [Jochem Marotzke, Germany]	Editorial
10-1612	10	44	41	44	42	"distinguishing". "in the attribution of changes". [J. Graham Cogley, Canada]	Editorial
10-1613	10	44	41	44	42	I think readers will want to understand what the implication of this global assessment is for the SREX assessment that was described near the top of this page. It would be useful to say whether you consider them to be consistent with each other, because readers will, presumably, be most interested in the regional information in the SREX assessment. [Francis Zwiers, Canada]	Taken into account. There is now explicit statement that this assessment is consistent with SREX assessment.
10-1614	10	44	44	45	2	Subsection lacks a summary statement. [Jochem Marotzke, Germany]	Taken into account. A summary is added.
10-1615	10	44	49	44	50	The bit that refers to 10.3.3 is not very informative - is it possible to remind readers of the sense of that discussion here? [Francis Zwiers, Canada]	Taken into account. Text modified
10-1616	10	44	55	44	56	The openning part of this sentence reads awkwardly. Suggest replacing "The average global cyclone activity" with "Overall global average cyclone activity" [Francis Zwiers, Canada]	Accepted. Text modified as suggested.
10-1617	10	45	1	45	1	Add additional reference: Ulbrich et al. 2009 (Ulbrich U., G. C. Leckebusch, and J. G. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theor. Appl. Clim., 96, 117-131) [Urs Neu, Switzerland]	Taken into account. The paper is now cited.
10-1618	10	45	4			Tropical Cyclones: This section currently lacks reference to Chapter 14, Box 14.2. Please ensure consistency and careful cross-referencing between chapters. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Box 14.2 is now clearly referenced.
10-1619	10	45	6	45	19	Revise to provide additional and new information on the relation between SSTs and Atlantic hurricanes. The recent studies by Veechi and Soden (2007) and Ramsey and Sobel (2011) suggest the potential intensity of hurricanes in the Atlantic basin is controlled by the difference in SSTs locally from some tropical-wide average. The current draft text gives the incorrect impression that the relation is only to local Atlantic SSTs. I see that this point is addressed in the subsequent paragraph, nonethless I recommend that the first paragraph be revised to give a proper balance of views. [Martin Hoerling, United States of America]	Taken into account. This passage simply states findings from past assessments.
10-1620	10	45	6	45	19	The reasoning in this paragraph may be clearer to readers if you insert "merely" after "They concluded" in line 14. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text modified.
10-1621	10	45	11			“suggest” is weak enough. If there is only the appearance of a suggestion, it's not worth mentioning. Delete point or change “seem to suggest” to “suggest” [William Ingram, United Kingdom]	Taken into account. Text modified.
10-1622	10	45	21	45	21	Replace "gases emission" with "gas emissions" [Francis Zwiers, Canada]	Editorial
10-1623	10	45	23	45	23	Gillett et al 2008a should be 2008b [Fabrice Chauvin, France]	Editorial
10-1624	10	45	25	45	25	Reference to box 14.3 should be 14.2 [Fabrice Chauvin, France]	Editorial
10-1625	10	45	29	45	30	What is the relevance of "projections" in this chapter context? [Albert Klein Tank, Netherlands]	Taken into account. The logic is if it does not occur in future projection, it should not have occurred in the past as climate change would be stronger in the future.
10-1626	10	45	33	45	33	Remove brackets around Emanuel et al., 2012 [Government of Canada]	Editorial
10-1627	10	45	33	45	35	Suggest changing "reanalysis (as opposed to climate model) driving..." to "reanalysis using historical climate data, instead of climate model dynamical predictions, are closer in agreement with observations that indicate a late 20th century increase". Also suggest adding a sentence here to help explain why use of downscaling using reanalysis data might also be considered to provide more accurate predictions. [Government of	Taken into account. Text modified to improve clarity.

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						Canada]	
10-1628	10	45	33			Remove brackets from 2nd reference [William Ingram, United Kingdom]	Editorial
10-1629	10	45	38	45	38	Perhaps insert "Some recent" at the beginning of this sentence. [Francis Zwiers, Canada]	Accepted. Text has been modified.
10-1630	10	45	43	45	48	Seems somewhat strange that the same references are provided each time for these different views. [Albert Klein Tank, Netherlands]	Taken into acocunt. Text has been modified.
10-1631	10	45	43	45	48	The two papers by Villarini and Vecchi appear to support contradictory statements simultaneously. [Jochem Marotzke, Germany]	Taken into account. Text has been modified.
10-1632	10	45	44	45	48	Villarini and Vecchi suggest that both aerosols and internal variability are playing. They should appear only once [Fabrice Chauvin, France]	Taken into account. Text has been modified.
10-1633	10	45	46	45	48	There is a logic issue here in thet the two Villarini and Vecchi papers cannot be used to support two different arguments simultaneously as this is currently written. [Peter Thorne, United States of America]	Taken into account. Text has been modified.
10-1634	10	45	47	45	47	Zhang et al 2012 should be mentioned [Fabrice Chauvin, France]	Taken into account. Reference added.
10-1635	10	46	1	48	54	I find this section very good, clear and well written. The conclusion (p48, line 34-35: "We conclude that it is likely that human influence has substantially increased the probability of some observed heatwaves") is important and should be mentioned in the SPM. [Dim Coumou, Germany]	Thanks
10-1636	10	46	1	48	54	This section 10.6.2 must be reduced in length, and it requires a better focus. It reads too much like a set of individual, and needlessly detailed, summaries of various case studies. There is a lack of synthesis, and an overall lack of assessment. The exception is the issue of framing, discussed on pg 46. Overall, the reader is left wondering what is learned from event attirbution science that isnt already evident from more general analysis of observed changes in temperature extremes for the globe as a whole. The authors need to frame this science better, amd much more succinctly. My recommendation is to subsume the few core elements of this section within the section 10.6.1 on temperature extremes. This could be easily accomplished within a few sentences, and by adding to a list of current references in that section. [Martin Hoerling, United States of America]	Taken into account. Text has been revised.
10-1637	10	46	1	48		Visser and Petersen, 2012, Climate of the Past 8, p. 281, argue that event attribution is unwise to do. It is strong to show how probabilities change over time. Thus, a chance for hurricanes in a certain area might change, perhaps due to climate change. But do not try to pinpoint one specific event to (human-induced) climate change. There is no need to do that. The changing probabilities speak for them selves. Statistics is on groups of events, but not on one specific event. E.g., suppose we show a statistical correlation between smoking and long cancer. Now, we know a person who died from long cancer. But we cannot prove that this specific person died from long cancer due to smoking. You can keep the text as it is, but I believe you should mention this different view of event attribution. [Hans Visser, The Netherlands]	Taken into account. We believe this study is consistent with the event attribution framework discussed here -- havel referenced.
10-1638	10	46	1			This title suggests that the previous section concerned attribution of unobserved events. [Dáithí Stone, United States of America]	Accpepted. OK, have delete "observed"
10-1639	10	46	3	48	54	This section is excessively long and very long winded. It could be reduced dramtically. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Have tried, but note general comments from, eg Marotzke
10-1640	10	46	3			This “likely” statement should have include a reference to the appropriate WGII chapter if indeed it is accurate. [Dáithí Stone, United States of America]	Rejected. "many of" is uncontroversial, surely?
10-1641	10	46	6	46	8	Please revise to indicate that the Petersen et al. special issue of BAMS attempted to place extremes of 2011 in a "climate perspective" rather than a perspective of the "impact of external climate drivers" alone as implied. That coordinated assessment was focused on communicating how both natural and anthhropogenic factors may have contributed to extreme events. Also, the coordination was only in so far as events of a single year (2011) were examined via an organized arrangement of papers, but not a coordination of studies on a single extreme event. [Martin Hoerling, United States of America]	Accepted.

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10-1642	10	46	7	46	7	This reference should be Peterson et al. 2012 - also incorrect in reference list [Lisa Alexander, Australia]	Noted
10-1643	10	46	9	46	9	don't use "we": rephrase:... this assessment, selected studies were used to illustrate issues [European Union]	Will do
10-1644	10	46	12	46	20	Please revise to include the study by Hoerling and Co-Authors., 2012; Anatomy of an Extreme Event, JClimate, in press. This study applied the first two distinct ways of framing the question of how anthropogenic greenhouse has and aerosol forcing contributed to and affected the likelihood of the 2011 Texas heat wave. Concering "absolute risk" for weather event attribution, the cited reference to Hansen et al. is incorrect since no event analysis is done in that paper, rather a global analysis of the type done in many prior studies is performed. If no reference can be found, then I advise striking this sentence entirely. [Martin Hoerling, United States of America]	Accapted. Have clarified that there is little support for the "absolute risk"(or "would not have happened without") approach.
10-1645	10	46	14			Or decreased. [Dáithí Stone, United States of America]	Noted
10-1646	10	46	15	46	15	"occurrence of an event". [J. Graham Cogley, Canada]	Noted
10-1647	10	46	15			Insert "of" after "occurrence" [Chris Forest, United States of America]	Accepted
10-1648	10	46	19	46	19	Is there a difference between the use of 'likely' here, and the use of 'probability' in the rest of the paragraph? Also perhaps it should be noted that the results from this third approach can equivalently be easily expressed, at least in numerical terms, in the language of the 'attributable risk' of in the first approach, since both approaches consider occurrence probability/likelihood. [Government of United States of America]	Taken into acoount. No longer refer to a "third approach"
10-1649	10	46	19	46	20	It is not clear to me from this sentence how absolute risk would be different from attributable risk (except that in latter case, it is typical practice to report attributable risk as a ratio comparing the change in probability due to forcing with the probability of the event in a reference climate). [Francis Zwiers, Canada]	Taken into account. No longer refer to a "third approach"
10-1650	10	46	23	46	25	Some more recent publications on this topic include the following: Hirschi et al. (2011, Nature Geoscience), Mueller and Seneviratne (2012, PNAS), Quesada et al. (2012, Nature Climate Change). Mueller and Seneviratne (2012) additionally consider the specific case of the 2011 Texas drought and heat wave. References: 1) Hirschi, M., S.I. Seneviratne, V. Alexandrov, F. Boberg, C. Boroneant, O.B. Christensen, H. Formayer, B. Orlowsky, and P. Stepanek, 2011: Observational evidence for soil-moisture impact on hot extremes in southeastern Europe. Nature Geoscience, 4, 17-21, doi:10.1038/ngeo1032; 2) Mueller, B., and S.I. Seneviratne, 2012: Hot days induced by precipitation deficits at the global scale. Proceedings of the National Academy of Sciences, 109 (31), 12398-12403, doi: 10.1073/pnas.1204330109; 3) Quesada, B., R. Vautard, P. Yiou, M. Hirschi, and S.I. Seneviratne, 2012: Asymmetric European summer heat predictability from wet and dry Southern winter/springs. Nature Climate Change, 2, 736-741, doi:10.1038/nclimate1536. [Sonia Seneviratne, Switzerland]	Noted. However due to space considerations we have only cited one paper here as an example.
10-1651	10	46	25	46	27	The term "extreme-value theory" does not appear anywhere in the chapter, and this sentence may be a good place to mention it. Not all readers will know that it is possible to do what is described here, and to do it with some claim to rigour, in the absence of the constraint described in the previous sentence. [J. Graham Cogley, Canada]	Noted.
10-1652	10	46	25	46	27	Unclear whether this is generally true. Some rare extremes may be related to more common extremes indeed. Remember that engineers work from this assumption all the time for estimating extremes in the design of infrastructure. [Albert Klein Tank, Netherlands]	Taken into account. Clarified that this is a cautionary note, not a ban (engineers also extrapolate with caution)
10-1653	10	46	28	46	30	But if such a comparison could be made, would it not still be useful (at least to the extent that forcing, say over the 850-years prior to 1850, is small relative to anthropogenic forcing between 1850 and the present)? [Francis Zwiers, Canada]	Deleted second clause.
10-1654	10	46	37	46	38	What about the impact of the selected indicator, time period and spatial scale on which the event is analysed? [Albert Klein Tank, Netherlands]	Noted. Good point
10-1655	10	46	39	46	40	"of a given magnitude". [J. Graham Cogley, Canada]	yes
10-1656	10	46	42	46	43	"in terms of … (if human influence has more than doubled its probability of occurrrence).". The mathematical point being made here is slightly subtle, and plainer language would improve the chance of readers grasping	Taken into account in revision.

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						it. [J. Graham Cogley, Canada]	
10-1657	10	46	42			Change "term" to "terms" [Chris Forest, United States of America]	yes
10-1658	10	46	46	46	53	I really like the introduction and putting into context of the risk approach, and I appreciate that the debate on fractional attributable risk (and adaptation funds) is referenced (Hulme et al 2011). The IPCC SREX could be added a major reference for the definition of risk as a function of hazard and vulnerability. Most of the studies on attribution of extreme weather events use risk in a sense of risk of occurrence, and do not consider the above risk framework. This discrepancy may be made more explicitly. In terms of the risk approach a link to WGII chapter 19 on Key vulnerabilities and risks may be added. [Christian Huggel, Switzerland]	Accepted. The IPCC SREX is heavily cited in this chapter.
10-1659	10	46	48	46	49	Considering vulnerability to be unaltered here is useful for specific constrained questions, but is not an accurate assumption for the estimation of the change in risk due to emissions. In reality, vulnerability is dependent in the historical sequence of adaptation measures, which in turn depends on climate change. We would probably not be implementing climate change adaptation plans if climate change were not occurring. Almost all of the studies you discuss here did not consider the change in risk, but it's a shorthand we have used and it would be accurate to point that out. And consistent with what WGII are saying. [Dáithí Stone, United States of America]	Taken into account. Added a sentence on "risk" and "occurrence probability"
10-1660	10	46	49	46	49	To understand this, I think "hazard" needs to be defined. [Francis Zwiers, Canada]	Taken into account. OK, but space is tight and the word is standard
10-1661	10	46	50	46	50	"Fraction Attributable Risk": I believe this term originated with Stone and Allen, 2005b, and it is becoming common. I would like to suggest, before it is too late, that "Attributable Fraction of Risk" would be more clear-headed. It is the fraction, not the total risk, that is attributable in appropriately conducted analyses. For practical analytical purposes the total risk is a constant. No doubt there are always imponderable (and therefore unattributable) risks, but by definition nothing quantitative is known about them and they do not play any role in the analysis. [J. Graham Cogley, Canada]	It may be too late, but we will clarify that FAR does indeed mean the fraction of the risk that is attributable
10-1662	10	46	50			Insert "of" after "Fraction" [Chris Forest, United States of America]	Rejected. No, this is wrong (but widespread) -- see 10-1661
10-1663	10	46	52	46	53	No need to know P0 and P1: can a successful estimate be cited of the determination of FAR when P0 and P1 are unknown? In Pall et al. 2011, for example, FAR is calculated from empirical (modelled) estimates of P0 and P1. I cannot think of a way to do this without such estimates. [J. Graham Cogley, Canada]	Taken into account. Have clarified we mean that a systematic error scaling both P0 and P1 doesn't matter.
10-1664	10	46	53	46	53	One can wonder how reliable the estimate of the ratio is when the absolute estimates are far off. [Albert Klein Tank, Netherlands]	Will note (analogy with scaling-factors-being-consistent-unity issue earlier)
10-1665	10	46	57	46	57	"impossible" because the observed record is too short? If so, perhaps it is worth stating explicitly. [Government of United States of America]	Accepted
10-1666	10	47	1	47	5	While I agree about the use of a multi-step approach, what you describe here sounds like downscaling of a single-step approach. From my understanding it is multi-step only if some form of attribution occurs in all steps, but that is not obvious from what you describe here. [Dáithí Stone, United States of America]	This is consistent with the GPGP definition
10-1667	10	47	2	47	2	Insert "is" before "then used". [Francis Zwiers, Canada]	Accepted.
10-1668	10	47	2			Insert "is" after "model" [Chris Forest, United States of America]	Accepted.
10-1669	10	47	3	47	3	Replace "weather model" with "climate model"? I'm trying to imagine how this would be done - presumably one could estimate the trend in SSTs that is attributable to external forcing, and remove that component from observed SSTs to provide boundary conditions for a counter factual world. But wouldn't you still use an atmospheric climate model rather than a weather (forecasting) model? [Francis Zwiers, Canada]	We will clarify to just Atmospheric Model
10-1670	10	47	7			I don't know if after stating it is an example you need to then explain why it is an example, but in case you do I thought I should point out right now you don't explain it that way. What you describe is an attribution study of global SST changes downscaled to England and Wales runoff. [Dáithí Stone, United States of America]	Pall et al do consider competing hypotheses in the downscaling step (dynamic versus thermodynamic), so we would argue attribution applies at each step.

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10-1671	10	47	12	47	12	Please add 'and sea ice boundary conditions' directly after 'with composition and surface temperatures' to improve the sentence. [Government of United States of America]	accepted.
10-1672	10	47	15	47	15	How sensitive are these results for the choice of flood risk proxy; robustness for this choice seems important. See my earlier comment on the chosen indicator and scale (page 46, lines 37-38). [Albert Klein Tank, Netherlands]	Noted: the contrast between results of Pall et al versus Kay et al (2011) provide an illustration of this point.
10-1673	10	47	16	47	16	Delete the strange symbol between "that" and "including". [J. Graham Cogley, Canada]	Thanks
10-1674	10	47	16			Fix punctuation. [Chris Forest, United States of America]	Editorial.
10-1675	10	47	28	47	31	It is not clear what the source of data is for the figure in panel c. [Chris Forest, United States of America]	In the reference
10-1676	10	47	40	47	40	"Coumou". [J. Graham Cogley, Canada]	Thanks
10-1677	10	47	40	47	40	Rahmstorf and Connou (2011) --> Rahmstorf and Coumou (2011) [Dim Coumou, Germany]	Thanks, and apologies
10-1678	10	47	47	47	47	Please be consistent and use either "heat-wave" or "heatwave" [Oliver David Andrews, United Kingdom]	Will do
10-1679	10	47	47	47	50	The Dole et al (2011) looked just at July and not at the 'Summer - JJA' [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. This does not change the assessment.
10-1680	10	47	47	47	57	Could mention that the Russian Heat Wave occurred at the same time as the Pakistan floods. Maybe in future you'll be able to look at multiple events at the same time. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted, but the relevant literature does not exist.
10-1681	10	47	47	48	24	Please also refer to Trenberth, K. E., and J. T. Fasullo, 2012: Climate extremes and climate change: The Russian Heat Wave and other Climate Extremes of 2010. J. Geophys. Res., 117, D17103, doi: 10.1029/2012JD018020. which shows that a global perspective is necessary to unravel the influences on the Russian heat wave, and that record high SSTs played a major role by forcing anomalous atmospheric heating that set up teleconnections and led to the unusual persistence and intensity of the blocking anticylone associated with the Russian heat wave. Moreover, studies with models showed that they were unable to adequately simulate the monsoon circulation or blocking critical in this event and that previous studies that concluded it was largely natural had not taken this into account. On the contrary, the event was shown to be dependent on the SST anomalies and thus global warming. [Kevin Trenberth, United States of America]	Will cite
10-1682	10	47	51	47	51	"display". [J. Graham Cogley, Canada]	OK
10-1683	10	47	56			Given that long-lasting weather anomalies in mid-latitudes are often associated with blocking events, is there any idea whether and how observed and/or modelled changes in the general circulation of the atmosphere may affect the distribution/frequency of such blockings? [Government of France]	Cross reference to Ch09 needed here
10-1684	10	48	2	48	2	Change "relating" to "distinguishing between". [J. Graham Cogley, Canada]	We mean relating
10-1685	10	48	3	48	15	Both of these interpretations (the magnitude of a return value versus the change in probability for a fixed threshold) would, presumably, be affected by model bias. Perhaps that should be mentioned? [Francis Zwiers, Canada]	Model bias is mentioned in this section.
10-1686	10	48	12	48	12	Rupp et al. (2012c) and Rupp et al. (2012b) are the same paper. [David Rupp, United States of America]	Noted, thanks.
10-1687	10	48	17	48	17	Which events - the Russian and Texan heat waves? [Francis Zwiers, Canada]	Will clarify
10-1688	10	48	26	48	28	A very confusing sentence. Suggest "In summary, many recent studies have found increased probability of occurrence of extremely high temperature events, related in large part to the large-scale warming since the mid-20th century. Many such events have been observed to occur in recent decades (refs)." [James Renwick, New Zealand]	Will clarify
10-1689	10	48	26	48	38	Revise to clarify that there is no strong evidence that the weather patterns have changed in their frequency or intensity from which the extreme events studied to date emerged (e.g. blocking as in the case of Russia 2010, or La Nina as in the case of the 2011 Texas drought/heat wave), Rather, for the case of temperature and the	We will clarify the distinction between whole events and individual records

