Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1	2	0	0	0	0	This figure in AR4 http://www.ipcc.ch/publications_and_data/ar4/wg1/en/faq-3-1-figure-1.html was misleading. AR5 is now repeating this figure in figure 1 of Box 2.2. It would be much more informative for the discussion if the trend 1979-2000 (or 1979-1998 or 1979-2011 or slightly earlier start years) is compared with the warming early in the 20th century (1910-1940). In his first interview after climategate Phil Jones admitted that the early 20th warming was as large as the late 20th century warming. This is an important admission because it means the rate of change is certainly not unprecedented. In this chapter there are a lot of tables which show trends for 1850-2010, 1901-2010, 1951-2010 and 1979-2010. I strongly encourage you to at least add the period 1910-1940 (or start and end years close to that). This applies to tables 2.2, 2.4, 2.6, 2.14. Comparing these periods is very important, also for the detection and attribution of climate change and understanding of internal climate variability. AR4 was almost silent about the early 20th warming. AR5 pays a little more attention to it, but apparantly relatively few scientists publish about this period. [Marcel Crok, The Netherlands]	Taken into account; an additional trend period 1901-1950 has been included (to make our periods consistent with Ch10 on D/A. The recent decade 1998-2010 will be discussed in the text.
2-2	2	0	0	0	0	There is a statement in the first bullet of the ES. This says that the warming is virtually certain. Later it is said the warming is unequivocal. The latter is correct. The warming is CERTAIN. [Philip JONES, UK]	Rejected; measurement uncertainties preclude a statement of absolute certainty; unequivocal is not in the IPCC uncertainty guidelines.
2-3	2	0	0	0	0	Putting all the confidence and likely statements in itralics is useful, but this doesn't happen in other chpaters. This means that if these words are not in italics they can be used in other contexts. [Philip JONES, UK]	Noted
2-4	2	0	0	0	0	Saying something is virtually certain seems a silly thing to say. This is in a bullet in the ES and elsewhere. I know there are phrases like racing certainty (and these horses don't always win), but for all in this chapter there is a change that when considering the error range, there is no possibility of the error range encompassing zero, therefor the word certain can be used [Philip JONES, UK]	Rejected; measurement uncertainties preclude a statement of absolute certainty; bands are for particular intervals only; unequivocal is not in the IPCC uncertainty guidelines.
2-5	2	0				The chapter is well structured and exhaustive. The executive summary is clear and represents a useful guidance for the reader as well as the reminders to the IPCC AR4 conclusions at the beginning of each section. The references are in general well cited and up-to-date. The present revision has been focused on section 2.4.1 and some comments are given as specified below. Only a short revision has been provided because at the moment I am out of work for motherhood. I hope to give a more profitable and detailed contribution to the WGI AR5 SOD review process. [Florinda Artuso, Italy]	Noted
2-6	2	0				In all the chapter, I suggest that submitted paper could not be cited, because not yet reviewed nor accepted. [Michel Boko, Benin]	Rejected; IPCC rules on the use of literature are applied
2-7	2	0				In all the chapter some explanations are too long. New findings should be more highlighted. (see p. 24) [Michel Boko, Benin]	Taken into account; the length of the chapter has been reduced from FOD to SOD.
2-8	2	0				In all the chapter, the word “dataset” should be written in the same wording (data set or dataset) [Michel Boko, Benin]	Editorial
2-9	2	0				I applaud the authors for the extensive overview. I especially appreciate the style of bringing in AR4 and then highlighting changes with that as a baseline. It verified many of the issues that have come to light in the literature since AR4. [Michael Brewer, United States of America]	Noted
2-10	2	0				What can be said in the Executive Summary about regional observed trends in surface air temperature? This is important information for understanding observed impacts. [Timothy Carter, Finland]	Accepted; sentence on regional differences added in ES based on 2-22, lines 2-8
2-11	2	0				Please consider to include the following article and its findings in this chapter Section 2.2.3. Grant Foster and Stefan Rahmstorf 2011 Environ. Res. Lett. 6 044022 doi:10.1088/1748-9326/6/4/044022 http://iopscience.iop.org/1748-9326/6/4/044022 [Øyvind Christophersen, Norway]	Accepted
2-12	2	0				Text is easier to read if abbreviations for expressions are explained and introduced when the expression is used for the first time (e.g.: "SE" for standard error is used in Table 1 of Box 2.2, but is not introduced as abbreviation in the table caption) [Birgit Hassler, USA]	Editorial
2-13	2	0				Keep unifrom rules of citation throughout the chapter. Sometimes if there is a reference to a paper directly	Editorial; this is endnote post processing which was

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						incorporated in the text, it is of the style "Schmidt et al. (2000) explained…", sometimes it is of the style "(Schmidt et al., 2000) explained..." [Birgit Hassler, USA]	incomplete.
2-14	2	0				The important terms "state, pattern, mode, regime, teleconnection" must be used very carefully. The pages 14-3 to 14-5 define those expressions in a pure statistical way. The paper by Stephenson et al. (2004, Q.J.R. Met. Soc.) also includes important dynamical aspects. [Wanner Heinz, Switzerland]	Accepted; references to ch14 included.
2-15	2	0				Overlaps with chapter 14 should be avoided (see e.g. the differences between Box 2.4/Table 1 and Box 14.2/Table 1). [Wanner Heinz, Switzerland]	Accepted; reconciled with ch14
2-16	2	0				Overall chapter 2 needs to tighten up on terminology. For example in Section 2.2.1 on "Land-Surface Air Temperature" LSAT is used sparingly as a acronym, sometimes "surface air temperature" is used, and also "land surface air temperature" without the hyphen. In general the chapter needs to define acronyms or consistent terminology and subsequently use it to avoid confusion. [Elizabeth Kent, England]	Editorial
2-17	2	0				I wondered if the use of AR1 to indicate a 1st order autoregressive process and AR4 to indicate the 4th assessment report was a good idea [Elizabeth Kent, England]	Editorial
2-18	2	0				It should be mentioned made at some point that there has been no statistically significant warming since 2001 if using a linear trend approach. [Philip Klotzbach, USA]	Accepted; see comment 2-1
2-19	2	0				I expected to see more emphasis on regional variability in this chapter of AR5. Of great concern is availability of information at the scale that the public and decision-makers can identify with and use, even for historic data. Uncertainties, of course, have to be identified. One improvement is to expand section 2.7, and add associated figures. The current figures map regional variation, but it would be good to see enlargement of regions in additional figures. [Beverly Law, USA]	Taken into account; regional information is added in the ES on warming differences; no regional detailed trend maps are included because the evidence for trends is weak.