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						exteme event values which occur in concert with natural weather/climate fluctuations, there has been an increase in the probability of exceeding a prior record temperature threshold, owing mainly to backgrond warming not a detected change in variability. The text needs revision to distinguish between the notion of an "event" understood holistically as being the physical phenonemon itself (often an expression of natural variability), rather the notion of an "event" as being s single metric associated with the phenomenon (such as a record heat value). A revision is needed to carefully communicate this distinction. Reference to the study of Hoerling et al. (2012) might be useful, since that study provides a discussion on this matter. In addition, the concluding statement is problematic, and requires revision. Surely the statement that human influences have increased the probability of "some" observed heat waves is not surprising or unexpected, given the evidence for a warming planet. Nor is that summary statement even capturing the most interesting aspect of the event attribution science, or some of the most important results coming from the interpretation of causes for extreme events. Elsewhere in Chapter 10 one is given an assessment that various measures of heat extremes have increased since the mid-20th century, very likely due to anthropogenic forcing. That assessment was not drawn from diagnosis of individual events, but from global analysis. But they could almost certainly have been inferred from simple considerations of the superpositioning of natural variability and a background mean warming. This concluding statement weakens that assessment on the anthropogenic contribution to observed extremes (pg 10-4, line 35). And, it also raises the question of how "some" is defined. To date, a mere handful of case studies have been done. Are more needed, when exploring the narrow perspective of event attribution science as being an effort to quantify the anthropogenic effect alone? [Martin Hoerling, United States of America]	
10-1690	10	48	26		38	General comment: As for estabilishing a cause-effect relationship between two series, some caution is needed. [Sucharita Ghosh, Switzerland]	Noted
10-1691	10	48	28	48	28	Rupp et al. (2012c) and Rupp et al. (2012b) are the same paper. [David Rupp, United States of America]	Thanks
10-1692	10	48	29	48	30	Doesn't this over-interpret "very like most" a bit? The events in question are regional, whereas the "very likely most" assessment is global. An approach such as that of Christidis et al (2012b) could be used to do a regional attribution on the basis of a global D&A study. [Francis Zwiers, Canada]	Will qualify
10-1693	10	48	29	48	31	This statement is unsustainable. The large scale warming is attributable to the ENSO system being dominated by conditions on the El Nino side of absolutely neutral (I.e. SOI=zero) and heatwaves are attributable to stationary or near stationary pressure cells. Both appear to be driven by natural forces therefore the claim that anthropogenic forces "very likely" drive extreme warm temperatures cannot be sustained. [John McLean, Australia]	Attribution of large-scale warming is addressed elsewhere
10-1694	10	48	34	48	35	Is this a statement? What is “some”? It could be two or it could be thousands. [Dáithí Stone, United States of America]	With few specific studies, we can't say more than "some"
10-1695	10	48	35	48	36	The only literature quoted here that applies to this statement is the Kay study, unless you think Pall was close enough to precipitation too. Either way, they both look at the same event over a small island, so it is hard to see how “general” conclusions can be made. [Dáithí Stone, United States of America]	See 10-1697
10-1696	10	48	35			Fix the punctuation before "Attributable" or combine the sentences. [Chris Forest, United States of America]	Yes
10-1697	10	48	36	48	36	Can one generalize given the limited number of studies of precipitation events that are currently available? I agree that this is what we expect, so perhaps this could say something like "It is expected that attributable risks for extreme precipitation events will be generally smaller and more uncertain, consistent with the findings in Pall et al (2011)." [Francis Zwiers, Canada]	Good suggestion
10-1698	10	48	36	48	38	"... often using a single model...": I am missing in this discussion the issue of model reliability. We expect for instance that the representation of processes related to droughts or floods is strongly model dependent. This may have affected the assessment of attributable risk in the case of heatwaves affected by drought feedbacks (e.g. 2003 heat wave) or in studies investigating changed risks in flood occurrence (e.g. Pall et al. 2011). It would be helpful if the authors could expand the discussion on this point. [Sonia Seneviratne, Switzerland]	Yes
10-1699	10	48	40	46	42	Add references to Dole et al. (2010) and Hoerling et al. (2012) to support the statement. [Martin Hoerling, United States of America]	Will do

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10-1700	10	48	40	48	45	These paragraph is just repeating things already said. [Dáithí Stone, United States of America]	Disagree
10-1701	10	48	40	48	54	These summary paragraphs are too cautious and convoluted. Where comprehensive studies have been undertaken (e.g. Pall et al) a significant impact of increasing GHGs has often been shown. There is obviously a need for more studies but to say evidence does not support is incorrect. [European Union]	Will clarify that Pall et al did not show "extremely unlikely in the absence of human influence" in the absolute sense
10-1702	10	48	47	48	47	Rather than saying this is "much easier", perhaps this should say that we have greater confidence in evaluating contributions to changes rather than absolute risks. Even so, if the probability of the event is wrong in the reference climate that is simulated by a climate model, we should expect that we will make errors in the estimate of the change in risk. This would happen both because the number in the demoninator is wrong, and because the change is being estimated from a place where the shape of the simulated distribution is different from that in the real world (i.e., different points on the Gaussian density function, with different local slopes). [Francis Zwiers, Canada]	Noted. The dependence of these results on models is emphasised in the revised text.
10-1703	10	48	47	48	48	Throughout this section I think the argument that the convention is to use the term “risk” is good enough for sticking with it. But here it sounds like you are actually trying to talk about risk, but you are ignoring all the various factors that are driving changes in risk everwhere. [Dáithí Stone, United States of America]	Will clarify our use of risk earlier
10-1704	10	48	51	48	52	Huh? Some of these events were unprecedented in the historical record, and thus “extremely unlikely” to occur, no matter which climate. [Dáithí Stone, United States of America]	Noted. Unprecedented stuff happens all the time.
10-1705	10	48	51	48	54	This statement reads like a rejection of evidence such as that presented by Pall et al. 2001 (and perhaps Hansen et al. 2012), and as such it should be deliberated on carefully by the chapter team. In particular, I find the adverb "extremely" to be problematic. Would the evidence support the claim if the claim were about events that would be just "unlikely"? [J. Graham Cogley, Canada]	Will clarify this is a reference to absolute risks in the absence of human influence
10-1706	10	48	53			delete this sub section. It is weak and filled with conjecture [tim barnett, United States of America]	Not clear what this refers to
10-1707	10	48	56			(section 10.7) The claims made in this section cannot be sustained while natural forces are poorly modelled because improvements in the modelling of those forces might produce quite different results and conclusions. [John McLean, Australia]	Rejected. The uncertainty in modelling and forcing and the robustness of conclusions are discussed. However, to address the comment, a sentence discussing robustness has been added.
10-1708	10	48	56			Section10.7: Long term perspective. Please ensure consistency and careful cross-referencing with chapter 5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted,drafts have been exchanged with chapter 5 for the SOD and again for the final draft.
10-1709	10	48				While there may be perfect correlation, between two time series, one series need not be the 'cause' of the other. [Sucharita Ghosh, Switzerland]	Noted, this is discussed in the introduction.
10-1710	10	48				Partial linear models address this to some extent where the aim is to regress one series on another, while [Sucharita Ghosh, Switzerland]	Rejected as unclear
10-1711	10	48				both series may have some trend. Beran and Ghosh (1998) examine two global temperature series [Sucharita Ghosh, Switzerland]	Rejected, over the period considered the time series behaviour is much more complex than a trend. Furthermore, several results discussed are based on analysis to 1900, when trends due to greenhouse gases are small.
10-1712	10	48				to illustrate this method for time series with long-memory correlations. (continued below) [Sucharita Ghosh, Switzerland]	see previous response
10-1713	10	48				Full reference: [Sucharita Ghosh, Switzerland]	Do not understand which reference this refers to
10-1714	10	48				Beran, J., Ghosh, S. (1998) Root-n-consistent estimation in partial linear models with long-memory errors. [Sucharita Ghosh, Switzerland]	Noted.
10-1715	10	48				Scandinavian Journal of Statistics, 25: 345-357. [Sucharita Ghosh, Switzerland]	see previous response
10-1716	10	48				Contd. Methods such as these and related applications have potential for addressing some of the challenges [Sucharita Ghosh, Switzerland]	Rejected, report has to be based on published results.

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10-1717	10	48				in the problem of signal detection and attribution. [Sucharita Ghosh, Switzerland]	see previous response
10-1718	10	49	1	49	1	Should "late" be deleted since the main focus of this section is the period before the 20th century? Also, perhaps replace "for" with "of the". [Francis Zwiers, Canada]	Accepted
10-1719	10	49	1	60	18	These sections are just far too long. They need to reduce by a significant fraction - at least 50%. They repeat what is in the Chapters and what has been earlier - and they repeat too much from AR4. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. The section has been reduced. However, continued and continuing iteration with chapter 5 and 9 ensures that there is no overlap beyond handover, and other sections do not discuss the detection of forced responses.
10-1720	10	49	5	49	5	Change "The section here focuses on" to "We assess" [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, while use of 'we' is discouraged by TSU, sentence has been rephrased
10-1721	10	49	24	49	26	This isn't quite right. It is easy to include the spectral variation in solar irradiance and it will have more of an effect in models with more resolved stratospheres, but all models have absorption by lower stratospheric ozone and so will include this effect. A bigger effect that hasn't been widely applied (though this is changing in the historical CMIP5 runs is the response of ozone itself to the changes in UV, either through an a priori calculation (Eyring et al, 2012, submitted) or a parameterisation (Schmidt et al, 2011). [Government of United States of America]	Accepted, sentence rephrased
10-1722	10	49	30	49	31	The criticisms of Mann et al., 2012, by Anchukaitis et al., published in Nature Geoscience on 25 November 2012 ((http://www.nature.com/ngeo/journal/vaop/ncurrent/index.html) should be cited as well as the original paper. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Accepted, however, rthe Mann et al reference has now been removed following comment 1723. and referring to chapter 5
10-1723	10	49	30	49	32	This point about trees not responding to major volcanic eruptions is more than just speculation - it is wrong speculation. Leave this point to Ch 5 to dismiss which they have. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted, reference has been removed
10-1724	10	49	40	49	40	Insert "the" before "response". [Francis Zwiers, Canada]	accepted
10-1725	10	49	40	49	43	I recommend: "However, this is only the case all relevant forcings and their uncertainties are considered to avoid fictitious correlations between external forcings, and if the data are homogeneous and statistical tests properly applied (Legras et al., 2010)". [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks
10-1726	10	49	43	49	43	Suggest replacing "fictitious" with "spurious" - fictitious could be interpreted as being invented rather than being a result of sampling variability. [Francis Zwiers, Canada]	accepted.
10-1727	10	49	48	49	48	Suggest replacing "well-defined robust climatic periods in the last Millennium" with "well-defined periods in the last Millennium that can be robustly identified". [Francis Zwiers, Canada]	sentence has been reworded
10-1728	10	49	49	49	49	Clarify that these are the same type of models used elsewhere. [Albert Klein Tank, Netherlands]	accepted, clarified.
10-1729	10	49	49	49	49	do you mean " by climate models of the last millenum" or " simulations of the last millenium?" [John Mitchell, United Kingdom]	accepted, sentence rephrased
10-1730	10	49	49	49	49	"...simulated by climate models of the last…" [James Renwick, New Zealand]	accepted, sentence rephrased
10-1731	10	49	49	49	49	Replace "model" with "models". [Francis Zwiers, Canada]	accepted
10-1732	10	49		52		Section 10.7.2: Attention should be also paid to the detection of the historical position of the last 5-decade warming in the past millennium, or whether or not the recent warming is abnormal in terms of the historical climate change, in addition to the attributions of the reconstructed temperature to external forcings. It is also a little bit weak in discussing the role the solar forcing in driving hemispheric and regional paleo-temperature changes, considering the numerous publications relative to the topic (e.g. Ge, Q. S., J. Y. Zheng, X. Q. Fang, Z. Man, X. Q. Zhang, P. Y. Zhang, and W.-C. Wang, 2003: Winter half year temperature reconstruction for the middle and lower reaches of the Yellow River and Yangtze River, China, during the past 2000 years. The Holocene, 13, 933-940; Zheng, J. Y., Q. S. Ge, and X. Q. Fang, 2002: Seeing the 20th century warming from temperature changes of winter-half-year in eastern China for the last 2000 years. Acta Geographica Sinica,	Rejected. The relative warmth of the recent period in the historical context is addressed in chapter 5, and the suggested focus on reconstruction, not detection and attribution and hence are material for chapter 5.

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						57, 631-638. (in Chinese); Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977; Holmes, J.A., E.R. Cook, B. Yang, 2009. Climate change over the past 2000 years in Western China, Quaternary International, 194, pp. 91–107). [Guoyu Ren, China]	
10-1733	10	50	3	50	5	On line 3, I suggest rephrasing "forcing significantly contributed" as "forcing contributed significantly". On line 5, I would delete "significantly" because detectable implies scaling factors that are significantly different from zero (otherwise it would not be detectable). [Francis Zwiers, Canada]	Accepted, thanks
10-1734	10	50	9	50	9	Would it be better to say "decreases earlier in time" rather than "decreases over time"? The latter usually implies time marching forward. [Francis Zwiers, Canada]	accepted
10-1735	10	50	10	50	11	As I understand the sentence and as Fernandez-Donado deals with pre-PMIP3 simulations itself, I would use, line 11, "PMIP3 simulations" rather than "pre-PMIP3". [Hugues Goosse, Belgium]	accepted
10-1736	10	50	13	50	13	We feel it is worth pointing out that the response to solar is likely muted in the runs being discussed here because of the neglect of ozone variability (i.e. Shindell et al, 2006) and this might be conflated with uncertainties in the forcing in fingerprint studies. There may also be changes in the fingerprint itself as a function of additional mechanisms. [Government of United States of America]	Partly accepted - fingerprints in time only would be less affected by this then spatial response patterns, while the ozone feedback would affect amplitude most, which is accounted for in the analysis method. However, a caution has been added referencing Shindell et al. 2006 and discussing this point.
10-1737	10	50	21	50	21	"Results…results" is a little confusing. [Government of Canada]	accepted, reworded
10-1738	10	50	21	50	21	"Results (Figure 10.18) show that the simulations generally reproduce…" [James Renwick, New Zealand]	accepted
10-1739	10	50	21	50	21	The sentence needs a bit of word smithing - perhaps replace "Results (Figure 10.18) show that the results generally … " with "Figure 10.18 shows that the EMIC with data assimilation generally…". At the end of the sentence, it might help to say what conclusion to draw from the result - maybe add "indicating that the combination of the forcing and assimilated reconstruction are well able to constrain the EMIC over most of the millennium, therefore increasing confidence in the consistence of the reconstruction and the forcing." [Francis Zwiers, Canada]	accepted, thanks
10-1740	10	50	21	50	22	This last section is very vague - "generally", "generally", "possibly". Please try to use more quantitative language. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, language has been made more quantitative and text expanded.
10-1741	10	50	25	50	47	Presumably the CL line in the plot is CU? What is the scake on the left of Fig 10.18? It can't be deg C, so is probably deg C times 10, but this doesn't look right. Also there is no base period given for this plot - assume it is in anomalies? The extra information doesn't help in the Technical Details. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	accepted, figure revised, typoes in y-axis fixed (apologies) and base period given in caption.
10-1742	10	50	33	50	33	M8 and M9 not just M8 [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks
10-1743	10	50	43	50	43	Luterbacher et al.'s area was 35°N-70°N, 25°W-40°E [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, fixed, thanks
10-1744	10	50	51	51	32	Earlier comments on Chapter 8 call for clarification of remarks concerning the multi-decadal impact of volcanic eruptions. The e-folding time for stratospheric aerosols from individual high-latitude eruptions is quoted there to be 3-4 months, which make it difficult to understand how there could be a significant contribution to low frequency variability from an individual eruption unless of extreme magnitude. However the reference on page 10-51, line 19 to "substantial pulses of volcanism" makes a significant multi-decadal volcanic impact on climate variability much more understandable. But (although I might have missed it) I did not find reference to spells of enhanced volcanism in Chapter 8. See comments 240 and 246. [Adrian Simmons, United Kingdom]	Noted. Text has been revised
10-1745	10	50	54	50	54	This section open up more issues that are probably best in Ch 5. The large volcanic eruption needs to be given. Was it the event in 1453? This occurred after the cooling began. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	accepted, sentence removed
10-1746	10	50	55	50	55	Can the date of the eruption be given? [Francis Zwiers, Canada]	rejected, sentence removed.

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10-1747	10	50	57	50	57	Replace "fingerprint for" with "fingerprint of". [Francis Zwiers, Canada]	accepted
10-1748	10	50		51		Multi-century to millennial scale: Authors should communicate with Chapter 5 authors to ensure consistency in their treatment of this subject. [Government of United States of America]	noted, however, section is written in close collaboration with chapter 5
10-1749	10	51	10	51	10	Please consider adding the following "a role for orbital forcing in high-latitude trends" to improve this statement. [Government of United States of America]	accepted
10-1750	10	51	16	51	50	This section is also long and doesn't say much more than was in Ch 5. It all seems more like a Review than an Assessment. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted, section has been removed, with some of the material sharpened and merged into other sections. Any remaining overlap with chapter 5 has been removed
10-1751	10	51	18	51	18	Best cross-reference for LIA greenhouse gases is Section 6.2, Figure 6.7. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Thanks, accepted
10-1752	10	51	18	51	19	Clarify. Were there two pulses separated by a quiet 18th century, or a period of ~200 years of enhanced volcanism? [J. Graham Cogley, Canada]	accepted, sentence slightly revised
10-1753	10	51	21	51	21	"Larger estimates". [J. Graham Cogley, Canada]	accepted
10-1754	10	51	21	51	21	"Larger estimates of solar…" [James Renwick, New Zealand]	accepted
10-1755	10	51	26	51	27	Why "in contrast"? These two lines are discussing different issues and are not contrasted. [Government of United States of America]	accepted, sentence revised
10-1756	10	51	29	51	29	Suggest moving the bit in parentheses defining the periods to the being of the paragraph where the LIA is first introduced. [Francis Zwiers, Canada]	rejected, this relates to a specific study not the entire LIA
10-1757	10	51	31	51	32	The single model simulation used in Miller et al (2012) to suggest that there was a long term response in the NADW to the large eruptions is not particularly convincing (no assessment of multi-decadal internal variability in NADW was presented), and AFAIK hasn't been replicated in other models. Thus the text should not suggest that this is a general result. This line is in contrast to the Palastanga et al (2012) result mentioned above though. [Government of United States of America]	accepted, section removed.
10-1758	10	51	34	51	34	Little Ice Age (LIA) and Medieval Warm Period (MWP) are two classical terms widely used in paleo-science community. Much evidence have been accumulated to support the claim of existence of the MWP in AD 1000-1300 in Euro-Asian continent. Term Medieval Climate Anomaly was suggested by researchers whose interest is mainly focused on more recent change in climate, and it is not acceptable for majority of paleo-scientists. It is better that the term be changed back to the MWP. [Guoyu Ren, China]	terminology has been based on chapter 5's choices.
10-1759	10	51	34	51	47	It is not clear here whether the MCA is considered a regional or a global 'event'. [Government of United Kingdom of Great Britain & Northern Ireland]	section revised and sharply shortened, this question is addressed by ch5, referenced.
10-1760	10	51	36	51	36	Illustration cross-references should be Figure 5.9 d,e,f and Figure 10.18. [David Parker, United Kingdom of Great Britain & Northern Ireland]	thanks, accepted
10-1761	10	51	40	51	40	"change in forcing due to changes in land use" [J. Graham Cogley, Canada]	accepted
10-1762	10	51	40	51	40	Frank et al. reference should be 2010. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks
10-1763	10	51	43	51	44	This seems a bit open ended because it doesn't tell us what caused the long-term circulation changes. Is there any thought on that? [Francis Zwiers, Canada]	rejected, no published research available (CHECK)
10-1764	10	51	49	51	50	Is it possible to justify the quantification of "very likely" eg taking into account significance in formal studies, allowing for reconstruction and modelling uncertainties, and any qualitative physical arguments? [John Mitchell, United Kingdom]	accepted, logic explained
10-1765	10	51	50	51	50	Please consider adding "and" in between "reconstructions" and "forcings" to improve this sentence. [Government of United States of America]	apologies, cant find this

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10-1766	10	52	6			… ensemble). (typo) [Government of France]	accepted, thanks
10-1767	10	52	11	52	14	This sentence made no sense to me as currently written. I suspect some words are missing. [Peter Thorne, United States of America]	Sentence revised
10-1768	10	52	17	52	21	The description of volcanic eruptions and responses is related to tropical eruptions, the extratropical eruptions don't necessary have the same response (in 8.6.2.2 it is emphasized that the response is for tropical eruptions). [Gunnar Myhre, Norway]	accepted, clarified, although the paper in question uses both all strong and only tropical eruptions.
10-1769	10	52	19			… detectable (typo) [Government of France]	accepted
10-1770	10	52	24	52	25	This is a bit unclear - it's not obvious if this is saying that the reconstructions lie outside a broad spread of model results in some instances, or that the models lie outside broad uncertainty ranges on the reconstructions. [Francis Zwiers, Canada]	Sentence removed
10-1771	10	52	30	52	30	p52, line 30. Not sure why Esper et al (2012) is discussed here, that was not a detection and attribution study, and many people have suggested a role for orbital forcing in high latitude millennial trends (Mann et al, 1999; Bauer et al (2003), Kaufmann et al (2009)). [Government of United States of America]	accepted, sentence removed
10-1772	10	52	33	52	34	Can a confidence level be assigned to this summary statement? [Thomas Stocker/ WGI TSU, Switzerland]	accepted, rephrased
10-1773	10	52	51	52	52	Solar activity is also a climate forcing in the changes in Northern Hemispheric temperatures in the past 1000 years, which should be added. Please revise this conclusion based on the content 10.7. [Xuemei Shao, China]	rejected, evidence for solar forcing is not assessed to be strong given degeneracy with other forcings that can be identified with more confidence.
10-1774	10	52	52	52	53	"confirm". Should "combined with forcing" be "combined with external forcing"? [J. Graham Cogley, Canada]	accepted, reworded
10-1775	10	52	56	52	56	"supports and strengthens". [J. Graham Cogley, Canada]	accepted
10-1776	10	52	57	53	2	Do you mean"medium confidence" rather than "medium evidence"? I think medium confidence would be quite a conservative assessment - it sounds to me that there is more than medium confidence (high confidence?) that external forcing has played a role in the evolution of the preindustrial millennium, even if uncertainty remains for some parts of the record and full quanitification of the role external forcing is not possible. [Francis Zwiers, Canada]	accepted, statement reworded, but assessment remains
10-1777	10	53	4			Section 10.8. This section overlaps considerably with 12.5.3 and 12.5.4. I don't think it makes sense to a reader for there to be sections on this in different chapters. I have a made a similar comment on ch12. [Jonathan Gregory, United Kingdom]	Taken into account by clarifying the scope of this section and its contribution to the overall assessment at the beginning of the section. Otherwise rejected, chapter 10 assesses evidence from studies of observed change.
10-1778	10	53	4			Section 10.8.: While I fully recognize the high importance of TCR and ECS I think 10.8.1. and 10.8.2 could benefit from a considerable reduction of the text. Furthermore I suggest to make the definition of TCR eventually adopted in this report clearer. I understand it is a more generic definition, but similar to a normalized TCR which is indicated as the rate of warming per year. Eventually it is not clear to me whether the magnitude of forcing is still the same as in AR4, ie. a doubling of CO2 (I guess it is). In particular I recommend reconsideration of the respective paragraph in the Exec Summary where TCR, TRCE and ECS are all given without definition and I doubt that anybody reading this ES will understand it properly. [Christian Huggel, Switzerland]	Taken into account, the sections have been tightened up and revised. The terminology in the ES has been explained.
10-1779	10	53	4			(section 10.8) Unless the papers cited in this section can be shown to accurately incorporate all climate forces they should be disregarded, in fact competent reviewers of these papers should have either rejected them or demanded comprehensive statements as to what the models simulated and how thoroughly. The IPCC does everyone a disservice by citing the output of papers based upon flawed models. [John McLean, Australia]	Taken into account both in assessment already, and by adding a further callout to the role of forcing uncertainty in revision.
10-1780	10	53	4			Equilibrium climate sensitivity (ECS) is discussed quite extensively in section 10.8. But it is also discussed at length in Chapter 9. Does it need to occur in two separate Chapters, particularly adjacent ones? [Adrian Simmons, United Kingdom]	Taken into account by explaining the scope of text in chapters 9, 10 and 12. deletion rejected

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10-1781	10	53	7	53	7	Perhaps replace "predict" with "constrain projections of"? In the near term, the constrained projections are predictions, but farther into the future, the projections become dependent upon the choice of emissions pathway, whether constrained or not. [Francis Zwiers, Canada]	Accepted, thanks
10-1782	10	53	10	53	11	"near-term" is not defined in this context. A more precise time-scale would be helpful. [Chris Forest, United States of America]	Accepted, clarified
10-1783	10	53	15	53	17	We feel that this use of ECS in Solomon et al (2009) was incorrect and should not be repeated here. The ECS calculation assumes that many things will be stable that in the long term will certainly not be (vegetation in particular, but also atmospheric composition of aerosols and ozone, ice sheet extent and height, and ocean dynamics). Thus we do not feel that a statement should be made that relates the metric of ECS to the real eventual temperature as a function of the stabilised CO2 level. [Government of United States of America]	Taken into account, text has been revised
10-1784	10	53	18			Presumably this refers to Box 12.2 on climate sensitivity. [Reto Knutti, Switzerland]	Accepted, thanks
10-1785	10	53	20			Forest, Chris	No action required. Just a name.
10-1786	10	53	29	53	46	It should be made clear that TCR is only a useful concept when considering cases of steadily increasing forcing over a fixed 70 year time period. Although TCR is not sensitive to the rate of a steady increase in forcing, as well as depending on the increase being fairly steady, the transient response to a given total increase in forcing is sensitive to the period of time involved. For instance, EBM simulations driven by steadily increasing forcing over periods of 50, 70 and 100 years show the transient response to the same period-total increase in forcing increasing from 1.33 to 1.52 to 1.72 C as the period is increased over that range, using an ECS of 3.0 C and ocean parameters corresponding to fairly typical AOGCMs (mixed layer depth 75m, upwelling-diffusion model with effective vertical diffusivity Kv 4 cm^2/s). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Rejected. TCR still provides the better indicator of the response on a broad range timescales as parameters are varied
10-1787	10	53	29	53	46	Furthermore, the variation of transient response with time period is sensitive to the underlying combination of ECS and Kv that gives rise to the TCR. Using the same EBM model but with an ECS of 1.9 and Kv of 0.3 cm^2/s, levels broadly consistent with observational data, also produces a TCR of 1.52 C over 70 years, but in this case the variation in transient response to the same total increase in forcing occurring over periods from 50 to 100 years is only from 1.44 to 1.59 C, rather than from 1.33 to 1.72 C. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted
10-1788	10	53	32	53	32	What about a response to constant radiative forcing? You could imagine CO2 emissions that occur at a rate such that a given TOA radiative imbalance (forcing), once established, is maintained at a constant level over time. That would be slightly different than "a gradual increase in forcing", which would imply a growing radiative imbalance over time (i.e., one that outpaces the rate at which the planet can reestablish equilibrium). [Francis Zwiers, Canada]	Noted. One could imagine this scenario, but why? It seems strange.
10-1789	10	53	34	53	34	Delete "over" (2nd last word in the line). [Francis Zwiers, Canada]	Accepted.
10-1790	10	53	50	53	51	We feel that this statement is incorrect and ask that the authors correct it. The "eventual" response to 2xCO2 would be the Earth System Sensitivity (Lunt et al, 2011; Hansen et al 2010). If the response is considered to be driven by emissions, and the carbon cycle is included, the eventual response (after 100,000 years or so) will be zero. [Government of United States of America]	Will clarify link to ESS
10-1791	10	53	50	53	51	It is not, as implied here, only after stabilisation that ECS eventually becomes the relevant climate system property. With continuing steady forcing, as the time period increases beyond 70 years the "transient" response of the climate system gradually increases from the TCR (defined as the response over 70 years) towards ECS, reaching fairly close to ECS over a timescale of a few hundred years even if ECS is as high as 3 C (assuming that an upwelling-diffusion ocean model applies). Simulations using an EBM with an ECS of 3 C, a 75m mixed layer, effective vertical diffusivity Kv 4 cm^2/s and diffusion reduced by upwelling with an e-folding depth scale of 1000m, driven by linearly increasing forcing that doubles over the period concerned, give a temperature rise of 1.52 C over 70 years (TCR), increasing to 2.10C after 200 years, 2.53 C after 500 years and 2.74 C after 1000 years. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Note that the upwelling-diffusion formulation makes results more dependent on time-scale than, e.g. Held's 2-box model. Not clear which simple model is "better"
10-1792	10	53	53	4	45	this section is quite incomprehensible; maybe try to re-write it [European Union]	Taken into account. Section has been revised.

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10-1793	10	53	60			this subsection is really something for WGII. Perhaps it is redundant w.r.t Chapter 12. it seems out of place here. Suggest deleting or reducing to 1-2. pages the rest of the material would be a fine paper for publication elsewhere. [tim barnett, United States of America]	Misplaced comment?
10-1794	10	54	9	54	9	We recommend the addition of a caveat to this justification in the systematic limitations of the energy balance models. [Government of United States of America]	There are numerous caveats on model limitations throughout this chapter.
10-1795	10	54	14	54	14	“ECS; Section 10.8.4”: should be 10.8.2 [Martin Juckes, United Kingdom]	Noted.
10-1796	10	54	16			The Stott and Forest (2007) study used results from an earth system model of intermediate complexity (EMIC) and not an energy balance model. This needs to be corrected by deleting "energy balance model (EBM) and inserting "for an earth system model of intermediate complexity (EMIC)" after parameters. [Chris Forest, United States of America]	Noted, thanks.
10-1797	10	54	23			It would make more sense to give the cross-reference to Figure 10-19 in the first line of this paragraph. [Chris Forest, United States of America]	Accepted.
10-1798	10	54	24	54	24	Slightly lower values than what? Presumably the AR4 range, but please state explicitly. [Government of United States of America]	Accepted
10-1799	10	54	25	54	25	The citation "Gillett et al. 2011a" is actually 2012: Gillett, N. P., V. K. Arora, G. M. Flato, J. F. Scinocca, and K. von Salzen (2012), Improved constraints on 21st-century warming derived using 160 years of temperature observations, Geophys. Res. Lett., 39, L01704, doi:10.1029/2011GL050226. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Thanks
10-1800	10	54	31	54	32	Figure 10.19 shows far more than 3 estimates of TCR but nonetheless excludes some of the works cited in the text. What is your criterion for including/excluding studies from Figure 10.19? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Clarified.
10-1801	10	54	45	54	45	Delete "where". [David Parker, United Kingdom of Great Britain & Northern Ireland]	OK
10-1802	10	54	48	54	48	TCR is necessarily lower than ECS [because of unrealized warming in the transient case] - is probably worth saying. [Government of United States of America]	Section has been clarified.
10-1803	10	54	48	54	50	This sentence does not logically follow from the previous one. [Government of United States of America]	Taken into account. Deleted "Hence"
10-1804	10	54	52	54	54	Camp and Tung 2007, while on a subject related to this discussion, doesn't match what you say here. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Wrong paper is cited. Has been corrected.
10-1805	10	55	9	55	10	Please consider revising the sentence in the following way: "assuming some constant boundary conditions such as ice sheet extent, vegetation etc." Alternatively, please consider merging this line with line 17-20 which is more coherent and demonstrates clearly that the previous descriptions of the utility of ECS on p53 cannot be correct. [Government of United States of America]	accepted, sentenced revised
10-1806	10	55	23			What exactly is mean by single response timescales? Most EBMs if the use an ocean do have multiple timescales asseciated with ocean and atmosphere. Suggest clarification. [Reto Knutti, Switzerland]	Accepted, sentence rephrased
10-1807	10	55	28	55	36	Non-informative priors also exist, or at least this is what some authors think, myself included. I developed this point in a recent paper (Pueyo, S., 2012: Solution to the paradox of climate sensitivity. Climatic Change, 113, 163-179). Bayesianism has two major branches: the subjective and the objective. Objective Bayesianism has just entered the climate sensitivity debate, and I understand that the authors of the chapter do not necessarily have to be convinced of this approach, but I think that it should be mentioned, if only for completeness. At the very least I would mention that it exists with some uncompromising sentence such as “A non-informative prior distribution has been suggested (Pueyo, 2012), but this needs further evaluation.” I also suggest modifying the sentences that imply that Bayesian statistics always involves prior information or prior beliefs. For example, this set of sentences could be rewritten as follows: “As discussed in the AR4, such estimates are inherently based on Bayesian statistics and, therefore, even if it is not explicitly stated, involve using some prior distribution. This distribution may imply particular information or beliefs. The prior shapes the sampling distribution of the models and, since the constraints by data on transient warming is fairly weak, results are	Accepted, paper is cited and text has been expanded similar to suggestion

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						sensitive to the prior constraints or assumptions (Aldrin et al., 2012; Sansó and Forest, 2009) . Constraints on the upper tail of ECS are particularly weak if the assumed prior distribution levels off for high sensitivities, as is the case for uniform priors (e.g., Frame et al., 2006). Uniform priors have been criticised (e.g., Annan and Hargreaves, 2011), but, in principle, this point applies to any prior: if the data do not distinguish between a high and very-high value for ECS, their relative probability must be determined by the prior. A non-informative prior has been suggested (Pueyo, 2012), but this needs further evaluation". [Salvador Pueyo, Spain]	
10-1808	10	55	28			It is not clear why only a single reference (Tanaka et al. 2009) is mentioned here. If this is only in reference to the "optimization" methods then this should be clarified. [Chris Forest, United States of America]	Taken into account, reference has been moved to link it to forcing uncertainties
10-1809	10	55	29	55	30	Non-Bayesian, transfer function type approaches like Knutti (2006) and Piani (2005) should be discussed here. These methods are not directly sensitive to the prior disrtibution of sensitivities in the ensemble. [Government of United States of America]	Sampling strategy also in such approaches indirectly introduces prior assumptions, hence rejected; although sentence has been revised to improve
10-1810	10	55	32	55	36	Whilst literally true, the statement that if the data do not distinguish between a high and very-high value for ECS, their relative probability must be determined by the prior is highly misleading. Over the instrumental period, temperature changes are known much more accurately (have much lower errors relative to the size of the change) than changes in net forcing and ocean heat uptake and/or radiative imbalance. Moreover, these latter, larger, errors have distributions which do not have sharp cut-offs – indeed their distributions are generally considered to be approximately Gaussian. It follows that instrumental-observation based estimates of the climate feedback parameter lambda will have distributions which are approximately Gaussian (see, e.g., Roe and Baker, 2007, Science; Lewis, 2012, Climatic Change (submitted)), or at least which do not have a sharp cut-off at low lambda, rather declining in a broadly similar fashion to a Gaussian or Student's t distribution. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by revised discussion of use of priors.
10-1811	10	55	32	55	36	On the foregoing basis, application of the standard Jacobian formula for converting PDFs on a change of variables implies that as lambda becomes very small and, correspondingly, ECS very high, the slope of the PDF for ECS should decline with 1/S^2. That is because, in that region, a unit change in S will correspond to a tiny change in lambda, over which the PDF for lambda will change little. This is a matter of simple mathematics, although it does not appear to be universally recognised in climate science. If the prior used in noninformative, the PDF for ECS will indeed decline with 1/S^2. If the PDF for ECS declines less fast than with 1/S^2 at very high S, as many of the PDFs illustrated in AR4 WG1Fgure 9.21 and in Figure 10.19 (bottom) of this draft of AR5 WG1 appear to, despite the data not distinguishing between different values, the prior used must be introducing a bias towards very high levels of ECS, and the results cannot be objectively valid. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by expanding the section on sensitivity of results to priors
10-1812	10	55	32	55	36	I am assuming that, as is generally acknowledged, there is no separate information that very high values of ECS are more likely than merely high values. It must be understood that what shape a prior must have to be noninformative, so that inference is objective, depends on the form of the likelihood function for the experimental model concerned, and hence on the relationship between the parameter(s) and the data. A noninformative prior has no direct interpretation in probabilistic terms (see, e.g., Bernardo and Smith, Bayesian Theory, 1994). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by expanded discussion of priors
10-1813	10	55	32			The work by Annan 2011 Climatic Change discusses priors in quite some detail. [Reto Knutti, Switzerland]	Accepted, now cited
10-1814	10	55	34			This reference should presumably be to A+H 2011 "On the generation and interpretation of probabilistic estimates of climate sensitivity. Climatic Change, 104(3-4)" which is not listed in the references. [James Annan, Japan]	Accepted, apologies
10-1815	10	55	45	55	45	Not clear why Meehl et al (AR4) is being cited here in a statement providing the updated AR5 findings. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This statement referred back to the AR4 findings as a point of departure. However, writing has been clarified.
10-1816	10	55	51		52	See also Church et al. (GRL 2011) [John Church, Australia]	Thanks, accepted
10-1817	10	55	55			This point was already made by Knutti 2002 Nature. [Reto Knutti, Switzerland]	Accepted, cited
10-1818	10	56	1	56	3	I think it would be necessary to discuss the aspects of climate variability that are not accounted for in this	Taken into account, text revised

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						study. [Francis Zwiers, Canada]	
10-1819	10	56	2	56	2	Delete "(Figure 10.19)" from this line as it does not appear to display climate variability. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, reference was misplaced within sentence, fixed
10-1820	10	56	4		4	Sensitivity ranges should be in a consistent format throughout and stated in terms of the standard IPCC 'likely' range. [Government of United States of America]	taken into account by rewording the sentence, however within this text referring to published results is prefereable to translating the ranges.
10-1821	10	56	6	56	6	Define "sampling range". [Francis Zwiers, Canada]	accounted for, sentence revised
10-1822	10	56	9	56	23	Erroneous statistical formulations and data processing errors at various stages have been revealed by study both of the computer code used in Forest et al (2006) (as made available by Dr Forest at http://svante.mit.edu/research/IGSM/data/IGSM_1//GRL06_reproduce.tgz) and of the intermediate and final results produced by running that code on the associated data. The same code, or at least the key parts in it, also appears to have been used for Forest et al (2002), Forest et al (2008) and Libardoni and Forest (2011). Particular problems that have been identified are: [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by discussing errors in text, referring to Lewis 2012, and showing a revised version of the resulting pdfs in the figure based on erratum.
10-1823	10	56	9	56	23	a) the likelihoods are computed using the cumulative distribution function (CDF) for an F-distribution rather than, as should be the case, its probability density function (PDF); [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1824	10	56	9	56	23	b) no geometrical correction is made to convert the likelihood from a function of the error sum-of-squares to a function of the underlying error variables; [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1825	10	56	9	56	23	c) the F-distribution used to compute the likelihood relating to the deep ocean temperature trend has its argument divided by 3, and its first degree of freedom parameter set at 3, whereas the divisor and DoF parameter should both be only 1; [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1826	10	56	9	56	23	d) deep ocean observational errors are added directly to internal variability rather than being added in quadrature, which, since these error sources are completely independent, overstates the combined error; [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response; referring to erratum
10-1827	10	56	9	56	23	e) although the methods description states that the error sum-of squares is computed as the (EOF truncated) inverse of the climate noise (natural internal variability) covariance matrix as estimated from segments of an AOGCM control run, pre and post multiplied by the difference vector between the observation and model data, the error sum-of-squares is, for the upper air and surface data, reduced therefrom by division by a factor of 1.25; [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1828	10	56	9	56	23	f) in addition to the unwarranted division by 1.25, the code appears substantially to miscalculate the error sum-of-squares for upper air data, for unknown reasons. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1829	10	56	9	56	23	The overall effects of these errors, all of which relate to processing after the data has been put into the form required for computing model – observation differences (diagnostic data), are unknown. The reduction in the discriminatory power of the three sets of model – observation differences arising from errors c), d) and e), in particular, could well be substantial. (Although the upper air data may appear to have little effect on parameter inference, when correctly calculated it does help constrain estimation of ECS at low to moderate Kv.) Furthermore, errors e) and f) may have led, at least in relation to the upper air data, to an invalid choice for the key truncation parameter. When correctly calculated, it appears that the minimum error sum-of-squares value using the Forest et al (2006) upper air data and chosen truncation parameter may well be inconsistent with the statistical model, using the preferred criterion in Allen and Tett (1999), cited in that study. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1830	10	56	9	56	23	Turning to items that relate specifically to the Forest et al, (2006) GRL paper: g) the turnkey computer code released by Dr Forest, acting on the accompanying data, does not in fact correctly reproduce the marginal PDF graphs in the Forest et al. (2006) paper. The differences vary in	see 1822 response