2-20	2	0				I am wondering if a section could be added that deals with observations of processes, which are relevant for climate change, but do not present trends. E.g., the paper by Lendering & van Meijgaard, Nat Geosci, 2008, discusses observed super-Clausius-Clapeyron relationships, which are definitely relevant for climate change, but they do not study trends. This paper has been mentioned in Chapter 9, but would deserve a discussion in the observational chapter (maybe in Section 2.7.2, see also comment 16). [Douglas Maraun, Germany]	Taken into account; no separate section required as the information in this paper fits well in section 2.7.2. Extremes; Hydrological cycle
2-21	2	0				citations to Assessments are often too general (e.g., WMO, 2011); if made consistent with past reports they should be to the authors of the relevant chapters. [Stephen Montzka, USA]	Editorial
2-22	2	0				section 2.4.1, too many significant figures are presented for trace gas mole fractions [Stephen Montzka, USA]	Taken into account; no of significant digits made uniform throughout chapter.
2-23	2	0				Well-written on the whole, with very few problems. [David Pearson, United Kingdom]	Noted
2-24	2	0				The WATCH dataset seems to be omitted, but perhaps should be included. See WEEDON, GOMES, et al, "Creation of the WATCH Forcing Data and Its Use to Assess Global and Regional Reference Crop Evaporation over Land during the Twentieth Century", JOURNAL OF HYDROMETEOROLOGY 12, 823-848 (October 2011). [David Pearson, United Kingdom]	Rejected; WATCH is based on ERA-40 and this dataset has been included. Relevant trends in the WATCH dataset which are not seen in ERA-40 have been included.
2-25	2	0				Overall, this chapter presents a very good first draft. I have tried to review chiefly the evidence for quality of data and the evidence in time series of the observations. I have restricted myself to areas where I am more expert: SST and atmosphere observations from satellite, and also uncertainties. I find that in general there is a much better description of satellite data sets but it is still a litttle sparse in some sections in a manner inconsistent with the rest of the chapter. As importantly I found some good discussion of the errors but there was often no indication of uncertainties on the figures themselves. I think this important and hopefully the authors are well aware of this! I also found a few places where model studies using satellite data had not been included. I have tried to indicate these. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Taken into account; uncertainty information is added to the figures as much as possible.
2-26	2	0				Thank you for the opportunity to review the FOD of the AR5 WGI. Here I offer a plethora of comments on Ch. 2. Before offering specific comments, I'd like to explain the perspective I am bringing to this review process. First, note that I contributed a small bit of text and a figure to the chapter (in Section 2.6.5), but otherwise I	Noted

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						have not seen earlier drafts of any of this material. Second, I am very mindful of the fact that this is the AR5, so it should reflect the lessons learned over the past few decades of work on the previous four reports. Third, given the scrutiny this report is likely to undergo at this and later review stages, and once it is released, I feel it should reflect the best efforts of our community to present a clear and honest picture of the state of the science. Fourth, I think the IPCC assessments should be written in language that is easy for a graduate student (or more a senior scientist) in related fields to understand and that is fairly accessible to other educated readers. Having just completed a class in Plain Language communication, I am particularly sensitive to this issue. Fifth, although I very much appreciate the major time commitment that the authors have already made to this effort, and even though review comments are not submitted anonymously, I have not been shy about suggesting additional work that could improve the chapter. [Dian Seidel, USA]	
2-27	2	0				The chapter is very comprehensive and does not appear to neglect any important relevant work. However, it suffers from several significant problems that may not be especially easy to correct. Five of these are summarized in the next five comments. Details are given later. [Dian Seidel, USA]	Noted
2-28	2	0				(1) The chapter is very long and lacks clear focus. I cannot tell if it is meant to be an update of results since AR4 or a general discussion of observed atmospheric climate change. And, in contrast with other chapters (.e.g., Ch 10) presented with a pretty consistent "voice", Ch. 2 is clearly a compilation of disparate contributions. To rectify this, I suggest focusing on an update since AR4 and having a single author edit the entire chapter in a consistent style. Ideally, the chapter should read like a review article written by a single author, or a group of like-minded collaborators, not as a series of unconnected sections. [Dian Seidel, USA]	Taken into account; further harmonization in SOD.
2-29	2	0				(2) Despite a clear attempt at consistent calculation and reporting of trends (using consistent statistical methods and consistent time periods), this effort collapses early in the chapter, leaving the reader to struggle with a potpourri of results that are not consistently interpreted or assessed. To rectify this, I suggest avoiding discussion of all short period trends (e.g., less than 20 yr) and standardizing the trend period and calculation method as is already done for temperature. I recognize this is asking for additional calculations with datasets that have not been prepared for this sort of AR5 effort, and I would not have considered making such a suggestion for the FAR, SAR, or TAR. But by now, the community should be able to "get its act together" sufficiently to coordinate a consistent assessment of observed climate change. Also, please be mindful of the number of signficant figures in trend estimates. Do we really want to report temperature trends to the 0.001 K/decade? [Dian Seidel, USA]	Rejected; including trend periods longer than 20yr only is not meaningful for atmospheric composition. Different trend periods per variable explained in the introduction.
2-30	2	0				(3) The level of detail varies markedly from section to section, some reporting only key results and others reporting methodological details. To rectify this, I suggest summarizing key results and avoiding discussion of methodology. [Dian Seidel, USA]	Taken into account; methodology discussions have been shortened.
2-31	2	0				(4) The "abundant use" (p 2-9 line 45) of reanalyses in this chapter is an unwise decision, in my view, and it opens the chapter to unnecessary criticism. Even though the number of papers reporting climate trends computed from reanalyses is growing, the reliability of this work has not been demonstrated. It is the role of an assessment to judge what is worth assessing. I suggest greatly reducing the discussion of reanalysis-based trends, with particular caution regarding using the 20th Century Reanalysis (based only on surface pressure data) to evaluate upper-air changes. [Dian Seidel, USA]	Taken into account; 20c reanalysis not used for upper air trends.