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						magnitude between the parameters and are significant for Kv. This has been verified by running the code both under Unix and Windows versions of IDL. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-1831	10	56	9	56	23	h) following the release by Dr Forest of data and code used in Forest et al (2006), I have been able to resolve the differences between the two differently processed versions of the model surface diagnostic data relating to that study, for both of which results are presented in Lewis 2012. Both sets of surface data appear to have been misprocessed, in that neither set of model data relates to a period ending in the same year as does the observational data. The timing mismatch is substantially greater for the dataset from the study that, although stated in Forest et al (2006) to have used its data, did not actually do so. There is also a masking discrepancy in the model surface dataset from that study. In addition, significant misprocessing of the upper air data appears to have occurred in both datasets, the sources of which it has not been possible to identify. It is uncertain whether a timing mismatch of the surface data, and misprocessing of the upper air data, also affect Forest et al (2002), Forest et al (2008) and/or Libardoni and Forest (2011). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1832	10	56	9	56	23	Until the statistical errors, of both principle and calculation, affecting the processing of diagnostic data used in all four of the studies referred to have been corrected, it seems inappropriate to cite results from them. Whilst Lewis (2012, submitted) is not affected by those errors, it is affected by the misprocessing of both sets of data relating to Forest et al (2006) – as, in relation to the dataset that it used, is Forest et al (2006) itself. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see 1822 response
10-1833	10	56	9	56	23	The principal conclusion of Lewis (2012), that use of uniform priors for the parameters results in very substantial overestimation of the probability that ECS is very high, as compared with using an objective Bayesian method that correctly relates volumes in data space to volumes in parameter space, is unaffected. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by adding a discussion of the role of priors and by showing pdfs based on different priors in figure 10.20, now expanded; generally, no uniform prior results are shown in isolation where results using multiple priors are available
10-1834	10	56	14	56	14	Should "effective climate sensitivity" be "equilibrium climate sensitivity"? If not, define "effective climate sensitivity". [Francis Zwiers, Canada]	taken into account, 'effective climate sensitivity' now defined earlier in text.
10-1835	10	56	17	56	23	Although the main reason for Lewis' low sensitivity is the dataset, the method used also helps, and I do not think that it is correct. In a recent paper (Pueyo, S., 2012: Solution to the paradox of climate sensitivity. Climatic Change, 113, 163-179) I developed a non-informative prior distribution for climate sensitivity based on different criteria, with substantial evidence in its favour. This problem could be mentioned. For example, in lines 19-20, the fragment “However, this author also presents two very different results (...)” could be replaced by “However, there is disagreement between Lewis (2012) and Pueyo (2012) about the correct way to apply the objective Bayesian approach in this context. Furthermore, Lewis presents two very different results (...)” [Salvador Pueyo, Spain]	partly accepted, Pueyo is now referenced and discussed
10-1836	10	56	18	56	18	What is an "objective Bayesian method" (what makes it distinct from other Bayesian methods?). Does this refer to Empirical Bayesian techniques in which the prior is estimated from data? [Francis Zwiers, Canada]	taken into account, by referring to an expanded section on priors discusses objective methods.
10-1837	10	56	19	56	22	The Lewis (2012) study is flawed. When using the data from the unpublished study, Lewis (2012) did not use the correct segment of the annual data to compare the model simulations and observational data. This is known to the author. [Chris Forest, United States of America]	taken into account in revision
10-1838	10	56	29	57	3	Good discussion. [Robert Kandel, France]	noted
10-1839	10	56	29	57	3	This whole section feel svery jumbled and like someone is calling a boxing match. There is a lack of clarity and structure and a glaring lack of any meaningful assessment and there is no summary. The section should be completely rewritten for clarity and a meaningful assessment undertaken. I suspect this assessment will conclude that observationa / methodological uncertainties make the whole thing moot. The section should refer to and be consistent with the discussion of radiation within Chapter 2 (Section 2.3). [Peter Thorne, United States of America]	as the comment above and the one two down shows, this view is not generally shared, and there is an assessment although we accept it needs to be pulled out stronger, which has been done. Overall, taken into account by revising text and crossreferring to Section 2.3
10-1840	10	56	31	56	46	I suggest that the language is used in this paragraph is neither even handed nor appropriate for a scientific report, and should be changed to a neutral tone. In particular, stating that Spencer and Braswell (2008)	Taken into account by doublechecking evenhandedness of assessment. However, where

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						"suggest" but Murphy and Forster "show" is not a neutral description of the disagreement between the two studies, nor is saying that Lindzen and Choi "claim that climate models overstate" neutral wording. I suggest replacing all of "suggest that", "show that" and "claim that" by "present evidence that". [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	results are assessed to be based on flawed assumptions, this needs to be expressed in writing
10-1841	10	56	31	57	22	I approve of devoting some space to these low feedback estimates. I think the biggest problem with Lindzen and Choi is that they fail to include a forcing term in their energy balance equation. So their energy balance "model" is unphysical and simply wrong! [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Thanks, noted, this has been referred to in the text
10-1842	10	56	46	56	52	Very long run-on sentence - perhaps some punctuation is missing? [Francis Zwiers, Canada]	accepted, sentence revised
10-1843	10	56				Section 10.8.2.4:Please check with authors of chapter 5 for consistency in their treatment of patterns, processes and interpreations of the MCA and LIA. Expand on proxy-contained sensitivity. [Government of United States of America]	Taken into account, this section has been iterated earlier but ihas been further synchronized
10-1844	10	57	24			See also Hargreaves et al (GRL 2012 in press) for a new (albeit tentative) estimate of climate sensitivity based on the PMIP2 models and the most recent proxy data. [James Annan, Japan]	accepted, now cited
10-1845	10	57	30	57	30	Maybe replace "is not important" with "were small relative to the present"? [Francis Zwiers, Canada]	taken into account, text revised
10-1846	10	57	39	57	39	Delete one "a" before "small". [Christian-D. Schoenwiese, Germany]	accepted, thanks
10-1847	10	57	40	57	40	It isn't justified to neglect uncertainties. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted
10-1848	10	57	40	57	40	Say something about which uncertainties are neglected? [Francis Zwiers, Canada]	accepted, text revised
10-1849	10	57	49	57	49	The insertion of see also here in the middle of a run of four references seems very odd and unnecessary. It should be deleted. [Peter Thorne, United States of America]	Rejected, this is useful information
10-1850	10	57	49	57	50	Something seems to be missing - perhaps EndNote has obliterated the break between sentences here (should there be a sentence break after the Otto-Bliesner reference?). [Francis Zwiers, Canada]	accepted, revised
10-1851	10	57	50	57	50	Line should probably begin "Otto-Bliesner et al., 2009). Holden et al., 2010) analyzed…" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account, Text revised
10-1852	10	58	4	58	7	On the very longest timescales also by the distribution of land and ocean. [Peter Thorne, United States of America]	taken into account, text revised
10-1853	10	58	6	58	6	On these very long tome-scales, changes in Earth's geography also make comparisons more difficult. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, text revised
10-1854	10	58	9	58	9	Explain "Charney-type ECS". I assume this is the ECS that is discussed throughout the chapter (i.e., assuming no very long timescale feedbacks). [Francis Zwiers, Canada]	accepted, text expanded
10-1855	10	58	9	58	11	When is the deep past? Earlier than 800K years before present? [Francis Zwiers, Canada]	accepted, textx has been clarified
10-1856	10	58	10	58	10	Change "1.1°C to 7.0°C" to "1.5°C to 5.2°C" to be consistent with Chapter 5, page 20, line 25. [David Parker, United Kingdom of Great Britain & Northern Ireland]	taken into account, text has been synchronized with chapter 5
10-1857	10	58	21	58	22	Statements here seem subjective without a clear reference: "will not be small"; "very high or very low" [Albert Klein Tank, Netherlands]	accepted, this part of sentence has been deleted
10-1858	10	58	21			We feel that the sentence is rather vague. Please either remove it or define 'small' greenhouse warming. [Government of United States of America]	accepted, this part of sentence has been deleted
10-1859	10	58	23		28	This section should be expanded, as it provides a useful reference to debunk the most commonly cited papers advocating an unrealistically low climate sensitivity. I suggest that each of the papers is taken in turn, specifically stating the problematic assumptions involved, rather than lumping all papers into a list. [Government of United States of America]	rejected, as detailed discussion of these papers has been done earlier in 10.8.2
10-1860	10	58	26	58	26	Lin et al reference should be 2010a (and the Lins in the citation list are different people!) [David Parker, United	accepted, thanks!

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						Kingdom of Great Britain & Northern Ireland]	
10-1861	10	58	43	58	45	We would suggest adding "for estimates of climate sensitivity" to this sentence, to avoid this being misinterpreted as a general statement. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
10-1862	10	58	46			FAQ 10.1: "We must first detect -- who is "we" referring to? The Chapter authors, the IPCC, the D&A science community? Suggest to avoid personal nouns and to rephrase as, e.g., "one must first detect" or "…must first be detected." [Thomas Stocker/ WGI TSU, Switzerland]	MISPLACED comment
10-1863	10	58	47	58	53	Evidence from observations does not appear actually to support the assessment that ECS is 'likely' to be in the range 2 C to 4.5 C. That range appears to reflect also AOGCM simulations and, perhaps, a desire not to alter the 'likely' range given in previous IPCC synthesis reports. I set out below in some detail what best estimate of ECS the observational evidence set out in AR5 WG1appears to support. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	taken into account, assessment based on observed climate change has been revised, emphasizing thevery robust lower limit at 1.0 and widening the 'likely' range downwards. Note that the overall assessment is based on all lines of evidence assessed in 10.8.2 not only recent observed change
10-1864	10	58	47	58	53	a) Trend estimates over 1880-2011 for all three of the main global temperature data sets are very similar (Table 2.7). The decades ending in those two years correspond to similar positions in the quasi-periodic AMO cycle. Moreover, per Figure 8.18 those two decades were similarly, and only modestly, affected by volcanic activity, and had similar levels of solar forcing. The difference in the global mean temperatures for the decades to 1880 and 2011 per HadCRUT4, the dataset with the highest 1880-2011 trend, gives a temperature change of 0.73 C. ENSO indicators show a greater tendency to La Nina relative to El Nino type conditions during the decade to 1880 than that to 2011, so if anything this 0.73 C increase in temperature is likely to overstate the underlying rise in global temperature. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	noted, see response 1863
10-1865	10	58	47	58	53	b) Satellite-based estimates for total direct plus indirect aerosol forcing (AFari+aci) in 2011 are –0.73 ± 0.6 W/m^2 (Section 7.5.3). This observationally based estimate is slightly below the 0.90 ± 0.6 W/m^2 overall estimate given, which includes GCM based and inverse estimates (Section 7.5.3). The mean Total Adjusted Forcing (AF) for the decade to 2011 per Figure 8.18 is 2.02 W/m^2. I have extrapolated to 2011 the last data point shown in Figure 8.18, which contrary to what is indicated therein is actually for 2010 not 2011, by reference to the recent trend in Total Anthropogenic AF and 2010 to 2011 changes in solar and volcanic forcing. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Aerosol forcing assessed in chapter 7; estimates revised; also see response 1863
10-1866	10	58	47	58	53	c) Figure 8.18 uses the 0.90 W/m^2 composite observational/ AOGCM derived estimate for 2011 total Aerosol Adjusted Forcing (AF); changing to the purely observational best estimate increases the mean AF in the decade to 2011 from 2.02 to 2.19 W/m^2. Deducting the mean Total AF for the decade to 1880 of 0.09 W/m^2 per Figure 8.18 gives a change in AF between the two decades of 2.10 W/m^2. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see response 1863, aerosol estimates have been revised
10-1867	10	58	47	58	53	d) Per Box 3.1, Figure 1, almost all (95%) of the Earth's energy change, which corresponds to its integrated radiative imbalance, is reflected in ocean heat uptake. Observational estimates of the increase in ocean heat content (OHC) over the past several decades are 0.3 W/m^2, 70% of which relates to the better observed 0-700m deep layer (Section 3.2.3/3.2.4). Figure 1 in Levitus et al., 2012 (World ocean heat content and thermosteric sea level change (0-2000). Geophys. Res. Lett.), shows, using data up to end 2010, an increase in 0-2000 m OHC at very close to 0.45 W/m^2 over the final decade, with a reducing rate of increase post 2005. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	assessment revised
10-1868	10	58	47	58	53	e) The increase in OHC below 2000m, together with heat uptake in other parts of the climate system, is considered to be very small (under 10% of the total): see Figure 3.3 a). Gregory et al 2002 (An Observationally Based Estimate of the Climate Sensitivity, Jnl. Climate) gave a modelling-derived estimate for ocean heat uptake circa 1880 of 0.08 W/m^2. There is, however, little observational evidence to support this. Since the difference between ocean heat uptake in the decade to 1880 and global heat uptake excluding the ocean 0-2000m layer in the decade to 2011, whilst of uncertain sign, will be extremely small, I will take it to be zero. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Noted. This is assessed in chapter 5; and reflected in the energy box in chapter 13.
10-1869	10	58	47	58	53	f) Deducting, therefore, the full 0.45 W/m^2 post 2000 increase in 0-2000 m OHC per Levitus et al. 2012 from the 2.10 W/m^2 increase in AF between decades ending 1880 and 2011 gives a forcing change net of	Noted, estimates based on recent data are now discussed referring to Otto et al. 2013

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						radiative imbalance of 1.65 W/m^2. That provides, when divided by the corresponding increase in global surface temperature of 0.73 C, an estimate of the climate feedback parameter of 2.26 W/m^2/K. Converting this to an estimate for ECS by dividing it into 3.71 W/m^2/K, the radiative forcing corresponding to a doubling of CO2 (Myhre, 1998), gives an estimate for ECS of 1.64 C. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	
10-1870	10	58	47	58	53	g) The purely observationally-based estimate of 1.64 C for ECS is close to the border of the "very unlikely" range, and shows that the "likely" ECS range of 2 to 4.5 C is unsupported by direct calculation from the observational evidence as reflected in the AR5 WG1 best observationally-based estimates. Even using the composite best estimate of Aerosol forcing that is partly based on AOGCM simulations and inverse calculations, the estimated ECS of 1.8 C is still well below the bottom of the 'likely' range. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	See response 1863
10-1871	10	58	47	58	53	h) The main results of Aldrin et al. (2012) give a best estimate for ECS of between 1.6 and 1.7 C, even when using a uniform-in-ECS prior. Their best estimate using a uniform-in-climate-feedback-parameter prior is between 1.4 and 1.5 C. (These figures represent the mode of the PDF for S, not its mean as quoted in the study. The mean is an inappropriate central measure for a highly skewed distribution like that for ECS.) [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	see response 1863
10-1872	10	58	47	58	53	i) Using data covering 1851 to 2010, Gillett et al (2012a) obtain a central estimate for TCR of 1.54 C. They show that this relatively low TCR is due to the use of longer period than usual, with TCR estimates using the common 1901-2000 period being affected by the abnormally cool first two decades of the 20th century. EBM simulation, using ocean heat uptake parameters that result in increases in ocean heat content consistent with the observational estimates in Chapter 3, implies that the value of ECS consistent with a TCR of 1.54 C lies in the region of 1.8 to 2.0 C. Like most TCR studies, Gillett et al. relies on the accuracy of patterns of temperature change simulated by an AOGCM. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	noted, however, this paragraph is about ECS
10-1873	10	58	47	58	53	j) If the Gregory et al (2002) study, whose PDF for ECS was featured in AR4 WG1 Figure 9.21, were repeated using the estimates of the time evolution of Total Anthropogenic Adjusted Forcing per Figure 8.18, the change in the mean such forcing between 1861-1900 and 1957-1994 would be some 0.90 W/m^2, far higher than the 0.36 W/m^2 change used in Gregory (2002). Accepting Gregory (2002)'s decline of 0.01 W/m^2 in solar + volcanic forcing between those two period means, which is consistent with RCP45 forcing data, and the ocean heat uptake figures used in the study despite them being based on the erroneous (too high) Levitus et al 2000 dataset, the resulting central estimate for the climate feedback parameter is (0.89 - 0.16) / 0.335 = 2.18, implying a central estimate for ECS of 1.70 C. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account, revised estimate using Gregory method has been published and is referenced and shown (Otto et al., 2013)
10-1874	10	58	47			"likely" and "very likely" in this paragraph should be in italics. [Reto Knutti, Switzerland]	accepted
10-1875	10	58	52	58	53	This is contradictory to the definition of ECS on page 55 (lines 9-10). Is it "equilibrium" or not? What timescales are usually used for estimation? [Erica Thompson, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks, definition on page 55 has been clarified
10-1876	10	58	56	59	5	Suggested tidy-up of caption of Figure 10.19 which is a bit consfusing at present: Distributions of the transient climate response (TCR, top) and the equilibrium climate sensitivity (bottom). PDFs and ranges (5–95%) estimated by different studies (see text) [DEFINE THE CIRCULAR ECS SYMBOLS]. The dark (light) grey shaded TCR range marks the likely (very likely) range of 1°C–3°C for TCR as assessed in this section. The dark grey shaded ECR range marks the likely range of 2°C–4.5°C for ECR as assessed in this section. The figure compares some selected old estimates used in AR4 (no labels) [THESE ARE UNCLEAR] with new estimates available since (labelled). Distributions are shown where available, together with 5–95% ranges. Ranges that have been queried in the literature or have problematic assumptions are labelled by arrows at the border. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks
10-1877	10	58		58		add "The paper by Semenov and Latif (2012) indicates that Arctic wide sea ice area reduction in winter time during the Early 20th Century Warming could have been comparable to what is presently observed, although their results are based on atmospheric model simulations and refer to winter anomalies only." Citation:	MISPLACED comment

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						Semenov, V.A. and M. Latif (2012) The early twentieth century warming and winter Arctic sea ice, The Cryosphere 6, doi:10.5194/tc-6-1-2012. [Douglas Maraun, Germany]	
10-1878	10	59	26	59	42	The issue of model vertical mixing is discussed here and in two other sections as alluded to in an earlier comment (p.33-23). But each time I have got a slightly different take home message. Great care is required to make sure these discussions are all consistent and that they are cross-referenced. Better would be to try to bring all such discussions into one place but I recognize that this may not be possible. [Peter Thorne, United States of America]	taken into account, cross-referenced to ocean section and checked against it
10-1879	10	59	36	59	36	Citation should be Forest et al. (2008). [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted, thanks!
10-1880	10	59	36	59	44	The tendency of AOGCMs to have excessive deep ocean heat uptake may make only a modest difference to their TCRs. However, it will mean that large differences in their ECS are compatible with giving rise to the same estimates of TCR, and hence similar simulated increases in global temperatures over the 20th century. For instance, simulations with an EBM (with a 75m mixed layer depth) show that a TCR of 1.28 K is compatible with an ECS of 3.0 K and an effective ocean vertical diffusivity Kv of 4 cm^2/s - not untypical values of those parameters for AOGCMs. It is also consistent with an ECS of 6.5 K and a Kv of 8 cm^2/s, within the range of those parameters for AOGCMs. But a TCR of 1.28 K is equally compatible with an ECS of 1.6 K and Kv of 0.4 cm^2/s. These ECS and Kv values are consistent with purely observationally based estimates – for ECS per Forster and Gregory, 2006, Aldrin et al, 2012, and as per my comments on the 'likely' range for ECS; and for Kv per Hoffert et al, 1980, Forest et al 2006 and Lewis 2012 using the same dataset as Forest et al 2006. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Taken into account by revising text on effective ocen diffusivity.
10-1881	10	59	36			Forest and Reynolds (2008) should be replaced with the correct reference: Forest et al. (2008) in Tellus A. [Chris Forest, United States of America]	accepted, thanks
10-1882	10	59	42	59	43	Does this have implications for D&A results? Shouldn't this lead to scaling factors that are less than unity, since ocean area more than twice the land area, and variability over oceans is generally lower (such that optimization in global D&A analyses of surface temperature would tend to favour oceans). [Francis Zwiers, Canada]	taken into account by cross-referencing to ocean detection results
10-1883	10	59	51	59	51	I would think that for readability it would be required to define, in brackets, what is meant by 'emission floors' here. [Peter Thorne, United States of America]	Rejected. We think this is clear from the context.
10-1884	10	60	2			TCRE and TRCE used in different chapters for the same quantity. In analogy to TCR which quantifies the transient warming to a forcing I suggest TCRE, the response to emissions. From the unit it is implicit that this refers to cumulative emissions. [Reto Knutti, Switzerland]	Accepted. TCRE is now used.
10-1885	10	60	9	60	9	Describe units (TtC). [Francis Zwiers, Canada]	Taken into account. Units are now written out in full when first used.
10-1886	10	60	9	60	14	Suggest not using ESM for a simple model, because ESM is used for CMIP5. Meinshausen 2009 uses a simple EBM, not an EMIC. C4MIP should have a reference. [Reto Knutti, Switzerland]	Taken into account. We now call it a simple climate-carbon-cycle model.
10-1887	10	60	18	60	18	Should "very likely" be reduced to "likely"? This appears to be based on a number of 90% intervals. Most ranges are narrower than the synthesized range, but at least one has a higher upper bound. Given that structural uncertainty (e.g, associated with the use of simplified models) is difficult to account for, it might be reasonable to assess likely rather than very likely. [Francis Zwiers, Canada]	Taken into account. A likely range is now given.
10-1888	10	60	18			Very likely should be in italics. [Reto Knutti, Switzerland]	Accepted. Typographical change.
10-1889	10	60	20	62	29	Section 10.9 does a good job of synthesizing the various climate systems presented in the chapter giving an overall picture of the system as a whole. This section does the job of synthesis by presenting the analysis, providing a reasonable amount of uncertainty quantifying given the available data, and finally investigates gaps in the analysis. This section presents the results necessary for the reader to generate ones own conclusions. [Government of United States of America]	Positive comment on section, noted
10-1890	10	60	22	60	27	This sentence makes no sense. [John McLean, Australia]	Accept - see response to comment 1891

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10-1891	10	60	22	60	27	Sentence beginning "The evidence accumulated from widespread anthropogenic changes detected in aspects…" is lacking gramatically. You mean to say that evidence accumulated from widespread changes in climate allow detection and attribution of an anthropogenic influence? Also, the last word in the sentence should be millenium, not millennia. [James Renwick, New Zealand]	Accept - this sentence was deconstructed into a number of small sentences.
10-1892	10	60	23	60	23	includes [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accept - see comment 1891
10-1893	10	60	26	60	27	climate variations during the last few millennia [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accept - see comment 1891
10-1894	10	60	27	60	28	McLean et al (2009) showed that the ENSO was the likely primary driver of global average temperature since 1960 (at least) and that little temperature variation remained unaccounted for. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) Further, the observations of many climate factors is consistent with the post-1976 dominance of ENSO conditions on the El Nino side of absolutely neutral (i.e. SOI=zero) (refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures"). On this basis your statement is false. I suggest that if the ENSO was properly modellled the conclusions that you have drawn might be very different. As things stand your comments are based on papers contructed on the output of climate models that do not accurately incorporate all natural climate forces. Comptetent reviewers would have demanded that those papers eithe rbe rejected or accepted only if they explicitly stated their limitations. Or was the latter the case and it is you who has failed to make explicit the limitations of climate models? [John McLean, Australia]	Taken into account - new box on temperature haitus for last 15 years added
10-1895	10	60	28	60	28	"past several decades" -> it would be stronger to provide a date here, egg, "since 1950". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - since 1950 has been worked into the last sentence.
10-1896	10	60	33			Section 10.9.1. Suggest to incorporate the potential for improved signal-to-noise ratios and detection times as a result of using multi-variable fingerprints into Section 10.2. [Oliver David Andrews, United Kingdom]	Rejected. This discussion belongs naturally in this section.
10-1897	10	60	33			Section 10.9: Regarding the method, maybe other challenges could be mentioned. In optimal fingerprinting, the choice of the truncation is usually a weak step of the method, as it is usually at least partly arbitrary. The ROF method may help in this respect, but other regularisation techniques may be more efficient and the impact of using the ROF method has not been assessed on many variables. Then one may wish to make the pre-processing step more objective (in particular in order to perform multi-variable analysis). Investigation on methods that do not assume each spatio-temporal pattern to be known a priori could also have some interest, as mentioned e.g. in Jones et al., 2012. Of course, this list is non-exhaustive, and other methodological improvements could be of interest (e.g. approaches avoiding the additivity assumption, etc). [Aurélien Ribes, France]	Accepted -
10-1898	10	60	33			The ordering here seems odd, and it would be more logical to have the "remaining challenges" section coming last, i.e., swapping 10.9.1 and 10.9.2. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account
10-1899	10	60	35	60	44	It feels a little remiss not to reference back anywhere here to the discussion way back in the SAR in their section 8.1.2.4 where such multivariate formal analyses were first (to my incomplete knowledge) discussed. To not do so potentially paints an incomplete picture of the heritage of this nascient avenue of investigation. [Peter Thorne, United States of America]	Takne into account
10-1900	10	60	35	60	56	I think it would be worth pointing out a bit more strongly that, while there have been a few examples of successful multi-variable analyses, it is not clear yet whether this is generally desirable or feasible approach. A general problem is that as multiple variables are added, dimension reduction has to become much more severe in the conventional approach. If the amount of information available for estimating internal variability does not increase with increasing numbers of variables, then the ability to separate signals on the basis of space-time patterns of change, for example, might be lost. Alternatively, we we could use full-rank covariance matrix estimators (such as that proposed by Ribes, which consists of a weighted average between the sample covariance matrix and the identity matrix). If the overall dimension of the problem increases, but the rank of	Taken into account

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						the sample covariance matrix does not, then I suspect that the weight given to the sample covariance matrix will decrease, which effectively reduces the amount of optimization in the analysis. [Francis Zwiers, Canada]	
10-1901	10	60	39	60	43	The three climate factors mentioned in the first sentence are all related because they all correlate with variations in the ENSO. The hydrology of the western USA (in sentence two) is closely linked to the ENSO (refer Loik, M.E. et el (2004) - "A Multi-scale perspective of water pulses in dyland ecosystems: climatology and ecohyrdology of the western USA" and Jin, J et al (2006) - "Relationship between atmospheric circulation and snowpack in the Western USA") and I am puzzled as to why you omit papers based on empirical observation but cite papers based on the output of incomplete models. [John McLean, Australia]	Taken into account - see new hiatis box 10.2
10-1902	10	60	40	60	40	A degree of hedging / caveating may be required here given the low confidence assigned to DTR changes in Chapter 2. [Peter Thorne, United States of America]	Taken into account
10-1903	10	60	47	60	47	Remove brackets around Scott and Jones (2009). [Government of Canada]	Editorial
10-1904	10	60	47	60	47	Wrong brackets (Stott and Jones, 2009) [Roman Zweifel, Switzerland]	editorial
10-1905	10	60	49	60	49	Insert "ratio" after "noise" [Government of United States of America]	Taken into account
10-1906	10	60	50	60	52	As written this is confusing. How can low correlation variates multivariate regression depend upon how well the model gets the covariance? The very reason for the power is their lack of covariance after all? If the co-variate behaviour reality is important then they must be correlated so on the face of it this sentence as written is self-contradictory. [Peter Thorne, United States of America]	Taken into account
10-1907	10	60	65			good thing to end on. Can you cut down on the justification between FAQs? [tim barnett, United States of America]	Not clear what comment is saying given it has been truncated.
10-1908	10	61	1			Because much of the details in Section 10.9.2 are repetitive of earlier statements for the individual quantities, much care is needed here to ensure all wording is consistent with the earlier statements, and any post-SOD revisions will need to be carefully carried forward into this section. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Care has been taken in revision to ensure consistency here.
10-1909	10	61	2	61	2	Could refer in this section to FAQ 2.1, which provides an integrated view from the perspective of observations. [Albert Klein Tank, Netherlands]	Accepted.
10-1910	10	61	2			You need to explain why you are trying to do this. CO2 concentrations are clearly rising. So bringing in temperature changes doesn't affect your conclusion of an anthropogenic influence on the atmosphere, because it was so clear anyway. Globally integrated vorticity is not changing, I would guess, so would inclusion of that reduce your confidence? [Dáithí Stone, United States of America]	Taken into account. We make clear that is is changes in the climate system we are interested in that are documented in chapter 2 (in revision we include a reference to FAQ 2.1), in terms of temperatures, the cryopshere etc. not just increasing co2 levels.
10-1911	10	61	5	61	5	Change "all of the common" to "multiple" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This phrase has been deleted.
10-1912	10	61	13	61	14	The coherence with ENSO variation can very likely account for these observations, even if the ENSO is poorly handled in climate models. [John McLean, Australia]	Rejected. The assessment in the chapter assesses the extent to which internal variability can explain the observed changes.
10-1913	10	61	13	61	19	This section may benefit from cross-referencing FAQ 2.1? [Peter Thorne, United States of America]	Accepted.
10-1914	10	61	18	61	18	Change "and in the case of" to "but for" [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted.
10-1915	10	61	18	61	18	Sea ice emerges from what? I think you mean that sea-ice changes can only be explained by human and natural forcings? If so, say so. [Peter Thorne, United States of America]	Accepted.
10-1916	10	61	18	61	18	For Antarctica see very recent paper in Nature Geoscience by colleagues from BAS which implies a wind forcing component to sea-ice changes. Arguably by a two-step attribution process this is also anthropogenic in origin given your confidence in Antarctic circulation changes being drive by ODSs. [Peter Thorne, United States of America]	Noted. This section summarises the evidence presented in section 10.5.
10-1917	10	61	18	61	19	Again, this depends on the way the magnitude of internal variability is estimated. [Hugues Goosse, Belgium]	Noted. This summarises the assessment in section 10.5.

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10-1918	10	61	22	61	32	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the graphs and associated statements should be removed or seriously qualified. [John McLean, Australia]	Rejected. The chapter assesses the ability of models to be adequate for detection and attribution for each of the variables considered.
10-1919	10	61	34	61	36	How useful is Table 10.1 without reference to the papers. It just refers back to sections here. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. The table provides the link between the ES and the sections of the report by summarising the evidence.
10-1920	10	61	38	61	49	Much of what is described here is consistent with the dominance of the El Nino side of the Southern Oscillation Index since 1976 (refere Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures"). Modify your sentence accordingly. [John McLean, Australia]	Rejected. The chapter assesses internal variability.
10-1921	10	61	40	61	40	I think this could be nuanced a bit better. Saying that the signals exceed internal variability is not completely clear (a time and space scale needs to be specified, etc), and implicitly associating the assessments with confidence intervals of width that corresponds to the assessed likelihoods ignores the contribution from expert judgement that would downweight likelihood assessments when the purely statistical evidence would suggest something higher. I would replace the bit that reads "that exceeds internal variability .... across a range of likelihoods ranges from likely to extremely likely" with "such that the assessed likelihood of a detectable, and often quantifiable, human contribution ranges from likely to extremely likely." [Francis Zwiers, Canada]	Accepted.
10-1922	10	61	41	61	41	Although the old AR4 term "extremely likely" is included as an acceptable term in the new uncertainty guidance document it would be preferable if one of the 7 primary likelihood terms could be used. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. For attribution there is a big step from very likely to virtually certain and we need to use it in a small number of cases.
10-1923	10	61	42	61	43	the parenthesis "(i.e., volcanic eruptions and solar)" should be shifted to come after "natural forcings". [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. This bit was deleted in revision.
10-1924	10	61	47	61	49	The final sentence of this paragraph, duplicates what is already stated on lines 38 - 41 (ranging from very likely to extremely likely etc), and could be removed. Note also, that it would be preferable not to use the old AR4 term "extremely likely" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Text has been consierably tidied up. However note we continue to use the term "extremely likely" for reason given to 1922.
10-1925	10	61	47	61	49	This sentence could be more carefully written. It mixes likelihood with confidence (again … see previous comments about the inappropriateness of using the term confidence level when significance level is meant), and it mixes assessments of bodies of literature with outcomes of individual studies. [Francis Zwiers, Canada]	Accepted. Sentence has been deleted.
10-1926	10	61	51	61	52	"by virtue of the Clausius-Clapeyron". But there is also a missing comma, and it is not possible to say whether it should be placed after "warming" or after "7% K-1", or indeed whether the necessary correction is to replace ", and at local scales as a consequence of warming at 7% K-1" by "at 7% K-1 as a consequence of warming,". [J. Graham Cogley, Canada]	Taken into account. Sentence revised.
10-1927	10	61	51	61	52	I'm not sure that this is true everywhere - would saturation vapour pressure may increase at the rate of 7%/K in the lower troposphere, but does that mean that column integrated water vapour will increase at that rate everywhere? It appears to hold in the global mean in models (e.g., Held and Soden, 2006), but does it locally, for example in the downwelling branches of the Hadley circulation? [Francis Zwiers, Canada]	Taken into account. The 7% bit has been removed as un necessary detail at this ppint.
10-1928	10	61	51	61	53	"Water in the free atmosphere is expected to increase, and at local scales is expected to increase as a consequence of warming at 7% K–1 by the Clausius-Claperyon equation, while atmospheric circulation controls the global distribution of precipitation and evaporation." [James Renwick, New Zealand]	Taken into account. Sentence revised.
10-1929	10	61	54	61	54	typo: "of" [Albert Klein Tank, Netherlands]	Editorial.
10-1930	10	61	57	61	57	The general statement on the wet getting wetter, dry getting drier seems overly simplistic and broad. For example, the GRL study by Sun et al. (2012) suggests that over land there has been a spatial redistribution in precipitation over land such on average the dry became wetter and the wet drier. This should be considered in the assessment. [Government of United States of America]	Taken into account. Sentence revised to remove this formulation.
10-1931	10	62	6	62	29	This latter part of the final section of the chapter looks hastily written. Check grammar carefully. [James	Agreed. Section has been re written.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Renwick, New Zealand]	
10-1932	10	62	8	62	10	Change ", Greenland … warming." to "The". [J. Graham Cogley, Canada]	Editorial.
10-1933	10	62	11	62	12	This is not accurate. Say "The available information, and our level of scientific understanding, are too low to provide an attributable explanation … Antarctica". [J. Graham Cogley, Canada]	Rejected. The evidence for the assessment is provided in the chapter.
10-1934	10	62	13	62	13	"exceed". [J. Graham Cogley, Canada]	Editorial.
10-1935	10	62	13	62	14	Because the internal variability of the models is not very realistic for sea ice in the Southern Ocean, I would state that the confidence is low for this point. [Hugues Goosse, Belgium]	Accepted. Confidence changed to low.
10-1936	10	62	14	62	14	"envelope of internal". [J. Graham Cogley, Canada]	Editorial.
10-1937	10	62	15	62	15	"Warming is likely …". [J. Graham Cogley, Canada]	Editorial.
10-1938	10	62	18	62	19	Observational uncertainties are certainly a factor, but it could also be that there is no trend! [Dáithí Stone, United States of America]	Rejected. There is warming seen in Fig 10.7 but observational uncertainties are large.
10-1939	10	62	19	62	20	"Antarctic". "On millennial time scales, temperature changes can be attributed to anthropogenic forcing and volcanic eruptions in Europe in some seasons …". But this, particularly as far as anthropogenic forcing is concerned, seems inconsistent with section 10.7.5. One or more of "millennial", "in Europe" and "[result] 29" must be wrong. [J. Graham Cogley, Canada]	Taken into account. Text revised to be consistent.
10-1940	10	62	20	62	20	"29". What does this mean? [Christian-D. Schoenwiese, Germany]	Taken into account. It refers to the row of the table. Text revised to
10-1941	10	62	20	62	20	Isn't this statement also true for NH reconstructions? [Francis Zwiers, Canada]	Taken into account. This sentence has been deleted.
10-1942	10	62	20	62	22	Result numbers should be 30 and 31. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text corrected.
10-1943	10	62	22	62	22	"probability of SOME OBSERVED heat waves has risen" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Text corrected.
10-1944	10	62	25	62	29	Change "come" to "relate", and all the "from"s to "to". At L28, change "is around" to "relates to". [J. Graham Cogley, Canada]	Editorial.
10-1945	10	62	27	62	27	Ocean acidification is not discussed in Section 10.4. Although the topic is covered in Section 3.8.2 I would recommend a short summary in 10.4 to support the inclusion of surface ocean acidification in Table 10.1 and its prominence in 10.9.2. For example, with reference to recent work on detecting surface pH changes by Friedrich et al. (2012) and Bates et al. (2012). [Oliver David Andrews, United Kingdom]	Accepted. Ocean acidification is discussed in the revised chapter.
10-1946	10	62	27	62	27	Ocean acidification is not discussed in Section 10.4. Although the topic is covered in Section 3.8.2 I would recommend a short summary in 10.4 to support the inclusion of surface ocean acidification in Table 10.1 and its prominence in 10.9.2. For example, with reference to recent work on detecting surface pH changes by Friedrich et al. (2012) and Bates et al. (2012). [European Union]	Accepted. Ocean acidification is discussed in the revised chapter.
10-1947	10	62	27			I don't remember anything about ocean acidification in 10.4. [Dáithí Stone, United States of America]	Accepted. Ocean acidification is discussed in the revised chapter.
10-1948	10	62	31	62	31	We suggest to explain the flattening of the global mean temperature trends in the last 10-15 years seprately in a FAQ to help interpreting figure 9.8. There is intense media attention given to this behavior that justifies additional attention to what has been written in section 10.3.1.1.3. A good response to this discussion has been given by the Met Office recently (October 14, 2012). See the link http://metofficenews.wordpress.com/2012/10/14/met-office-in-the-media-14-october-2012/ . [Government of Netherlands]	FAQ topics were determined prior to the first draft of AR5 in 2011. We cannot add a new FAQ to AR5 at this stage in the writing process. However there is a new box 9.2 which discusses the last 15 years.
10-1949	10	62	31	63	49	FAQ 10.1 Perhaps the temporal scale of "Climate" should be define here. People often misuse the term Climate, especially when talking about climate change. People will remember a heavy snowfall event 2 years in a row and automatically say that's the new norm and that contributes to the thought that the "Climate is Always Changing." when in reality an individuals perception of climate is incorrect. [Government of United	Accepted - We already specify "20th Century" climate change in the chapeau. A parenthetical clarification has been added to specify "long term" time scales "longer than a decade"

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						States of America]	
10-1950	10	62	31	63	49	I quite like this explanation and think it could nicely cover for almost all of 10.2, considering the audience. [Dáithí Stone, United States of America]	Taken into account, thanks.
10-1951	10	62	33	63	34	Overall I found this FAQ to be largely talking to an audience that was more technically minded than I believe the TSU guidance implied. I would personally try to make this somewhat simpler and easier to follow. A good analogy might be to a detective sleuth here, where the detective gathers all the possible evidence (forcings) then attempts to discern the appropriate suspect who commited the 'crime'. [Peter Thorne, United States of America]	Taken into account -- we understand that we are trying to convey a rather technical procedure to a lay audience. This FAQ text has been revised previously with input from a science writer with experience writing for our intended audience.
10-1952	10	62	33			FAQ 10.1: Given that we are trying to avoid the use of calibrated uncertainty language in the FAQs, we wonder if an alternative wording can be used in the title to avoid "...the most LIKELY causes of...." [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - title changed. We also changed the wording of the first sentence in the chapeau to remove the word "likely"
10-1953	10	62	33			FAQ 10.1: Current title includes IPCC Uncertainty Language. To reduce confusion and to clarify the title, we therefore suggest that the title be changed to "Climate is always changing. How do we determine the causes of observed changes?" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - title changed
10-1954	10	62	36	62	36	Change "identified" to "described", " 'fingerprints' ". [J. Graham Cogley, Canada]	Taken into account - We have incorporated the latter correction but kept "identified" in preference to "described" here. The attribution exercise is in fact intended to provide identification of the most likely sources of observed change.
10-1955	10	62	36	62	36	Change "predictable 'fingerprint'" to "identifiable 'fingerprints'" ? [James Renwick, New Zealand]	Taken into account - We changed "predictable" to "expected" (cf comment 1958)
10-1956	10	62	36	62	36	Grammar: "Fingerprint" should be replaced by "fingerprints". [David Wratt, New Zealand]	Accepted - typo fixed
10-1957	10	62	36	62	37	I think the notion of predictability might confuse the lay reader, who would be thinking of prediction in a different way than meant here. I suggest rephrasing this sentence as follows: "The most likely causes of observed climate change are identified by determining whether the expected 'fingerprints' of the different possible agents of climate change are present in the historical climate record." [Francis Zwiers, Canada]	Accepted - this sentence is reworded to avoid use of the term "predictable"
10-1958	10	62	36	62	39	But climate models do not accurately incorporate all natural climate forces and that means that the output from climate models is unreliable. [John McLean, Australia]	Rejected - the attribution simulations described in this FAQ do incorporate the natural forcings (principally solar insolation and volcano-related aerosol fluctuations) that are important for changing climate on the multidecadal time scale of interest here.
10-1959	10	62	36	62	39	Detection of "fingerprints" by modelling is limited to climate forces that are accurately modelled. The ENSO is poorly modelled (refer Jin,E.K. et al (2008) "Current status of ENSO prediction skill in coupled ocean–atmosphere models", Climate Dynamics; Nov2008, Vol. 31 Issue 6, and Power, S. et al (2006) - "The Predictability of Interdecadal Changes in ENSO Activity and ENSO Teleconnections" and Power, S and Colman, (2006)) but its characteristics are well understood and described in several papers (e.g. Trenberth and Stepaniak). Those characteristics are consistent with many? most? of observations that form the basis of claims of anthropogenic forces. Excluding the ENSO because it cannot accurately be modelled lacks integrity, as does the repeated failure to mention the consistency between observations and known ENSO characteristics. [John McLean, Australia]	Rejected - Attribution of observed multi-decadal climate change is different than annual-scale ENSO prediction.
10-1960	10	62	36	62	41	It should be mentioned that forcing factors that show no trend or whose trend is opposite to that which would lead to the observed change over a certain period can be excluded as a cause for the change over that period. [Urs Neu, Switzerland]	Taken into account - this point is clarified in the subsequent text of the FAQ. We incorporate this point later in order to keep the chapeau short and concise.
10-1961	10	62	36	62	47	This comment astounds me. The underlying assumption is that climate models represent natural variability perfectly. By examining other parts of this and Chapter 9, this is obviously not true. Models, with little abiltiy to represent multi-annual and multi-decadal natural variability, are therefore inadequate to determine whether a	Taken into account - we added text to indicate that climate models do not represent variability perfectly. However Ch 9 supports the contention that current