2-32	2	0				(5) Somewhat related to the previous comment, but more comprehensively, I am concerned about the degree to which this chapter truly assesses recent work. This draft includes a number of significant statements that "backtrack" from AR4, which is essentially an indication that AR4 made statements with unjustified confidence. This reflects poorly not only on the IPCC process but also on the climate science community as a whole. Material that is not ripe enough for inclusion need not be included. It would be a shame for AR6 (if there is one) to have to backtrack on AR5 statements. [Dian Seidel, USA]	Rejected; backtracking from AR4 on the grounds of new evidence is not problematic. AR4 had uncertainty/confidence statements that allow for this. It will be unavoidable that AR6 will use the state of knowledge of that time to backtrack or confirm AR5 findings.
2-33	2	0				Some figure captions include references, others do not. Anything that is not the original work of the chapter should reference either the original version of the figure, or the data shown, or both. [Dian Seidel, USA]	Accepted; references added in all captions.
2-34	2	0				One more picky comment. Why is this draft called the "First Order Draft" rather than simply the "First Draft"? Think of how many words could be saved with this easy change! [Dian Seidel, USA]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-35	2	0				This chapter is in great shape for a first order draft - congratulations to all [Keith Shine, UK]	Noted
2-36	2	0				This Chapter is generally well written, the Executive Summary is excellent and serves as a model for rewriting the Executive Summary of Chapter 1. On a few occasions authors did not define complex ideas or acronyms [Robert Waterland, United States of America]	Noted
2-37	2	0				As a technical point - consistent precision in the number of digits given is required across the chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted; see 2-22
2-38	2	0				Careful coordination is required with Chapter 7 so that observed changes in clouds are well covered - possibility to include related figures in Chapter 2. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
2-39	2	0				While often it is essential and justified, in some instances the amount of text describing the methodological details of various studies seems excessive. Given Chapter 2 is on the long side, there is potential to shorten some passages by focusing on results. More extensive details regarding how methods have changed could be moved to an appendix. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted; see 2-30
2-40	2	0				Patterns of variability (Section 2.6) - coordinate with Chapter 14 to ensure consistency [Thomas Stocker/ WGI TSU, Switzerland]	Noted
2-41	2	0				Reference period - ensure to the extent possible that a consistent reference period is used. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
2-42	2	0				Ensure that in all relevant sections, a consistent treatment of the SREX assessment is included. In combination with the AR4, this latest assessment should form the basis for your update. For example, there is no reference to SREX made in regards to flooding (page 80), while SREX is discussed on the next page concerning droughts. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account; SREX reference added to section about flooding
2-43	2	0				Box 2.5: There is no mention here of 'return periods' which was a metric used extensively in SREX, and was the basis of figures which were to the SPM of SREX. This concept is now well established with the users of IPCC reports and should be employed where appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted; add sentence on this based on 2-80, lines 21-29
2-44	2	1	1	1		Observations: Atmosphere and Surface [Medani Bhandari, Nepal]	Noted
2-45	2	1		1		References Chapter 2 (and 10) Burn, D. H., Sharif, M., and Zhang, K. (2010) Detection of trends in hydrological extremes for Canadian watersheds. Hydrol. Process. 24, 1781–1790. Krakauer, N. Y. and Fung, I. (2008) Mapping and attribution of change in streamflow in the coterminous United States, Hydrol. Earth Syst. Sci., 12, 1111–1120. Gudmundsson, L.; Tallaksen, L. M.; Stahl, K.; Dumont, E.; Clark, D.B.; Hagemann, S.; Bertrand, N.; Gerten, D.; Hanasaki, N.; Heinke, J.; Voß, F. and Koirala, S. (2011) Comparing Large-scale Hydrological Models to Observed Runoff Percentiles in Europe. Journal of Hydrometeorology. doi: http://dx.doi.org/10.1175/JHM-D-11-083.1 Guo, Z., Dirmeyer, P. A., Gao, X. and Zhao, M.(2007) Improving the quality of simulated soil moisture with a multi-model ensemble approach, Q.J.R. Meteorol. Soc., 133 (624), 731-747, doi:10.1002/qj.48. Hannah, D.M., Demuth, S., Van Lanen, H.A.J., Looser, U., Prudhomme, C., Rees, G., Stahl, K. and Tallaksen, L.M. (2011) Large-scale river flow archives: importance, current status and future needs. Commentary. Hydrol. Proc, 25, 1191-1200. DOI: 10.1002/hyp.7794. Stahl, K., Hisdal, H., Hannaford, J., Tallaksen, L. M., van Lanen, H. A. J., Sauquet, E., Demuth, S., Fendekova, M., and Jódar, J. (2010) Streamflow trends in Europe: evidence from a dataset of near-natural catchments, Hydrol. Earth Syst. Sci., 14, 2367-2382, doi:10.5194/hessd-14-2367-2010. Stahl, K., Tallaksen, L.M., Hannaford, J. and van Lanen, H.A.J. (2012) Filling the white space on maps of European runoff trends: estimates from a multi-model ensemble. doi:10.5194/hessd-9-1-2012. [Lena M. Tallaksen, Norway]	Taken into account; Section 2.3.2 has included relevant findings in these hydrological journal papers.