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						fingerprint is natural or anthropogenic. Do the writers really believe that we understand everything about natural variability and can reproduce it accurately? in my opinion (based on the evidence) the answer is no (just look at the comparisons since 1979.) [John Christy, United States of America]	models are sufficiently reliable to carry out attribution assessments.
10-1962	10	62	36			FAQ 10.1: It would be very important early in this FAQ to introduce what is meant by 'observed climate' in the context of Chapter 10. The time-frame used in Chapter 10 to established observed changes in climate should be clearly introduced upfront. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - see response to comment 10-1950 above.
10-1963	10	62	38	62	38	Amend "climate forcings" by "individual climate forcings" [Urs Neu, Switzerland]	Accepted
10-1964	10	62	43	62	47	This claim is false because not all plausible causes of climate variation can be accurately modelled. It is dishonest to exclude something just because you can't model it. [John McLean, Australia]	See response to comment 10-1959
10-1965	10	62	44	62	44	Suggest replacing "20th century climate change" with "climate change since the beginning of the 20th century". It is now the 21st century, and many D&A studies consider surface temperature since the beginning of the 20th century. [Francis Zwiers, Canada]	Accepted - the suggested wording has been incorporated here.
10-1966	10	62	46	62	46	The expression "extremely likely" should be avoided in AR5. It is part of the agreed uncertainty language outlines in the AR5 Guidance Notes on Uncertainty, but only mentioned in a footnote. The more uncertainty expressions are used in AR5 the more diluted the messages become and we encourage the authors to stick to the 7 main agreed expressions for AR5, especially in regard to this very important statement. In addition, it is confusing for the reader to find likelihood terms that not are included in Chapter 1, please introduce all terms used in AR5 in Chapter 1." [Government of Germany]	Taken into account - this sentence is being revised as needed to be consistent with the attribution statement in the main text.
10-1967	10	62	46	62	46	changing "most of (at least 50%)" to "more than half of" [Zong-Ci Zhao, China]	Taken into account - this sentence is being revised as needed to be consistent with the attribution statement in the main text.
10-1968	10	62	49	62	54	All these internal forcing factors could be influenced by anthropgenic and natural external forcing. This should be mentioned. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - we added a sentence in the second paragraph of the main text of this FAQ
10-1969	10	62	50	62	50	Suggest replacing "detect" with "ascertain". Detect would be slightly confusing because this sentence is about determining whether a detected change (in a broad sense) is likely to have been forced. [Francis Zwiers, Canada]	Accepted - the suggested wording has been incorporated here.
10-1970	10	62	51	62	54	It is a fallacy to regard the ENSO as "internal variability" because it involves ocean warmth, which is primarily drive by the sun, an external force, and the circulation that the ENSO state induces, be it Hadley Circulation or Walker Circulation, influences cloud cover which in turn influences the amount of solar irradiance (another external force!) striking the Earth's surface. [John McLean, Australia]	ENSO dynamics should (and to a large extent are) described by global climate models, which simulate ENSO variability as an internal process that exists without external forcing. So it is appropriate to regard ENSO as internal variability.
10-1971	10	62	52	62	52	We feel that land and cryosphere need to be included as well in internal variability of the climate system. [Government of United States of America]	Accepted - we changed "atmosphere and ocean" to "climate system"
10-1972	10	62	53	62	53	Suggest replacing "cycle" with "phenomenon" (I think the notion that there could be cycles in the climate system is generally misunderstood, and thus I think it would be best to avoid that word). [Francis Zwiers, Canada]	Taken into account - wording is amended and the term "cycle" is no longer used here
10-1973	10	62	56	62	56	It might be prudent to omit "to the climate system", because the Glossary defines the climate system as including the lithosphere (and therefore volcanoes). The authority for this definition is the Framework Convention, but admittedly nobody ever thinks, as do the Glossary and the Convention, that volcanoes are internal to the climate system. [J. Graham Cogley, Canada]	Rejected - The AR5 Glossary (FOD version) entry for "climate system" explicitly includes volcanic eruptions as an forcing external to the climate system. So the wording in the FAQ here seems both appropriate and consistent.
10-1974	10	62	57	62	57	"are responsible" needs to be qualified with IPCC-calibrated language. [J. Graham Cogley, Canada]	Rejected - The reference here is to paleoclimatic change, not current climate change. Calibrated assessment of the individual natural causes of paleoclimate change is outside the scope of AR5 attribution analysis.

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10-1975	10	63	1	63	1	Change "present" to "documented". Delete "Possible". [J. Graham Cogley, Canada]	Accepted - wording changed.
10-1976	10	63	5	63	5	append a sentence "It should be noted that these climate models do not accurately incorporate all climate forces and their output should be treated with caution." [John McLean, Australia]	Rejected - we are unaware of external forcings important for decade-century scale climate change that could plausibly compete with the forcings used in the simulations described here.
10-1977	10	63	5	63	7	This bit is potentially confusing because it put the emphasis on climate models rather than the observations. I suggest restructuring this so that the subject of the paragraph is the fingerprints. Thus the first sentence could be something like "The 'fingerprints', or patterns of change that are expected to be the result of individual climate forcings, are obtained by ...." [Francis Zwiers, Canada]	Taken into account - this paragraph is revised to make fingerprint patterns the principal subject of the paragraph.
10-1978	10	63	9	63	16	These statements are only valid if the models used were 100% accurate for all natural forces. If this is not the case then the graphs and associated statements should be removed or comprehensively qualified. [John McLean, Australia]	Rejected - see response to comment 10-1976
10-1979	10	63	18			If you are evaluating the models to be “correct” by being able to do this then you cannot then apply them to an attribution study. [Dáithí Stone, United States of America]	Accepted - wording here is revised to clarify the distinction between model validation and attribution assessment.
10-1980	10	63	19	63	19	Pinatubo was in 1991, not 1992. [J. Graham Cogley, Canada]	Accepted - typo fixed
10-1981	10	63	26	63	26	Is this correct? The inset doesn't doesn't really illustrate the long term (despite what is said on line 25), and in any case, the figure doesn't tell us the relative importance of cooling due to ozone depletion (which is most important in the southern polar atmosphere) and increases in well mixed ghgs (which also cause stratospheric cooling). [Francis Zwiers, Canada]	Taken into account - see response to comment 10-1984
10-1982	10	63	28	63	29	This statement has no credibility unless it can be shown that climate models accurately encompass all natural forces, and I see no evidence of that anywhere. [John McLean, Australia]	Rejected - see response to comment 10-1976
10-1983	10	63	36	63	47	What are the observations used in FAQ 10.1 Fig 1? This plot could include obs uncertainty if it is HadCRUT4. What does the grid box outlining mean in the RH panels? Only seems to occur in the upper and lower ones. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted - The observations in the middle map panel are from HadCRUT4, and this has been added to the caption. There are multiple estimates of observations in the time series. Observational uncertainty is discussed in the context of these results in section 10.3 in the main part of the chapter. Grid box outlining is removed for simplicity and clarity.
10-1984	10	63	37			FAQ 10.1, Figure 1: We would suggest removing the small inset panels showing stratospheric temperature. For an FAQ audience this added complexity may not be useful, and for the inset panels to meet the necessary minimum font requirements for publication, the size of these inset panels would need to be significantly increased. Captions needs expansion - What are the red lines on the time series? What does the hatching indicate on the maps? Please also add A, B, C, D, E to the 5 panels to help navigation through the figure and caption. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - Figure 1 is revised and simplified in accordance with this suggestion.
10-1985	10	63	51	63	51	I think the response to this FAQ is very nicely crafted. Chapter 11 also discusses the concept of emergence, so I hope that this response has been coordinated with them. [Francis Zwiers, Canada]	Accepted - we have coordinated the text and figure with authors in Ch 11
10-1986	10	63	53	63	53	"become obvious". Make the same change at P64 L47. [J. Graham Cogley, Canada]	Wording changed on line 47.
10-1987	10	63	53			FAQ 10.2: Care should be taken to avoid any misinterpretation that human influences are expected to emerge at some precise date in the future. The phrase "The precise date of future emergence of projected warming trends...." might imply that such a date is to be expected, and such wording should be revised. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - wording of this sentence is amended to remove potentially confusing implications of precise dates of emergence
10-1988	10	63	53			FAQ 10.2: We would suggest that trendy expressions such as "loading the weather dice" that have resulted from the publications and media based statements from individual scientists are best avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - this colloquial phrase has been removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-1989	10	63	55	63	55	What is general scale of "Local Scales"? Square km? Please explain. [Government of United States of America]	Accepted - "local scales" here means the same general horizontal scale as a typical climate model grid cell, or roughly on the order of (100 km)^2. We have not changed the wording in the chapeau, rather we have clarified our intended meaning of "local" in the main body of the FAQ text.
10-1990	10	63	55	63	56	According to the "Mean Global Surface Temperature Anomaly: the globe has not been warming at all for the past ten years, so this is nonsense [Vincent Gray, New Zealand]	Rejected -- the statement in the FAQ refers to long-term but spatially local variability.
10-1991	10	63	55	63	56	A vague and speculative statement that fails to provide any indication of time period. [John McLean, Australia]	Rejected -- the statement is supported by the subsequent text.
10-1992	10	63	55	63	56	I recommend changing "obvious" to "locally obvious" to emphasise the point of this FAQ. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - wording changed.
10-1993	10	63	55	63	56	The warming has already become obvious in tropical regions not only over land, but also over oceans, especially over the tropical Indian Ocean and western Pacific (Kattsov and Sporyshev, 2006). [Petr Sporyshev, Russian Federation]	Taken into account. We amended the wording in the third paragraph of the FAQ to clarify that detected warming is not restricted to continental temperature.
10-1994	10	63	55	63	56	Is there a cool part of the year in tropical regions? [Francis Zwiers, Canada]	There can be small differences of the temperatures in the season --perhaps a few degrees. But of course it never is cold relative to high-latitude winter conditions.
10-1995	10	63	55	64	3	The comments in this paragraph can be disputed by simple analysis. "obvious on land in tropical regions". This is not borne out in the evidence. In Christy et al. 2009 I built the most comprehensive climate dataset of East African temperatures (which I expanded to incude Uganda this year). The result indicates no change in TMax temperatures over the period of record (starting in 1900). Some trends are indeed attributable to humans, such as TMin increases, - due to surface development (Christy et al. 2006, 2009, McNider et al. 2012), not to GHG increases. How can the Arctic ice loss be attributable to humans when the Antarctic gain cannot? (See testimony of CMIP5 model comparisons for both sea ice areas in my congressional testimony, 20 Sept 2012 House subcommittee on Energy and Power.) [John Christy, United States of America]	Taken into account -- we cannot assess every region explicitly in the FAQ and so we have made it clearer that here we are not confirming Detection and Attribution at every location on the planet. Attribution of Arctic sea ice decline is discussed in detail in Section 10.5.
10-1996	10	63	55	64	3	These are unsustainable claims because climate models do not accurately include all natural forces. There's good reason to consider that what you blame on human activity is in fact due to the ENSO. [John McLean, Australia]	Rejected - See response to comment 10-1970 regarding ENSO as internal variability. Global climate models do reproduce ENSO variability with sufficient fidelity to carry out attribution experiments on multidecadal climate change.
10-1997	10	64	2	64	2	Change "detectable" to "detected". [J. Graham Cogley, Canada]	Accepted - wording changed.
10-1998	10	64	3	64	31	The expression "loading the weather dice" does not work for non-native speakers. Please try to find a better expression. [Government of Germany]	Accepted - this colloquial phrase has been removed.
10-1999	10	64	5	64	5	"caused by global change" This is very unclear. Do you mean "manmade climate change"? If so, say so. [John McLean, Australia]	Accepted - Wording changed.
10-2000	10	64	5	64	6	Better would be: "Warming trends caused by global change are generally more obvious in the global temperature average than locally". Then the next sentence follows naturally. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - Wording changed.
10-2001	10	64	5	64	11	We recommend that this paragraph may be better placed at the beginning of FAQ 10.2. [Government of United States of America]	Rejected - This paragraph is the beginning of the main text of FAQ 10.2 and seems appropriate here. The preceding italicized chapeau text is designed to concisely and explicitly answer the question posed in the FAQ title.
10-2002	10	64	6	64	6	"the local variability of climate". [J. Graham Cogley, Canada]	Accepted - wording changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10-2003	10	64	6	64	7	Amend "most of the variability of local climate is averaged away in the global mean" by "most of the variability of local climate is due to the internal distribution of energy within the climate system and is averaged away in the global mean..." [Urs Neu, Switzerland]	Taken into account - wording is changed here but we choose to avoid the technical phrase "internal distribution of energy"
10-2004	10	64	7	64	7	Suggest replacing "a long-term trend" with "the long-term trend", and replacing "widespread changes in greenhouse gases" with "greenhouse gas increases". I don't think it is necessary to explain the "widespread" nature of well-mixed ghg changes. [Francis Zwiers, Canada]	Accepted - wording changed.
10-2005	10	64	7	64	8	The global temperature trend includes periods of temperature variation that previous IPCC reports have attributed to ENSO events, usually El Ninos. (For some reason there seems to be a reluctance to admit that La Nina conditions cause cooling.) Take these out of the record and the trend changes. [John McLean, Australia]	Rejected - long term trends in temperature are detected across time scales longer than ENSO events.
10-2006	10	64	7	64	8	McLean et al (2009) showed that the ENSO was a very good indicator of global mean temperature seven months into the future and was the likely primary driver of global average temperature since 1960 (at least). (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) The relationship to the ENSO left very little temperature variation to be blamed on other forces. Your statement is therefore refuted by empirical evidence and should be removed. [John McLean, Australia]	Rejected - The climate change we discuss here occurs on time scales much longer than an individual ENSO extremum (i.e. > 7 months).
10-2007	10	64	8	64	9	This statement is nonsense (a) because you falsely regard ENSO as "internal variability" and (b) climate models do not accurately simulate ENSO evolution. The ENSO modifies cloud cover, which means a change in solar irradiance and that's not an internal force. [John McLean, Australia]	Rejected - See response to comment 10-1971 regarding ENSO as internal variability.
10-2008	10	64	10	64	10	It is not "noise"; it is the net effect of a large number of forces that vary in strength and in duration. With better understanding of these forces we might one day able to explain every variation. [John McLean, Australia]	Rejected - It is not necessary to explain individual annual-scale variations to assess long term climate change.
10-2009	10	64	10	64	10	Again change "obvious" to "locally obvious". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - we added "local" in front of "trend" on the previous line.
10-2010	10	64	13	64	13	FAQ 10.2, Figure 1 is based on CMIP5 climate model simulations. So it is not an observational result. [Petr Sporyshev, Russian Federation]	Taken into account, although there is no error or attempt to mislead in the text here. The caption to Fig. 1 explains clearly that the figure is based on CMIP5 simulations.
10-2011	10	64	13	64	15	The tropical Indian Ocean and western Pacific are characterized by the earliest warming (Kattsov and Sporyshev, 2006, Figure 2), because of high warming and low variability in the region (Mahlstein et al., 2011, Figure 1). The pattern does not significantly depend on which annual or summer means are used. There is also qualitative agreement between the model and observationally derived pictures. Kattsov, V. M., and P. V. Sporyshev (2006), Timing of global warming in IPCC AR4 AOGCM simulations, Geophys. Res. Lett., 33, L23707, doi:10.1029/2006GL027476. [Petr Sporyshev, Russian Federation]	Taken into account - The FAQ text here is wholly consistent with the comment. The FAQ provides just a few examples of local trend emergence, and does not attempt to provide a comprehensive survey of all locations where trends are observed to emerge.
10-2012	10	64	14	64	15	This attempts to exclude the powerful impact of the ENSO on tropical climate and ignores the dominance of conditions on the El Nino side of SOI=zero since 1976 (refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures"). [John McLean, Australia]	Rejected - see response to comment 10-958
10-2013	10	64	18	64	19	Have you audited the HadCRUT3 data? There are in fact several problems with the data for Eurasia (as you term Eastern Russia) that you quote because if you had you would know there are several problems with it. The number of stations supplying data for that area reduces after 1989; the data usually disagrees with data from Russian meteorological agencies (by just over 10C in some cases and typically by at least 0.2C which is far beyond what rounding might account for). It is very strange that in most locations the February average temperature anomaly is positive - in the order of +6 is not uncommon - in the great majority of years since 1990 whereas January and March anomalies show a range of positive and negative values. Until these isues with the data are resolved your statement lacks credibility. [John McLean, Australia]	Rejected - the statement, figure and results are all based on peer reviewed published results.

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10-2014	10	64	21	64	22	This statements would be valid if and only if the models used were 100% accurate for all natural forces. If this is not the case then the statement should be removed or the uncertainties clearly expresed (e.g. within the limits of incomplete and therefore inaccurate modelling). [John McLean, Australia]	Taken into account - see response to comment 10-1961
10-2015	10	64	21	64	23	This statements would be valid if and only if the models used were 100% accurate for all natural forces. If this is not the case then the statement should be removed. [John McLean, Australia]	See response to preceding comment.
10-2016	10	64	22	64	22	Consider mentioning the Antarctic too because you do mention the Arctic and the reader will expect information for the other pole. [Albert Klein Tank, Netherlands]	Rejected - This is a reasonable suggestion but we are tightly length-constrained and the text specifically refers to the Arctic as an example.
10-2017	10	64	24	64	27	These observations are consistent with the dominance of cdonditions on the El Nino side of the SOI since 1976 and this should be stated. [John McLean, Australia]	Rejected - see response to comment 10-958
10-2018	10	64	30	64	34	Or these events may be related to the dominance of conditions on the El Nino side of SOI=zero since 1976. If this is correct then the trends derived from climate models will not continue but will be related to future ENSO conditions. [John McLean, Australia]	Rejected - Changes in future ENSO conditions are internal to the couple model projection of future climate, and may themselves be part of the forced climate change response.
10-2019	10	64	32	64	32	Suggest replacing "may have contributed" with "are estimated to have contributed" [Francis Zwiers, Canada]	Accepted - wording changed.
10-2020	10	64	38	64	38	McLean et al (2009) showed that the ENSO was the likely primary driver of global average temperature since 1960 (at least) and that little temperature variation remained unaccounted for. (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?). If temperature is linked to ENSO then the ability to project/predict future temperatures depends on the ability to predict ENSO conditions, and the accuracy of predictions weakens after 6 months into the future and 12 months appears to be the absolute limit. Further, applying the 7-month time lag described in this paper suggests no significant warming or cooling before about June 2013 at the earliest. [John McLean, Australia]	Rejected - see response to comment 10-958
10-2021	10	64	38	64	38	I think this needs to be made more clearly conditional on the chosen emissions scenario, and I think it also needs to make the point that a precise "prediction" is not possible. Consistent with comments on other FAQs, I also think it is best to avoid the word "predict", particularly in contexts where the discussion concerns projections. Here is a suggestion for another way to write this: "The date of future emergence of a warming trend projected under a given emissions scenarios cannot be determined precisely because this also depends on local climate variability, ..." [Francis Zwiers, Canada]	Taken into account -- wording is changed to avoid implications of precise prediction
10-2022	10	64	38	64	45	This paragraph feels very out of keeping with the rest of the FAQ in terms of both language and degree of technical details / jargon. I'm not sure that the most appropriate place is here for such language. [Peter Thorne, United States of America]	Taken into account - we have striven to maintain a reasonable balance between thoroughness and clarity.
10-2023	10	64	41	64	43	This statements would be valid if and only if the models used were 100% accurate for all natural forces. If this is not the case then the statement should be removed because attribution between natural and manmade forces cannot be made with flawed models. [John McLean, Australia]	Rejected - see response to comment 10-1961
10-2024	10	64	49	64	49	Reasoning seems strange here; hard to see how global analyses do inform us about local changes? [Albert Klein Tank, Netherlands]	Rejected - global analyses do inform us about local changes if global trends are of sufficient magnitude to account for a significant fraction of local climate variance.
10-2025	10	64	50	64	50	This comment is dishonest while climate models remain flawed and therefore it should be deleted. [John McLean, Australia]	Rejected - the consensus view is that there is indeed a wealth of evidence from across the climate system.
10-2026	10	64	53	64	58	The choice of colours for the global map is irritating. Blue shows the zones which highest temperature increases while red are those with lowest increases. Better vice versa. It also unclear what the map shows: I assume differences in temperature per grid cell for two observation periods, indicate which [European Union]	Accepted - The colour bar is reversed in the Third Order Draft.
10-2027	10	64	53	65	2	The caption to Figure 1 of FAQ 10.2 is problematical, and the figure itself may need changing accordingly. The	Accepted - the figure is replotted and seasonal

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						reference to "summer" and "winter" are OK for the points in the northern hemisphere. But the bottom right point looks to be on or just south of the equator. If it is south of the equator the DJF curves should be coloured red, and the JJA curves should be coloured blue. If the point is actually on the equator, it would be better to choose a different point. [Adrian Simmons, United Kingdom]	variability clarified for the two hemispheres
10-2028	10	65	6			Please add a FAQ: "Why has the global mean temperature not increased in the last decade, despite the increase in anthropogenic releases of GHG? [Government of NORWAY]	FAQ topics were determined prior to the first draft of AR5 in 2011. We cannot add a new FAQ to AR5 at this stage in the writing process. However there is a new box 9.2 which assesses the last 15 years.
10-2029	10	66		88		Following references may be added:Oza S. R., I. M. L. Das, R. K. K. Singh, A. Srivastava, M. Dash, and N. K. Vyas, 2011a: Inter-annual variations observed in winter and summer Antarcic sea ice extent in recent decade. MAUSAM, 62, 633-640. Oza S. R., R. K. K. Singh, N. K. Vyas and Abhijit Sarkar, 2011b: Spatio-temporal analysis of melting onset dates of sea-ice in the Arctic. Indian Journal of Geo-Marine Sciences, 40, 497-501. Oza, S. R., R. K. K. Singh, N. K. Vyas and Abhijit Sarkar, 2011c: Study of inter-annual variations in surface melting over Amery Ice Shelf, East Antarctica using space-borne scatterometer data. Journal: Journal of Earth System Science, 120, 329-336. Srivastava, A, I. M. L. Das, S. R. Oza, A. Mitra, M. Dash, and N. K. Vyas, 2011: Assessment of Sea Ice Melting Rates in the Antarctic from SSM/I observations. MAUSAM, 62, 601-608. Bolch T, A. Kulkarni, A. Kääb, C. Huggel, F. Paul, J. G. Cogley, H. Frey, J. S. Kargel, K. Fujita, M. Scheel, S. Bajracharya, M. Stoffel, The State and Fate of Himalayan Glaciers, Science, 336 , 310 (2012); DOI: 10.1126/science.1215828 [Government of India]	Taken into account. We have included some of these references.
10-2030	10	67	29	67	32	Booth et al 2012a and 2012b are the same reference: should keep 2012a [Fabrice Chauvin, France]	Corrected thanks.
10-2031	10	68	30	68	31	check format of title [European Union]	Editorial.
10-2032	10	68	35	68	35	update publication [European Union]	Done.
10-2033	10	69	13	69	12	check format of title [European Union]	Done.
10-2034	10	69	31	69	31	Page numbers for DelSole at al. (2011) are 909-926. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Done.
10-2035	10	70	47	70	47	Please add missing paper ID and doi: " L16706, doi:10.1029/2011GL048529". [Georg Feulner, Germany]	Accepted.
10-2036	10	71	36	71	36	Add references: Frederiksen, C.S., J.S. Frederiksen, J.M. Sisson and S.L. Osbrough, 2011: Australian winter circulation and rainfall changes and projections. Int. J. Clim. Change Strat. Mang., 3, Issue 2, 170-188. Frederiksen, J.S., C.S. Frederiksen, S.L. Osbrough and J.M. Sisson, 2010: Causes of changing Southern Hemispheric weather systems. Chapter 8, Managing Climate Change, Eds. I. Jupp, P. Holper and W. Cai, CSIRO publishing, pp85-98. Frederiksen, J.S., and C.S. Frederiksen, 2007: Interdecadal changes in Southern Hemisphere winter storm track modes. Tellus, 59 A, 599-617. [Jorgen Frederiksen, Australia]	Rejected. Given length constraints references have not been included.
10-2037	10	74	32	74	34	Very similar references. Is this the same paper? [Government of Australia]	accepted. Corrected.
10-2038	10	75	34	75	36	Karoly and Wu 2005a and 2005b are the same reference [Fabrice Chauvin, France]	Accepted. Corrected.
10-2039	10	76	6	76	6	Reference can also be appropriately inserted after line 6 Kishtawal C.M., Neeru Jaiswal, Randhir Singh and D. Niyogi, Tropical cyclone intensification trends during	Rejected due to length constraints.

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						satellite era(1986 2010),Geophysical Research Letters, Vol. 39, L10810, doi:10.1029/2012GL051700, 2012 [Government of India]	
10-2040	10	78	6	78	9	Mann and Emanuel 2006a and 2006b are the same reference:correct pages are 233,238,241 [Fabrice Chauvin, France]	Accepted. Corrected.
10-2041	10	79	25	79	29	Molg and Kaiser reference should possibly be Mölg, T. and G. Kaser (2011), A new approach to resolving climate-cryosphere relations: Downscaling climate dynamics to glacier-scale mass and energy balance without statistical scale linking, J. Geophys. Res., 116, D16101, doi:10.1029/2011JD015669. [David Parker, United Kingdom of Great Britain & Northern Ireland]	The reference no longer used and taken into account.
10-2042	10	79	32		34	Are these really 2 separate references? [William Ingram, United Kingdom]	Taken into account. Corrected.
10-2043	10	83	51	83	51	Page numbers for Sheffield and Wood (2008) are 432-458. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Corrected.
10-2044	10	87	30	87	32	Change Wong et al. 1999a to Wong et al 1999 and remove Wong et al. 1999b [Taiyo Kobayashi, Japan]	Taken into account. Corrected.
10-2045	10	87	63	87	63	Please change "Submitted to Nature" to "Submitted to Jpournal of Atmospheric Sciences". [Rong Zhang, United States of America]	Taken into account. Corrected.
10-2046	10	89	1	90	23	Presumably HadCRUT3 here should be HadCRUT4? The Figures themselves say 4. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text has been corrected.
10-2047	10	89	5	89	5	"Observed global annual mean temperature" What nonsnse! Nobody has ever measured such a quantity. It would require somultaneous measurements of thermometers situated randomly over the entire earth's surface. Including the oceans. You are surely referring to the so-called "Annual Global Surface Temperature Anomaly" which is not a temperature at all. but a complex multi-average of a large number of unrepresentative non-standard weather station maximum an minimum temperature measurements. This concoction bears only a very slight resemblence to any genuine global mean suface temperature [Vincent Gray, New Zealand]	Rejected. GMST anomaly is the term used.
10-2048	10	89	27	89	30	I thought that models ran with 360 day years. Do some now have 365/6 days years. The obs from HadCRUT4 are derived from monthly averages. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised to make clearer.
10-2049	10	89	37	89	45	I assume the final draft will use HadCRUT4 and this text and the figures will both be amended accordingly? If not then the issue of using a dataset not used by Chapter 2 comes into play here. [Peter Thorne, United States of America]	Noted. Yes it is redone with HadCRUT4.
10-2050	10	89	47	89	48	A "Global Mean Anomaly: is not the same as a "Global Mean" [Vincent Gray, New Zealand]	Noted. GMST is the term used.
10-2051	10	89		90		Appendix 10.A. Cite HadCRUT4 grid rather than HadCRUT3 grid. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text revised.
10-2052	10	90	39	90	39	I significantly disagree with using two versions of the HadCRUT product which are very far from independent and an unpublished JMA analysis here. I think it raises very substantive cross-chapter consistency issues which lays both Chapters 10 and 2 open for unwarranted criticisms. On a practical and a philosophical level we should use only published analyses and only the latest incarnation of each group's offering. [Peter Thorne, United States of America]	Accepted. JMA Dropped. HadCRUT4 used.
10-2053	10	92	17	92	18	Pressure levels my be better specified in hPa (smaller numbers). [Christian-D. Schoenwiese, Germany]	Accepted.
10-2054	10	92	25	92	33	Caption of Figure 10.10 refers to Noake et al. 2012 but not to Zhang et al. 2007 though the text cites the latter. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text revised.
10-2055	10	92	57	92	57	…. based on … (d missing, typo). [Christian-D. Schoenwiese, Germany]	Editorial.
10-2056	10	93	16	93	16	… in the frequency... (space between words missing). [Christian-D. Schoenwiese, Germany]	Editorial.
10-2057	10	93	28	93	28	For readers outside the UK (99%) reference to 'the Don area' will make no sense whatsoever. [Peter Thorne, United States of America]	accepted. Caption revised to avoid referring to the Don valley.

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10-2058	10	94	44	94	44	Pierce et al., 2012 submitted is now published (2012) [Paul Durack, United States]	Editorial, and published reference is used.
10-2059	10	95	1	95	1	changing "most" to "more than half of" [Zong-Ci Zhao, China]	Editorial.
10-2060	10	95	1			Table 10.1: We noticed several inconsistencies in this table, relative to the underlying chapter and ES statements, and there are most likely more we did not notice. The usefulness of such a long, multi-page table could be questioned, and presents a big risk of introducing inconsistencies of the type we have noticed. The authors might want to consider if such a long table is really useful, and if this table remains, please very carefully check every entry to ensure any post-SOD revisions are carried forward into the table. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The table has been heavily revised to be consistent.
10-2061	10	95	3	95	3	I think this comprehensive table is very useful, but there does seems to be quite a bit of repetition between the columns describing the assessment (columns 6-8). Perhaps that could be reduced. Also, not all statements in column 1) are sufficiently concise to serve as pithy assessments when combined with the terms in columns 2) and 3). [Francis Zwiers, Canada]	Taken into account. The table has been simplified by combining information in multiple columns.
10-2062	10	95	5	95	5	Heading - suggest replacing "Global Scale Temperature Changes" with "Global Scale Atmospheric Temperature Changes", since the subsequent collection of roles only deals with atmospheric temperature. [Francis Zwiers, Canada]	Accepted.
10-2063	10	95	5	95	5	Row 2: The descriptions of the number of studies in columns 5 and 7 do not correspond all that well ("Some … studies" is used in column 5, while "a number of studies" is used in column 7. [Francis Zwiers, Canada]	Accepted. Table revised to correct.
10-2064	10	95	5	95	5	Row 2, column 8: As mentioned in a previous comment, citing circulation changes as a cause is a bit open-ended if we don't know the causes of those changes. [Francis Zwiers, Canada]	Taken into account. This was meant as a caveat but sentence has been revised to try to make that clearer.
10-2065	10	95		103		This table took a lot of effort to create and collate and does contain useful information for the expert. But I am unconvinced it does anything for the intended reader and I fear that for a layman reader non-expert in the field it is all but inaccessible and at 8 pages long incredibly daunting. My personal recommendation would be to transplant the current, dense, table to the appendix and create a simplified table with text only in the top row and first column and consisting of elsewhere a 'traffic light' colour scheme fill of all the cells (red, amber, yellow, light green, dark green) in place of the current text reflecting the confidence in all remaining cells. This simplified form in main text backed up by the information rich table in the auxillary information would be the best of all worlds in my view making for a substantively more accessible 'look up' table in the main text, backed by the detailed evidence / audit trail in a companion piece in auxillary information, [Peter Thorne, United States of America]	Taken into account. The table has been simplified with fewer columns. However we have retained it in the chapter body as it contains the essential information to trace the headline statements back to the main pieces of evidence.
10-2066	10	95		131		This synthesis table is very useful. Yet it is not always crystal clear which model ensemble has been used (AOGCMs and/or ESMs).Be careful that acronyms used for coupled models differ between chapters. [European Union]	Taken into account. Revised table makes clear when cmip3 or cmip5 for exmple.
10-2067	10	96	1	96	1	Row 4, column 1: Another way to make the statement would be to say something like "GHG induced warming has continued during the past 15 years, but has been offset by decadal-scale internal variations that have had the effect of sequestering much of the additional heat in the oceans". This would be a more direct way of describing our understanding of the current situation, and you could assess the confidence that you have in this statement, and I think also give a likelihood assessment when cast in this way. [Francis Zwiers, Canada]	Taken into account. This statement has been considerably revised and follows the conclusions of the new box 9.2.
10-2068	10	96	1	96	1	Row 5, column 5: There is some sloppy wording here - the attribution studies are NOT conducted on models, they are conducted on the observations. Models are used to estimate expected signals and internal variability. [Francis Zwiers, Canada]	Accepted - text revised
10-2069	10	96	1	96	1	Row 5, column 8: I'm not sure that "better sampled" describes the improvement (sampling suggests making a selection of observations). Perhaps replace this with "better documented" or "better understood"? [Francis Zwiers, Canada]	Accepted - text revised
10-2070	10	96	1	96	1	Row 6, column 8: Should "variability well represented" have a qualifier - perhaps "variablity is reasonably well represented"? [Francis Zwiers, Canada]	Accepted.
10-2071	10	96		96		Table 10.1. Row 5 box 7: no new studies? - you have Lott et al. 2012. [David Parker, United Kingdom of Great	Accepted - text revised.

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						Britain & Northern Ireland]	
10-2072	10	97	1	97	1	Row 9, column 1: Suggest rephrasing this in the past tense "Most of the increase in … is due to …" [Francis Zwiers, Canada]	Accepted - text revised
10-2073	10	97	1	97	1	Row 9, column 5: Perhaps replace "3-5" with "several"? Why is the number of studies uncertain (and does the range reflect a defined uncertainty range :))? [Francis Zwiers, Canada]	Accepted - text changed to several
10-2074	10	97	1	97	1	Row 9, column 8: It seems a bit odd to say that a temperature data set is a basis for observations (doesn't a dataset consist of observations??). Stating at the end that there are "no significant confounding factors" seems a bit over confident to me (and feels a bit like putting up a red flag asking for challenges). It wouldn't hurt to acknowledge additional uncertainties, such as the very limited instrumental observations pre-1950 (wouldn't that imply that there are limitations in the ability to study low-frequency natural variability in OHC?). [Francis Zwiers, Canada]	Accepted - text revised.
10-2075	10	97	1	97	1	Row 10, column 5: Perhaps replace "3-5" with "several"? Why is the number of studies uncertain (and does the range reflect a defined uncertainty range :))? [Francis Zwiers, Canada]	Accepted - text changed to several
10-2076	10	98	1	98	1	Column 4, rows 11-13: I think it would be useful to actually describe the evidence. [Francis Zwiers, Canada]	Accepted. Table has been rationalised with fewer columns.
10-2077	10	98	1	98	1	Row 11, column 8: There is somewhat confused use of the uncertainty language here - suggest replacing "The likely confidence level based ..." (which incorrectly mixes two types of uncertainty language in a very parsimonius way!) with "The likely assessment is based ..." [Francis Zwiers, Canada]	Accepted - text revised.
10-2078	10	98	1	98	1	Row 12, column 7: Not sure what the bit about "measurement uncertainty across the oceanographic literature" is attempting to say. Throughout this table, verbs and connecting words, such as "and", are frequently missing, sometimes making it harder to understand clearly what is being said. The need for clarity should obviously trump the need to keep the table compact, and using a few more characters shouldn't increase its size excessively. [Francis Zwiers, Canada]	Taken into account - text revamped and revised.
10-2079	10	98	1	98	1	Row 13, column 1: Suggest rephrasing this as "The observed decrease in global oxygen content can not be explained by natural internal variability" so that the target of the assessment is stated more clearly. [Francis Zwiers, Canada]	Taken into account - statement revised.
10-2080	10	98	1	98	1	Row 13, columns 6 and 7 - replace "studies" with "study". [Francis Zwiers, Canada]	Accepted - text revised
10-2081	10	99	1	99	1	Row 14, column 1: I don't think that the word "signicant" should generally appear in a statement that is to be assessed. That would imply making an assessment (using either confidence or likelihood language) of the result of a statistical test, where the testing procedure itself has already been designed to account for some range of uncertainties. Generally, whether something is significant according to a defined inference procedure, is a fact (that available data, plus a given statistical inference procedure, will lead to a clear determination of significance). If you say that there is xxxx confidence, or that it is yyyyy likely, that something is significant, you are either speculating about what a statistical test might say it it were to be conduct, or you are expressing doubt about the statistical procedure that has been used to infer whether some change is significant. The latter might be appropriate in some cases, but I think that source of uncertainty would have to be made clear in the discussion in the text (and subsequent columns). [Francis Zwiers, Canada]	Accepted - text revised.
10-2082	10	99	1	99	1	Row 14, column 4: Also mention which models are used? [Francis Zwiers, Canada]	Taken into account. Revised to refer to cmip3 and cmip5 models.
10-2083	10	99	1	99	1	Row 15, column 8: Do we know that climate models have difficult simulating extreme precipitation? I think we do know that this is true in some regions (e.g., those affected by tropical cyclones, which most models cannot simulate), but I'm not sure we know this in the mid-latitudes. What we do know is that assessment is very difficult (see, for example, Sillmann et al, 2012a, submitted, or Kharin et al, 2012, submitted). [Francis Zwiers, Canada]	Taken into account. Text revised.
10-2084	10	99	1	99	1	Row 16, column 2: Medium confidence seems a bit conservative, even with the caveats described in column 8. [Francis Zwiers, Canada]	Rejected. Text provides evidence.