2-46	2	1		75		Chapter 2 was reviewed most extensively and it looks fine. I have suggestions for the blocking sub-chapter above. [Anthony Lupo, USA]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-47	2	1		80		General comment: Overall, this chapter is too long, and at this point has too many writing styles evident. Some sections read quite well, while others do not read well at all. There are too many cases where a paper is cited, but no details about the data set used in that paper are indicated (this is particularly true in the temperature and column water sections). [Karen Rosenlof, United States of America]	Taken into account; see 2-28
2-48	2	1		80		General comment: Perhaps this isn'th within the purview of IPCC, but I think it would benefit the report to include recommendations as to what is needed to improve understanding of trends or processes for the species & quantities discussed in this chapter. [Karen Rosenlof, United States of America]	Rejected; key uncertainties in TS not in Ch2
2-49	2	1		80		General comment: Please check the chapter thoroughly for awkward sentence stucture. I haven't noted in this review most of the places where there are grammatical errors or poorly worded sentences. However, one example is on Page 63, where the word "changes" occurs 5 times in one sentence (first sentence in section 2.6). This isn't a problem throughout the document, but there are some sections that need some significant editing. [Karen Rosenlof, United States of America]	Accepted; see 2-28
2-50	2	1		80		General comment; Temperature and the hydrological parameters appear in two locations…once up front and then in the climate variability sections near the end. I'd suggest putting those parts closer together…ie, assess the quality of the data in the same section as the results using that data are assessed. [Karen Rosenlof, United States of America]	Rejected; breakdown always requires choices about interfaces; improved cross-referencing in SOD
2-51	2	1		85		This chapter is very well writtten and easy to follow. The second half has quite a lot of numbers and rates that need to be proofed carefully (as I am sure they have been) no matter how daunting that may be. There is some tendency to refer to various groups in an informal way ('Berkeley group, etc.) that should be made more formal. Some of the color combinations are hard for the color blind (who are dependent on intensity differences) to tell apart. Thicker lines in some figures would be a start. Using Yellow in a line drawing is almost never a good idea (see 2.11). [Larry Thomason, United States of America]	Taken into account; final colours for figures will change
2-52	2	1		178		The chapter 2 is generally very well written and organized. [Alice Grimm, Brazil]	Noted
2-53	2	1		178		This is a massive chapter and to some extent is a bit disjointed. The beginning is a bit like a shopping list of species, and then end is where there seems to be actuall discussion of climate implications. If possible, it would be better to reduce the length of the shopping list and make more statements as to the quality of the data being assessed. The end of the chapter is in better shape than the beginning. What i'd really like to see are more definitve conclusions. For example, for which data sets is there the most confidence in for comparing to models. [Karen Rosenlof, United States of America]	Taken into account; more focus in SOD
2-54	2	1		178		I think the chapter is a good first attempt and provides a comprehensive review of observed change. I wonder if the order should be atmospheric composition, radiation budget, temperature, hydrological cycle, circulation, patterns of variability, extremes. That way we go from climate drivers, radiative drivers, climate (in increasing detail). I do feel that the chapter is quite long and wonder if a more focused chapter would be more useful. I liked that differences from the AR4 findings were highlighted and I wonder if that should be the key aim of the chapter. So perhaps less on what is the same though keep those key statements in and more on the differences? [Simon Tett, United Kingdom]	Taken into account; alternative order of sections has been applied in SOD
2-55	2	1		178		I wonder if this chaper should also have an atlas of change with variables corresponding to the changes shown from models? [Simon Tett, United Kingdom]	The Atlas team has chosen to have no observations included because model evalutation is in ch9.
2-56	2	1				Chapter 2 paints a consistent picture of observed changes in parameters that determine, or are at least relevant to climate. I guess [Klaas Folkert Boersma, Netherlands]	Noted
2-57	2	1				that this was a deliberate choice by the authors, because when reading through this chapter, one gets the impression that the [Klaas Folkert Boersma, Netherlands]	Noted
2-58	2	1				measured changes mutually support one another. This will be convincing to many readers familiar with climate change science. [Klaas Folkert Boersma, Netherlands]	Noted
2-59	2	1				However, there is still considerable debate about the possibility that natural influences could explain observed climate trends and [Klaas Folkert Boersma, Netherlands]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-60	2	1				chapter 2 does not do much to present observations of the 'usual suspects' that are often brought up. To my opinion, showing a [Klaas Folkert Boersma, Netherlands]	Noted
2-61	2	1				number of time series of naturally varying climate parameters, such as AOT associated with volcanic eruptions (Fig. 2.18 in AR4 [Klaas Folkert Boersma, Netherlands]	Noted
2-62	2	1				is a good example), or changes in surface albedo (and associated changes in radiative forcing), would strengthen this chapter. This [Klaas Folkert Boersma, Netherlands]	Noted
2-63	2	1				could possibly be done in a Box 2.X titled 'Changes in natural climate influences' along with a discussion of peer-reviewed literature [Klaas Folkert Boersma, Netherlands]	Noted
2-64	2	1				demonstrating that such influences (in cosmic rays, solar activity, volcanic eruptions etc.) are probably insufficient to explain the [Klaas Folkert Boersma, Netherlands]	Noted
2-65	2	1				observed trends, or are in any case are not correlated with these trends. I think including a more detailed discussion of possible [Klaas Folkert Boersma, Netherlands]	Noted
2-66	2	1				natural influences (and for instance also lightning which is completely missing from Ch.2) and their observations could help in [Klaas Folkert Boersma, Netherlands]	Noted
2-67	2	1				strengthening the case that we think we now know what is driving climate change, and that it is unlikely that natural influences [Klaas Folkert Boersma, Netherlands]	Noted
2-68	2	1				mentioned above are the main agents. [Klaas Folkert Boersma, Netherlands]	Rejected; as explained in the introduction, this chapter does not attribute the changes to natural or human causes.
2-69	2	1				Due to medical emergency in Dec and heavy workload, I'm unable to provide a detailed review. Here are a few comments. [John Christy, USA]	Noted
2-70	2	1				General: The practice of writing in stone the understanding of observations as of mid-2012 (cut-off for publications) is almost a fool's errand. New findings will no doubt render the IPCC tablets out of date even before they are being printed. I'm dealing with new versions of upper air data now almost every year - in this case (2012) most new versions show even lower temperature trend values than previous versions. But, what will the next set of versions show? Higher trends? This is where the iPCC process needs to evolve into a dynamic process and take advantage of electronic information technology. This is the 21st century. How can the IPCC protect itself from the most simple of denigrations - "it's out of date"? [John Christy, USA]	Noted; likewise there are comments suggesting that well established findings should be included only, i.e. not the newest information. Also, websites exist which can update datasets on a regular basis, e.g. the UAH website on upper air temperature
2-71	2	1				A good balanced chapter, making clear what has changed since AR4. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted
2-72	2	1				The chapter is fairly well put together. However a general comment is that it often fails to carry out an assessment and rather lists a bunch of papers without saying which are flawed and why. However some sections require major revisions and need to be written by the whole team not a single author. I also looked at Chapter 14 and there is tremendous redundancy and often statements at odds between the 2 chapters.. [Kevin Trenberth, USA]	Taken into account; see 2-28
2-73	2	2	1	5	2	The Executive Summary content is good, but would flow better if combined into fewer sections, e.g. organised much as in the AR4 chapter 3. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected; related paragraphs are grouped which provides the necessary flow; one bullet per section of the text
2-74	2	2	5	2	8	There is no evidence to support this statement.. It is not piossible to measure the average surface temperature of the earth, which would involve monitoring temperature sensors randomly distributed over the entire surface. For this reason we cannot know if it is increasing. The Global Mean Temperature Anomaly Record is based on unrepresentative samples which change each year, are poorly standardized, is, based on multiply averaged means of daily maximum and minimum temperatures. It cannot claim to be truly representative of the mean temperature of the surface and it is subject to very large errors which cannot be entirely estimated. [VINCENT GRAY, NEW ZEALAND]	Taken into account; the observation network can be used to provide a robust estimate; there is no claim that we know the exact value.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-75	2	2	6	2	6	Oh, never, never doubt what nobody is sure aboujt (Graham, "Ruthless Rhymes":) [VINCENT GRAY, NEW ZEALAND]	Noted
2-76	2	2	10	2	11	Merely the opinioin of biased "experts". The only place where this has been investigated is in the USA where there are many stations. Elsewhere urban bias is inevitable [VINCENT GRAY, NEW ZEALAND]	Rejected; evidence supporting this statement is discussed in section 2.2.1.