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10-2085	10	99	1	99	1	Row 17, column 5: Why distinguish between "new studies" and "recent studies"? [Francis Zwiers, Canada]	Accepted - text revised
10-2086	10	99	1	99	1	Row 17, column 7: Something missing … "limited number of ???? Across studies". [Francis Zwiers, Canada]	Taken into account - corrected.
10-2087	10	99		100		Table 10.1. Rows 17 and 18 box 8: large-scale atmospheric winds? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
10-2088	10	99				Table 10.1 Result 17: Col2 "glaciers have lost significant mass … 1960s." Col8 "limited number of across studies" is unclear. Col9 "strongly dependent"; the final sentence needs to be cleaned up. [J. Graham Cogley, Canada]	Taken into account - text revamped and revised.
10-2089	10	100	1	100	1	Row 18, column 5: What distinguishes between "new" and "recent" studies? The "new" studies will no longer be new when the report is finally published. [Francis Zwiers, Canada]	Accepted. Corrected.
10-2090	10	100	1	100	1	Row 18, column 7: Some words seem to be out of order - did you intend to say "High agreement across a limited number of studies"?. [Francis Zwiers, Canada]	Accepted. Corrected.
10-2091	10	100	1	100	1	Row 18, column 8: This seems a bit unclear. I think the last sentence is intended to further explain the statement that internal variability is poorly characterized, but that might not be comletely clear to readers. Also, the connection between atmospheric winds and mass loss in 2010 and 2011 is not very apparent in the supporting text (10.5.2.1), which instead makes a link to Greenland west coast temperatures in those years. [Francis Zwiers, Canada]	Text in 10.5.2.1 has been revised and taken into account. Senetnce on winds has been deleted to align with section 10.5.2.1
10-2092	10	100	1	100	1	Row 20, column 2: I'm not sure what the bit in parametheses is intended to say; is there a typo? [Francis Zwiers, Canada]	Editorial and taken into account - revised.
10-2093	10	100	1	100	1	Row 20, column 3 ("substantial retreat, [that is] larger than [simulated by] models"): I assume that I am reading this correctly. Does this statement hold for CMIP5 as well as CMIP3? The Arctic sea-ice panel suggests that CMIP5 does a bit better than CMIP3, although it looks like the observed change lies near the lower end of multimodel uncertainty range for CMIP5 historical forcing runs. Perhaps there is room to note that there has been some improvement? [Francis Zwiers, Canada]	Only CMIP5 models are used in the upated figure. See new figure and supplementary materials. While means are better compared with CMIP3 in CMIP5, there is no improvement in uncertainty spread.
10-2094	10	100	1	100	1	Rows 20 and 21: One question that arises when reading column 8 for these two rows is the extent to which "structural uncertainty" in the sea ice models contributes to overall uncertainty. The level of confidence in our understanding of change in the two polar regions is clearly very different - but the same sea-ice models are clearly used in both regions. Should a reader infer that the sea-ice models themselves are not a major source of uncertainty? [Francis Zwiers, Canada]	Reference is now made to Section 9.4.3, and the evaluation of sea ice models. They do have different performance metrics for the Arctic and Antarctic. However sea-ice is a major source of uncertainly, not just sea ice parametrization, but clouds and atmospheric dynamics also crucial to the system
10-2095	10	100				Table 10.1 Result 18: Col2 Delete "ice" or change it to "Ice Sheet". The remaining columns repeat the columns of Result 17. [J. Graham Cogley, Canada]	Accepted.
10-2096	10	100				Table 10.1 Result 19: Col2 Change "caused by" to "attributable to". Col3 Confidence is assessed as low, not very low, at P39 L51. Col9 "of the Antarctic Ice Sheet and its interaction". "atmospheres" should be "atmosphere". Delete ", and their attribution to anthropogenic forcing". [J. Graham Cogley, Canada]	Accepted - text revised as suggested
10-2097	10	100				Table 10-1, item 21. This conclusion about sea ice changes in the Southern Ocean depends on our estimate of the magnitude of internal variability which is highly uncertain. Because of the mentioned low level of agreement on the physical processes, I consider that the confidence is low on this issue. [Hugues Goosse, Belgium]	Accepted - text revised - and main text in 10.5.2 notes the uncertainty of sea-ice model performance. The row (now item 22) is also modified, and the confidence is set at low.
10-2098	10	100				Table 10.1. Row 20 box 5: I think the main text mentions more than 2 detection and attribution studies. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
10-2099	10	100				Table 10.1: Result 19 (Antarctic ice sheet MB) -> "very low confidence" here, compared to "low confidence" given in the ES and chapter text (page 39, line 39). [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised and consistent confidence levels for Antarctic ice sheet mass balance.
10-2100	10	100				Table 10.1: Result 20 (Arctic sea ice) -> As already noted, "very likely" here is inconsistent with "likely" given earlier in text and ES. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised - consistent use of likelihood language

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10-2101	10	100				Table 10.1: Result 22 (Snow cover...) -> As already noted, "high confidence" here, is given as "medium confidence" earlier in the chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised - consistent use of likelihood language
10-2102	10	101	1	101	1	Rows 22 and 23 (and elsewhere): I noticed here that two successive entries in column 1 use different terms to refer to the response to anthropogenic forcing. It would be good, I think, to consistently use a single term throughout. [Francis Zwiers, Canada]	Taken into account. Tried to use anthropogenic forcing and human influence as the main terms.
10-2103	10	101				Table 10.1. Row 22 box 4: CMIP3 should be CMIP5. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
10-2104	10	102	1	102	1	Row 26, column 4: The North Atlantic does have a long, well studied, and relatively complete record, so perhaps this statement could be nuanced a bit, perhaps by adding "in most basins", or something to that effect. [Francis Zwiers, Canada]	Accepted - text revised
10-2105	10	102	1	102	1	Row 26, column 5: I think the sentence beginning with "However …" could point to uncertainty in process understanding more broadly. A suggestion would be to say "However, the mechanisms that link anthropogenically induced climate change, including that in SST, to changes in tropical cyclone activity are poorly understood." [Francis Zwiers, Canada]	Accepted - text partially revised to reflect comment, and
10-2106	10	102	1	102	1	Row 27, column 1: Insert "northern" before "hemispheric"? [Francis Zwiers, Canada]	accepted
10-2107	10	102	1	102	1	Row 27, column 8: Maybe insert a qualifier before "good agreement" (e.g., "generally good agreement")? [Francis Zwiers, Canada]	editorial
10-2108	10	102	1	102	2	As I pointed out in my comments to AR4 these graphs are spurious because they do not take into account the main reasons for the supposed increases which are the ocean oscillations (natural,notably ENSO) and urbanisation and land use change (anthropognic). [Vincent Gray, New Zealand]	Rejected -the lines of sevidence support these table elements.
10-2109	10	102				“Continental to regional scale changes”: This heading seems odd, because most of the previous two pages have been continental or regional. How are these different? [Dáithí Stone, United States of America]	Reject - previous pages have been continental and greater, or a particular process such as tropical cyclones or phenomena such as SAM
10-2110	10	102				#29: What does the “separately due to different dynamics” mean? [Dáithí Stone, United States of America]	Accepted - text revised
10-2111	10	102				#31: What is “some” here in the first column? And again in the second? I'm not sure that these statements are falsifiable. [Dáithí Stone, United States of America]	Accepted, relevant items have been rephrased
10-2112	10	103	1	103	1	Row 31, column 7: I wonder if the 2nd sentence isn't a bit over-confident. Perhaps it would be appropriate to insert "may" or "appear to" before "have played"? [Francis Zwiers, Canada]	Accepted - text revised as suggested
10-2113	10	103	1	103	1	Rows 31 and 32, column 1: It might be helpful if you could distinguish more clearly between the two attribution approaches that these two rows consider (attribution of the causes of changes in the climatologoligical characteristics of temperature extremes in Row 31, and attribution of the causes of indivual observed heatwave events in Row 32). A suggestion for row 32 would be to say "Human influence made the occurrence of several recent long-duration heatwaves substantially more likely." This says almost the same thing, but it makes it clearer that row 32 is about event attribution. [Francis Zwiers, Canada]	Accepted - text revised as a variant of the suggestion here. The distinction is clear now between TOD version of these two rows.
10-2114	10	103				Table 10.1. Row 32 box 8. "Possible confounding influences include urban heat island effect." True, but not mentioned in Section 10.6.2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Urban heat island now discussed in 10.6.1 but has been removed in the Table 10.1.
10-2115	10	103				#32: Again, what is “some”? Why isn't there an entry for “human influence has substantially decreased the probability of some observed heatwaves”? [Dáithí Stone, United States of America]	Accepted - text revised and aligns with the Executive summary, Human influence has substantially increased the probability of occurrence of heatwaves in some locations.
10-2116	10	105	1	105	10	green and black colour cannot be differentiated [European Union]	Figure is now changed
10-2117	10	107	1	107	10	Some people seem to think a chart such as Fig. 10.1 somehow gives credibility to climate model projections. The results depicted here are not scientific tests because every modeling group saw (knew) the "answer" (observations) ahead of time - therefore it is not a test of capability or accuracy, i.e. this is not a blind test by	Rejected. The point this figure is making is that the observations lie within the plume of models when the models including anthropogenic forcings and not

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						any means. A little better test is checking some component like tropospheric temperature at the end of the record (i.e. since 1979) because that is the time when the forcing is largest and most easily detectable in observations and models, i.e. tropospheric warming has a larger signal than surface warming. Such a comparison shows model failure in standard statistical tests. I realize such a result is not consistent with the views of those writing this document, but science doesn't rest on comparisons between dependent variables while implying the comparison is somehow of independent quantities. The readers need to see such results presented clearly and objectively (see remarks regarding Fig. 10.7). [John Christy, United States of America]	when they exclude anthropogenic forcings. Of course there is plenty of incentive for groups to provide climate model runs that do a better job with natural forcings only than with anthropogenic forcings since this would be a much more interesting conclusion than what the models actually show. Also there are other comparisons made in the chapter including the snythesis figure updating fig 10.20 in the sod which shows that this is also the case for a variety of aspects of the climate system indicating that it isn't just surface temperature for which models do a much better job when they include anthropogenic forcings than when they don't.
10-2118	10	107	5			But there are blue lines for the natural scenario in panel b, while CMIP3 did not include any such simulations. [Dáithí Stone, United States of America]	Accepted, and figure now modified.
10-2119	10	107				I think it is important to include year 2011 in the figure (also in FAQ 10.1 Figure 1) [Jochem Marotzke, Germany]	Accepted. Both 2011 and 2012 have been included in the figure.
10-2120	10	107				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 B. and Schwartz S. E.: Observing and Modeling Earth's Energy Flows. Surveys Geophys. 33 779-816 (2012). DOI 10.1007/s10712-012-9184-0 Figure 11. Also Mauritsen, T., et al. (2012), Tuning the climate of a global model, J. Adv. Model. Earth Syst., 4, M00A01, doi:10.1029/2012MS000154. 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 be expected to 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, United States of America]	Rejected. A comparison of the absolute global mean temperature from models with observations is made in chapter 9 (Fig 9.8) so there is no need to repeat it here. But also note that there is a large range of uncertainty in the average absolute global surface temperature - Jomes et al, 1999 provides an estimate of 14C but as it states in Jones et al there is an error or oder 0.5C - "Compari-sonwith the earlier climatologies suggests the value that of 14øC is within 0.5øC of the true value."
10-2121	10	107				As comments elsewhere JMA should not be used unless published, in which case JMA needs to also be included in Chapter 2. [Peter Thorne, United States of America]	Accepted. JMA has been deleted from figure.
10-2122	10	108	9			How mny models were used in rows 2-4? [Peter Guttorp, United States of America]	Taken into account. The number of models used is set out in the supplementary information.
10-2123	10	108				Figure 10.2 Why change the trend units from column to column? [Peter Guttorp, United States of America]	Noted. The change in trend units is so that we can display the trend over the period and then use the same colour scale throughout.
10-2124	10	109				Throughout: Graphs of quantities against latitude should be plotted against sin(lat) (area weighted) so as not to give distorted impression. [Stephen E Schwartz, United States of America]	Rejected. This is a standard projection.
10-2125	10	111				Figure should use HadCRUT4 if possible for consistency within and across chapters [Peter Thorne, United States of America]	HadCRUT4 is used on this figure
10-2126	10	112	2	112	10	Panels on Africa and South America. The black lines represent the continent averaged annual temperatures and are derived form the HadCRUT4 database. However, the quality of temperature stations on these two	Taken into account. The figure has been revised to have dotted line where spatial coverage is less than

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						continents is extremely low. This is clearly reflected in Figure 2.22 on page 2-164: the upper left panel shows that the inner part of both continents is empty: no data. This is also reflected in Table 3-2 on page 193 of the SREX report: low confidence due to insufficient evidence (Tmax in Africa, etc.). Note: this vision deviates from AR4. These conclusions are consistent with my own analyses of old stations in Africa and South America. The quality (homogeneity) of stations having data for more than 60 years, is awfull. This is not surprising, of course, knowing their history over the past century. My advice would be: do not show these panels and give more qualitative statements for these two continents. [Hans Visser, The Netherlands]	50%.
10-2127	10	112	7			Figure 10.6: 'Antarctica' was not one of the SREX regions, so this detail in the caption should be modified. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Caption has been modified appropriately.
10-2128	10	113				Figure 10.7: An altitude axis would be helpful as a complement to the pressure axis. [J. Graham Cogley, Canada]	Accepted - and now added to the right side of figures
10-2129	10	113				Error estimates for the HadAT2 and RAOBCORE data should be included on the plot to indicate the significance of the mismatch in the data. [Chris Forest, United States of America]	Rejected - the additional errors bars would impair the interpretation of the figure
10-2130	10	114				Wmgg in in-line key. Wmghg in legend. Also, should this acronym be all capitalized? [Peter Thorne, United States of America]	Accepted - using a more natural and logial naming convention on figure
10-2131	10	115	4	116	25	The uncertainty estimates for the precipitation observations should be included in the plot in some fashion to indicate the significance of the mismatch between the model and observations. [Chris Forest, United States of America]	Accepted - and indicated with a green line where the data quality is better. Errors bars not added to the actual lines.
10-2132	10	115	4			Delete "CMIP3 and"in the caption to match the text. [Chris Forest, United States of America]	Caption revised
10-2133	10	116				Given my comments on Min & al (2011) (above, at 10-43, 43-44) I naturally think the 2 Rmost bars should be omitted from the 1st panel [William Ingram, United Kingdom]	Rejected - part of the literature, and therefore part of the assessment
10-2134	10	116				The in-line key is too small and therefore illegible for the upper right panel. [Peter Thorne, United States of America]	Taken into account. Models not delineated by colour in revision and inline key therefore larger and more legible.
10-2135	10	117	4	117	4	Write more exactly "… border of the Hadley cell" (insert "of the" and "cell"). [Christian-D. Schoenwiese, Germany]	Accepted - text revised, editorial
10-2136	10	117	4			southern boarder of Hadley' -> sentence seems incomplete, please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised, editorial
10-2137	10	118				It feels very odd to have Marshall SAM defined in the NAM panel. Perhaps it would be neater to bring the key out of the top panel and place it underneath the bottom panel as e.g. in Fig 10.6 [Peter Thorne, United States of America]	Accepted - legend is now clear, and by forcing
10-2138	10	119	1	119	1	Figure 10.13b: We find that this figure is difficult to understand and, at the same time, not adequately address in text. The figure should be replaced or improved. We recommend replacement. [Government of United States of America]	Accepted - text revised and figure has been simplified.
10-2139	10	119	1	119	1	fig. 10.13 Please spell out OHC in 10.13 and define the light grey area of 10.13a. [Government of United States of America]	Accepted - text revised, and caption revised.
10-2140	10	119				Figure 10.13: The text in this figure "Last year of L-length trend in .." should better explain the purpose of the figure. [Government of NORWAY]	Accepted - text revised, and caption revised as well
10-2141	10	120	1	120	1	fig. 10.14: The y axis labels need to be defined and the P-E label should be spelled out, psu defined in Fig. 10.14a. In fig 10.14b what is PA and PC and what are the numbers from 2 to 24 on the graph? In fig 10.14C what are the dotted lines? [Government of United States of America]	Accepted - figure has been simplified, and all elements labelled
10-2142	10	120	12	120	12	This is a regional detection but not an attribution (the detection of a signal attributable to anthropogenic forcing	Accepted - caption corrected

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						but without considering multiple causal factors). The caption should be corrected. [Serge PLANTON, France]	
10-2143	10	120				Figure 10.14. Define the white lines in A (lower panel) (presumably density); the straight lines in B (salinity change per °C warming). The scaling factor of 1 in C is mis-labelled -1. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted and now labelled correctly in panel C and also Panel A
10-2144	10	120				Figure 10.14: update reference to Terray et al 2011 see above comment [Laurent Terray, France]	The reference is now correct
10-2145	10	122	1	122	2	fig. 10.15 Fig a axis should be km2. Figure needs "a" and "b". Axis labels should be larger fonts. Caption b) needs to add February. [Government of United States of America]	Figure has been recast. Axis labels are more legible
10-2146	10	124	9	124	9	… in the frequency of … (space between words missing). [Christian-D. Schoenwiese, Germany]	Editorial
10-2147	10	124		124		Figure: 10.16: Do all the grey land areas contribute to the panels? For example, comparing the 'Southern Hemisphere Land' panel to the 'Australia New Zealand' panel there is clearly a difference between the two – but what other land areas also contribute to former? Perhaps show missing data areas with white to make this clear. [Government of United States of America]	Considered. There are limited data from grey land areas that contributed to the calculation for the global and southern hemisphere land panels. There are also missing values in other regions. As spatial distribution of missing data varies from time to time, it is difficult to show missing data areas with white.
10-2148	10	125				Figure 10.17: In panel c the red arrows do not stand out very clearly. This is worth some graphical-design effort, because the arrows could serve as a valuable illustration of the difference between attributable-risk and attributable-magnitude approaches discussed in section 10.6.2. [J. Graham Cogley, Canada]	Arrows have been emphasised in the revised figure
10-2149	10	126	1	126	1	Should "CU" be "CH" on the x-axis of the inset in panel (a)? [Francis Zwiers, Canada]	Accepted - figure has been revised
10-2150	10	126				Figure 10-126. The reconstruction with the acronym CL corresponds to the acronym CU on the figure if I am right. [Hugues Goosse, Belgium]	Accepted - and figure has been revised
10-2151	10	126				figure 10.18 Why are the uncertainty bars so small relative to unity in Figure 10.18 (a) in the inset? I would have thought the forcing is relatively small, though the length of the integration is long , relative to 20century D&A studies [John Mitchell, United Kingdom]	Accepted and figure has been revised
10-2152	10	126				Figure 10.18. Y-axis in tenths °C? In legend of (a) CU should be CL. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted and figure has been revised
10-2153	10	127	1	127	1	The main results estimate for Gillett et al 2012a should be included in the top panel of Figure 10.19 - it is probably the most reliable of all the estimates of TCR, because it uses the longest period of observational data (1851-2010). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Results from Gillett et al (2013) are included in the revised figure.
10-2154	10	127	1	127	1	The estimate from Libardoni and Forest 2011 should be removed since that study, in common with the all the Forest et al studies, suffers from several serious statistical errors (see comments on Chapter10, page 56, lines 9 to 23). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Libardoni and Forest have reassessed their result after correcting their statistical methods (Libardoni and Forest, 2013), and confirmed it makes very little difference.
10-2155	10	127	1	127	1	An estimated range for TCR from Forest 2006 is given, but that study gave no range for TCR. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	This is now superceded.
10-2156	10	127	1		2	Typo in Figure 10.19: In the figure Chlek&Lohmann should be replaced by Chylek&Lohmann. [Petr Chylek, United States of America]	Figure has been revised.
10-2157	10	127	2	127	2	The main results PDF from Aldrin et al (2012) has been omitted from the bottom panel of Figure 10.19, and should be added. [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	Figure has been revised
10-2158	10	127	2	127	2	The estimated ranges and PDFs from Libardoni and Forest 2011 should be removed since that study, in common with the all the Forest et al studies, suffers from several serious statistical errors (see comments on Chapter10, page 56, lines 9 to 23). [Nicholas Lewis, United Kingdom of Great Britain & Northern Ireland]	The revised figure shows the PDF from Libardoni and Forest (2013) correcting statistical errors.
10-2159	10	127				Figure 10.19. In the lower panel not all the curves implied by the legend are visible, e.g. Schwartz (2012); and some of the colours aren't very distinguishable. [David Parker, United Kingdom of Great Britain & Northern	Figure has been revised.

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						Ireland]	
10-2160	10	129	10	129	10	Why are only 1 sigma bands shown for precipitation and OHC? Why not 2 sigma, which is more broadly used? [Government of United States of America]	All panels on this figure are 5 to 95% ranges
10-2161	10	129				Figure 10.20: Precipitation panels; is these values only over land areas, it is not specified in the caption? However, the values and variability is very similar to the results in Figure 10.9 which shows values from areas of land where there are observations. [Government of NORWAY]	Precipiations panel is now just for the 50N to 90N area, where there is the strongest signal, compared with other latitude bands.
10-2162	10	130	1	130	2	As I pointed out in my comments to AR4 these graphs are spurious because they do not take into account the main reasons for the supposed increases which are the ocean oscillations (natural,notably ENSO) and urbanisation and land use change (anthropognic). [Vincent Gray, New Zealand]	Rejected - internal variability is included, and other forcings are discussed in more detail in the discussion of Fig 10.1 in the main chapter text.
10-2163	10	130	4	130	14	FAQ 10.1 Figure 1. Right hand side top and bottom figures need to say what the squares mean. [Government of United States of America]	Squares have been removed for clarity.
10-2164	10	130				FAQ 10.1, Figure 1. The "yellow" solid lines in the left panels, presumably ensemble averages, appear red rather than yellow. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Colour ambiguity should be fixed in this version of the plot.
10-2165	10	130				FAQ10.1, Figure 1: In the caption, please also explain the red lines in the two left hand panels. [David Wratt, New Zealand]	Accepted - the thick blue and red lines are ensemble averages, as the caption now explains.
10-2166	10	130				FAQ 10.1, Figure 1: What are the small square black-edged boxes scattered over the lowest right-hand panel ? [David Wratt, New Zealand]	Squares have been removed for clarity.
10-2167	10	131	1	131	1	fig. FAQ 10.2 Figure graphs have too many y axis numbers and no tick marks. The four regions pointed to are confusing. Are these local regions? What is the definition of these regions? [Government of United States of America]	Accepted - the figure is replotted and caption revised
10-2168	10	131	4	131	4	The sentence acceleration of water cycle is not clear. Pls, make it clear? [LUCILA CANDELA, Spain]	The page and line numbers seem to be incorrect. We are unsure what part of the text is referred to in this comment.
10-2169	10	131				FAQ 10.2, Figure 1. In southern Africa, JJA is winter (with more variance) and DJF is summer (with less variance) so the colours could be reversed in the lower right panel. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - the figure is replotted and seasonal variability clarified for the two hemispheres
10-2170	10	131				FAQ10.2, Figure 1: This is a nice illustration for the points being made in the text. However the colour scale for the central map seems counterintuitive for me. With the drak red indicating only small temperature increases being required for the anthropogenic signal to emerge from the variability, and the dark blue indicating a need for large temperature increases. (Normally blue indicates small increases and red large increases). You may wish to consider reversing the order of the colours ? [David Wratt, New Zealand]	Taken into account - The colour bar shows 'warmer' colors where the warming signal emerges most quickly (in terms of magnitude of climate change). We have tried to make this point more intutitive by explicitly explaining the colour bar in the text.