2-77	2	2	13	2	15	Again mertely the opinion of biased "experts" [VINCENT GRAY, NEW ZEALAND]	Noted
2-78	2	2	17	2	19	Biased opinions.again. The;level;of uncertainties is not given. [VINCENT GRAY, NEW ZEALAND]	Taken into account; level of uncertainty added
2-79	2	2	21	2	23	Linear regression is an inadequate and irresponsible tool for judging changes in a climate time series, as it exaggerates the least reliable and most remote data. Also no estimates of inaccuracy are given. [VINCENT GRAY, NEW ZEALAND]	Rejected; choices for describing the trends are given in Box 2.2
2-80	2	2	22	3	25	Miscellaneous biased opinins of "presumed "experts" [VINCENT GRAY, NEW ZEALAND]	Noted
2-81	2	2	25	2	30	More biased opinions. Radiosonde measurements are even less representative than surface measurements. The increase in temperature they show can be related to the depression caused by volcanic eruptions at the beginning and the increased ENSO ocean osillations at the end. [VINCENT GRAY, NEW ZEALAND]	Taken into account; as explained in the introduction, this chapter does not attribute the changes to natural or human causes.
2-82	2	2	42	3	47	The different regions and time periods of the reported changes makes this hard to parse. Consistent time period(s) would be a big help. [Dian Seidel, USA]	Taken into account; a consistent set of time periods is chosen; see 2-1
2-83	2	2		35		2.3.4 Evapotranspiration including pan evaporation I have had my reservations about “Pan Evaporation”. I have seen tens of evaporation pans in many Arab countries and tens of them in Western, Eastern and Southern Africa. I have rarely seen a standard class A. pan . Many with dirty water and with the wood logs underneath the pan filled with soil. So I lost confidence in pan evaporation data. I believe the evaporation data from the Piche Tube in a standard Stevenson screen more reliable and gives a better relative measure of evaporation than the evaporation pan. [HUSSEIN ADAM, Sudan]	Noted
2-84	2	2		35		2.7.3 Storms: 2.7.4 Extratropical Storms: The increase in intense tropical cyclone activity in the North Atlantic since 1970 and the increase in the number and intensity of winter time intense extratropical cyclone system since the 1950s. should have been expected and forecasted. It is clearly related to the increase in saturation vapor pressure resulting from Global temperature increase. The following rough arithmetic calculations show the energy increase as a result of the increase of Absolute Humidity or density of water vapor. An increase of 1oC in atmospheric temperature gives an increase of 1mb if global temperature is 15Co The water vapor density will be increased by 1gm per m3 The surface area of the Globe is about 1.7×1014 m2. Assuming the depth of the active atmosphere is 0.6km, the volume of the active atmosphere is about 1017 m3 . The increase in water vapor content will be 1014kg. Assuming only 25% of that additional water vapor is condensed at any one point in time across the globe , the additional energy released will be 2.5 × 1013 kg × 2.45 Mjkg-1 which is about 6×1013 Mj (600 Billion Giga joules). The atmosphere sheds this huge additional energy resulting from global temperature increase in increased frequency of tropical cyclones accompanied by flooding and very high wind speeds . However there might come a day when the atmosphere will not be able to get rid of this huge energy increase and then the Globe may lose its equilibriurn !!! That is the real danger of climate change. [HUSSEIN ADAM, Sudan]	Noted
2-85	2	3	1	178	70	I find there is not much discussion on the recent work being done on the changes in south Asian monsoon circulation and rainfall. There are good papers coming out suggesting that south Asian monsoon is weakening.	Taken into account; added to 2-80 line 14 if appropriate, but note that monsoons are assessed in

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						I feel, a small discussion on this aspect may be included appropriately. The relevant references are Bingyi, Advances in Atmospheric Sciences Volume 22, Number 1, 21-29, DOI: 10.1007/BF02930866, Tianjun Zhou,Lixia Zhang, and Hongmei Li, GEOPHYSICAL RESEARCH LETTERS, VOL. 35, L16707, doi:10.1029/2008GL034881, 2008, Changes in global monsoon precipitation over the past 56 years, Bin Wang1 and Qinghua Ding (GRL), Is summer monsoon rainfall decreasing over India in the global warming era? Journal of geophysical research CV Naidu et al 2009, vol. 114, noD24, D24108.1-D24108.16] [Madhavan Nair RAJEEVAN, India]	ch14
2-86	2	3	1	178	70	Also, there are some results suggesting changes in the mean precipitation on regional basis using higher resolution data sets (for example, 28. Trends in the rainfall pattern over India, Guhathakurta, P, and M.Rajeevan, 2007, Int. J.Climatology, DOI 10.1002/joc.). A discussion on regional precipitation analysis may be more useful in addition to the results from the 5X 5 degree resolution data set. The results from 5 X5 data are not well matching with the high resolution analysis results. [Madhavan Nair RAJEEVAN, India]	Taken into account; added to section 2.3.1.2 if appropriate
2-87	2	3	1			The Executive Summary, presented as a series of bullets rather than in paragraph form, is choppy. A narrative form would be easier to digest. In my view, there are too many bullets here, and it is unclear how they are organized. (Some sentences are preceeded with bullet symbols, others are not, but it is not the case that the bullet sentences are overview statements for the non-bullet sentences that follow.) There are 28 separate items, and only a handful are clearly written. Suggested re-wording for some is given below. Eventually, some of these points will be "elevated" to AR5 WGI Technical Summary and/or Summary for Policymakers, or to the overall AR5 SPM, but it is not at all clear from this draft which are the key points to highlight. [Dian Seidel, USA]	Taken into account; the motivation for the bullets (one for each section of the text) has been clarified
2-88	2	3	5	3	5	superscript 'th' [Peter Burt, UK]	Editorial
2-89	2	3	5	3	5	"Nineteenth Century" in line 5 is spelled with a number ("19th Century"), whereas later in the page the century number is spelled out with letters (lines 18 and 26, for example). Should century numbers be consistently written as letters or numbers? [Richard Heim, U.S.A.]	Editorial
2-90	2	3	5	3	6	Combine the first two sentences into a single statement [Timothy Carter, Finland]	Rejected, but sentences changed to better explain what is virtually certain.
2-91	2	3	5	3	6	Such a purely qualitative statement, without any order of magnitude is not policy relevant, even if it is virtually certain. A less certain but quantified statement such as the one on lines 21 to 23, same page would be more useful. Adding,e.g. at the end of line 8, "with trend estimates ranging from .22 to .35 ° per decade" (from table2.2) [Michel Petit, France]	Rejected; too many quantified statements about the warming are to be avoided. We have chosen to provide the figures for the combined temperatures only.
2-92	2	3	5	3	6	I think the sentence about the temperature trends and about the confidence level belong together. The trend only has meaning when it has its associated uncertainty. So why not simply merge these two sentence (i.e. "It is virtually certain that…"). That would also be consistent with the rest of the exec summary. [Drew Shindell, USA]	Taken into account; sentences changed to better explain what is virtually certain.
2-93	2	3	5	3	8	To be honest, the trend has been minuscule in the past 15 years, "since the cool period of the 1970's there was a rise to 1998 and since then a leveling off" is a more transparent and honest representation of the facts. [John Christy, USA]	Taken into account; sentence added in the text on the warming trend since 1998; not lifted to the summary because many more short period trends can be distinguished without clear meaning.
2-94	2	3	5	86	51	There is alarming inconsistency with the convention used for 20th or 21st Century: 20th, 20th, century, Century, twentieth, Twentieth, mid 20th, mid-twentieth. Chapter 1, and the other Chapters which I have looked at, generally use 20th Century or 21st Century. I suggest the terminology in this Chapter is standardised to that form (number as a number, exponential 'th', 'st' and Century with a capital 'C'). I have not flagged all these instances in the following comments. [Peter Burt, UK]	Editorial
2-95	2	3	6			it seems like the authors are not certain about their findings. Use of certain words like virtually certain makes the general audience doubtful about the findings. [Shouraseni Roy, USA]	see 2-4
2-96	2	3	6			A link to the defintions of virtually certain and similar words would be a good idea here to help people who either skipped Chapter 1 or missed the import of this phrasing.Once a chapter is probably sufficient. [Larry	Accepted

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						Thomason, United States of America]	
2-97	2	3	7	3	7	What does "substantial heritage" imply in this context [Timothy Carter, Finland]	Taken into account; words removed from text
2-98	2	3	7	3	7	The term "data products" should be replaced by the more robust term "data records" where appropriate throughout the Chapter. "Products" implies something that's been fabricated, developed or manipulated while "records" implies measurements. [Dale Hurst, United States of America]	Accepted; CDR is also used for derived products in satellite community
2-99	2	3	7	3	7	While it is common practice to use the term "surface temperature" to denote "surface air temperature", in this example "land surface temperature" is doubly confusing. The could be remedied by equating the term "surface temperature" with "surface air temperature" at the outset, however, there are instances in this chapter and elsewhere where "surface temperature" actually does mean the temperature of the land surface, not air temperature. This needs to be reconciled. [George Kiladis, USA]	Taken into account; always full names used in SOD
2-100	2	3	7	3	7	"substantial heritage" is likely to cause confusion over exactly what you mean. Suggest you find a more precise way to express what you mean. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account; words removed from text
2-101	2	3	10	3	11	This claim is unlikely in my opinion, see my comments on page 13, line 21-36. The results there are partly due to UHI. [Marcel Crok, The Netherlands]	Rejected; see evidence in section 2.2.1.2
2-102	2	3	10	3	11	I would suggest to add one sentence to state the regional difference of urban heat island effect on temperature [Xuemei Wang, China]	Accepted; sentence based on p2-14 line 22-23 added to ES
2-103	2	3	10	3	15	The papers I have publish contradict this statement - in past 30 years and more there has been a rise in Tmin, but not Tmax which has affected the Tmean trend by more than 10 percent (and not greenhouse-caused because greenhouse forcing affects both Tmax and TMin.) I have just finished digitizing the complete Uganda data set and find the same thing - no warming in TMax, lots of warming in TMin. [John Christy, USA]	Rejected; the fact that Tmin has risen most can be related to greenhouse warming too (cloudiness effects).
2-104	2	3	10			can this sentence be more definitive? This chapter starts with a very speculative tone making the readers feel unsure about th [Shouraseni Roy, USA]	Noted; unclear what the comment suggests
2-105	2	3	13	3	15	This statement does not comment on the direction of these changes [Timothy Carter, Finland]	Accepted; "the reported decrease" added to the text
2-106	2	3	14	3	15	It would be helpful to be more precise about the types of biases here. The statement could be rephrased to read "...biases that affect maximum temperatures differently than minimum temperatures" [Timothy Carter, Finland]	Rejected; the ES has no space for these details which can be found in 2.2.1.3
2-107	2	3	17	3	18	Such a purely qualitative statement, without any ordeer of magnitude is not policy relevant, even if it is with very high confidence. A less certain but quantified statement such as the one on lines 21 to 23, same page would be more useful [Michel Petit, France]	Rejected; see 2-91
2-108	2	3	17	3	19	It might be better to have two separate statements for SST: one referring to the long series based on in situ data and a second on the more recent period where independent satellite estimates are available. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected; no reason provided as to why.
2-109	2	3	17	3	19	"With very high confidence" is a dangling modifier (SSTs didn't increase with confidence). The second sentence doesn't report any particular result. [Dian Seidel, USA]	Accepted; phrase has been added to the second sentence rather than the first.
2-110	2	3	18	3	18	Here and throughout the Chapter: is it "Twentieth Century", "twentieth Century" or "20th Century" ? [Dale Hurst, United States of America]	Editorial
2-111	2	3	21	3	21	The temperature has not gone up linearly, so don't use a linear trend to express the change. This leads to wrong results that will confuse people, such as those presented here. Comparing the AR4 and AR5 values implies the the extra data from 2006-2010 has raised the warming since 1901 by 0.1 degC, but has made no difference to the warming since 1979. How can it be that the same extra 5 years of data make one warmer and not the other? The obvious answer is that it is because the linear trend is a poor fit to one or both of these periods. I know you have a box on this and I make further comments about that later, but the box doesn't provide a basis for choosing a linear trend it just points out that there are different ways to do it -- and then you choose the wrong way. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	See 2-1

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2-112	2	3	21	3	23	Though this may be due to rounding, or to the method of trend fitting, it is potentially misleading to point to a similar increase over 1979-2010 compared to 1979-2005 reported in the AR4, but an enhanced increase over the longer periods between AR4 and AR5. The recent, shorter-term record also suggests no warming between 2005-2010. Is this something that should be highlighted, or is it simply the case that any warming has been less than 0.1 degC? [Timothy Carter, Finland]	Taken into account; see 2-1
2-113	2	3	21	3	23	Numbers here are confusing – they suggest that the change from 2005- 2010 was both 0 and 0.1. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account; see 2-1
2-114	2	3	21	3	23	It seems odd that adding 5 yr to a short data record does not change its trend but adding the same 5 yr to a much longer record does. [Dian Seidel, USA]	Taken into account; see 2-1
2-115	2	3	21			written “The global combined land and ocean temperature data show an increase of about 0.8°C over the period …..” it is necessary to write that the text is talking about air temperature in the following form “The global combined land and ocean temperature data show an increase in surface air temperature of about 0.8°C over the period …” [José Daniel Pabón-Caicedo, Colombia]	Rejected; the suggested alternative is confusing, because the global average is actually a combination of surface air temperature over land and sea surface temperature for the oceans.
2-116	2	3	22	3	22	"estimated" by a linear trend indicates subjectivity. "described" by a linear trend is more objective [Dale Hurst, United States of America]	Accepted
2-117	2	3	22			the description in the parenthesis is a little unclear, it is not clear if the start of the 'comparable' periods in AR4 were the same as now, or if they were also set back by 5 years. Maybe something like the following could improve it: (note that that the increases reported in the AR4 for 1901--2005 and 1979--2005 were 0.7C and 0.5C, respectively) [if they really started in e.g 1979, and not in 1974 [Martin Vollmer, Switzerland]	Accepted
2-118	2	3	23	3	23	Add "respectively": comparable values for the period until 2005 reported in the AR4 are about 0.7°C and 0.5°C, respectively). [Richard Heim, U.S.A.]	Accepted
2-119	2	3	25	3	25	"Based upon multiple independent analyses of measurements from radiosondes and satellite sensors …" [Dale Hurst, United States of America]	Accepted
2-120	2	3	25	3	26	By how much ? See comment on lines 17-18, same page [Michel Petit, France]	Rejected; see 2-91
2-121	2	3	25	3	30	I think the evidence is much stronger than reported here from multiple lines of evidence that tropical tropospheric temps do not match model-generated expectations. It's one thing to say tropical tropospheric trends are similar to surface, but it is more important to say that observed trends do not match model amplification values - or at least speak to that point indirectly. Perhaps that is the best way to go here - in terms of amplification factor or as I called it in my publications "scaling factor" (which also gets around the issue of differences in interannual variations beteween obs and models.) i.e. "Observations indicate the temperature trend of the tropical troposphere does not indicate an amplification of currently measured surface trends." There are lots of papers to back this up. [John Christy, USA]	Taken into account, but without mentioning the "amplification". This belongs in the model evaluation chapter.
2-122	2	3	26	3	26	Should "mid-twentieth Century" be capitalized to be consistent with other century references such as "Twentieth Century" from line 18? [Richard Heim, U.S.A.]	Editorial
2-123	2	3	27	3	36	All these figures are averages without any distribution curces or estimates of confidence limits [VINCENT GRAY, NEW ZEALAND]	Noted, but there are no figures in these lines
2-124	2	3	28	3	30	The phrase "include the potential for less warming or greater warming than that reported" is somewhat confusing and, for an executive summary at least, should be less obfuscating. I would recommend changing the wording to be "warming rates are uncertain" or similar. [Jeffrey Taylor, United States of America]	Taken into account; the phrase "has large margins" is used
2-125	2	3	28	3	31	I expect that the authors are attempting to state that the estimated rate of tropospheric warming could be different than that reported at the surface; “rates include the potential for less warming or greater warming” is a protracted way of saying this since “less or greater” includes all the possible deviations. [David Sauchyn, Canada]	Taken into account; the phrase "has large margins" is used; the suggested "could be different" is even more general
2-126	2	3	29	3	30	The equivalent statement on Page 30 is simpler and clearer. [Melissa Free, USA]	Taken into accout; phrase is made more simple, but the point is that the potential for greater warming is left

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							open
2-127	2	3	32	3	33	By how much ? See comment on lines 17-18, same page [Michel Petit, France]	Rejected; see 2-91
2-128	2	3	32	3	34	This statement should clarify that it applies to the lower stratosphere, not the middle or upper stratosphere. [Dian Seidel, USA]	Rejected; cooling over the full layer but vertical structure very uncertain
2-129	2	3	32	3	34	I personally found the section on stratospheric temperature trends perfunctory and not in enough detail to justify the "low confidence [Keith Shine, UK]	Noted
2-130	2	3	32	3	34	A summary of the long-term stratospheric observations of water vapour and ozone is missing and should be included [Michiel van Weele, The Netherlands]	Noted
2-131	2	3	36	2	54	Finding the global mean of precipitation seems like it is of much less interest than in how it is changing spatially. The amount of energy moved by way of latent heat releases is very large, and later chapters suggest high uncertainty in regional precipitation predictions...leaving in my mind substantial uncertainty in temperature predictions. What is clear about regional patterns. How good are our estimates in mountains, for instance, and how much precipitation gaging do we have there? Rather than zonal averages, can we divide the precipitation-scape into like units of land in physical relationship to oceans ... e.g. the eastern U.S. has more in common in with landscapes in the western Pacific than it does with either the western U.S. (on a shared continent) or Europe (with a shared ocean). [Charles Luce, United States of America]	Taken into account; global precipitation is less meaningful than global temperature, but providing detailed trends for regions is not possible in the Summary section
2-132	2	3	36	3	37	These three sentences seem to be mutually contradictory; I am not able to extract a clear message regarding precipitation changes. Also, the period of analysis is not specified. [Dian Seidel, USA]	Accepted; text changed
2-133	2	3	36	3	40	Does it make sense to mention the possible increase in global precipitation over land areas if confidence in this assessment is low? In the IPCC SREX chapter 3, we decided not to provide assessments of direction of changes in cases with low confidence (see Section 3.1.5 of IPCC SREX). [Sonia Seneviratne, Switzerland]	Accepted; text changed
2-134	2	3	36	3	40	This was one of those bullets that I understood less the more I read it. As I understand it, the second sentence is directly contradicting the first sentence, and so the confidence is actually zero? [Keith Shine, UK]	Accepted; text changed
2-135	2	3	36	3	40	This paragraph seems contradictory. Has global precipitation over land increased or not? Does data incompleteness allow you to say that 'Global precipitation over land areas has increased' - my reading of the material presented in section 2.3.1 is that there are insufficient data to draw such a strong conclusion. [Robert Waterland, United States of America]	Accepted; text changed
2-136	2	3	37	3	37	I prefer consistency in the centennial designations. "early to mid 20th Century" in this line and "mid-twentieth Century" in line 33 are inconsistent. Either spell out the century number everywhere, or use numbers for the century everywhere. [Richard Heim, U.S.A.]	Editorial
2-137	2	3	38	4	8	Futher examples of the opinions of supposed experts [VINCENT GRAY, NEW ZEALAND]	Noted
2-138	2	3	39	3	39	This discusses estimates of land precipitation "filled in using a reconstruction method". Would it be more informative to replace those words by "are estimated by spatial interpolation"? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Rejected; the reconstruction method involves more than statistical interpolation alone
2-139	2	3	42	3		I don´t sure of this affirmation: ¨Precipitation in the tropics has likely increased over the last decade reversing the drying trend that occurred from the mid-1970s to mid-1990s reported in the AR4 ¨. In Cuba and the Caribbean situation is different, with prolongated periods of droughts or precipitation down of months and annual average [CRISTOBAL FELIX DIAZ MOREJON, Cuba]	Noted; this summary statement refers to the tropics on average; trends will differ locally
2-140	2	3	43	3	43	latitudes misspelled as "latititudes". [Benjamin R. Miller, United States of America]	Accepted
2-141	2	3	45	3	45	I prefer consistency in the centennial designations. "early 20th Century" in this line and "mid-twentieth Century" in line 33 are inconsistent. [Richard Heim, U.S.A.]	See 2-136
2-142	2	3	45	3	45	is there "much uncertainty in the results" or "much uncertainty in the data" ? [Dale Hurst, United States of America]	Accepted; "results" changed into "data records"

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2-143	2	3	46	4	46	"to define a long-term temporal change" [Dale Hurst, United States of America]	Accepted
2-144	2	3	49	3	51	In this sentence it is compared the number of snowfalls in most regions with the snowfall (=amount of snowfall?) in Antarctica, but the two quantities (snowfall and amount of snowfall) are different - I suggest to separate the two part of sentence. In addition, the increase of snowfall in Antarctica is likely, or has confidence medium, low, high...? [Claudio Cassardo, Italy]	Taken into account. The text refers to changes in snow fall events only. Reference to Chapter 4 is added for changes in snow cover
2-145	2	3	49	3	51	My recent paper, I suppose, does not contradict this, but it shows that a critical region (for humans) of snowfall in California has shown no change over the past 133 and 50 years (Christy, 2012, J. HydorMet) where temperatures have not really changed either (Christy et al. 2006.) Thus the statement here begs for the "rest of the story" - i.e. there are many places which are receiving normal snow (as the NH snowcover maps show.) [John Christy, USA]	Rejected; as stated most regions with warming show decreasing snowfall.
2-146	2	3	49	3	51	A notable exception seems to be Greenland where snow cover is increasing in the summer, http://www.climate4you.com/images/GreenlandSnowCoverSince1966.gif (data Rutgers University Global Snow Laboratory) [Marcel Crok, The Netherlands]	Taken into account. Reference to Ch4 added where snow cover is assessed
2-147	2	3	49	3	51	Would it be reasonable to mention that the connection between snow trends and temperature trends is expected to depend on how close winter temperatures are to 0 deg. C? [Dian Seidel, USA]	Accepted; reference to the fact that temperatures in Antarctica remain below zero
2-148	2	3	49	3	51	Resume from the above comment on snowfall/snow depth changes in Russia should be placed there too. [Andrey Shmakin, Russia]	Noted; comment not understandable
2-149	2	3	49	3	51	Resume from the above comment on snowfall/snow depth changes in Russia should be placed there too. [Andrey Shmakin, Russia]	Noted; comment not understandable
2-150	2	3	49	3	51	It is a mistake to conclude that "Antarctica is the exception where increased snowfall is occurring with increased temperature" (see detailed comments 7, 8, 9, and 10 below) [Zhaomin Wang, UK]	Noted; see responses below
2-151	2	3	50	3	50	insert comma after 'exception' [Peter Burt, UK]	Accepted
2-152	2	3	50	3	50	Have "increased" or "increasing" winter temperatures been observed" [Dale Hurst, United States of America]	Taken into account; indicated that warming in Antarctica has been observed
2-153	2	3	50	3	50	What is the confidence of the Antarctic snowfall and temperature signal? [George Kiladis, USA]	Taken into account. Confidence statement added.
2-154	2	3	53	3	51	Why is this true? Are the analyses new (not available for AR4)? Change verb tense to simple past ("has increased" to "increased"). [Dian Seidel, USA]	Taken into account; newly assembled observational records mentioned.
2-155	2	3	53	3	54	Does this observation belong in this chapter or in WG II chapter 18? The finding reported here, by contradicting the AR4, should probably contain more detail. Does it imply that runoff has decreased, or that there is no discernible trend? Are there regional differences in the trends? How meaningful is global runoff as a measure anyway? The interest in runoff, from the point of view of the global climate system and of potential impacts is likely to be regional (e.g. freshwater discharge into the oceans; water supply; [Timothy Carter, Finland]	Taken into account. Sentence on lack of trend added
2-156	2	3	53	3	54	It is commendable that the authors recall a claim made in AR4. In earlier reports IPCC seldom made such admissions. So it's really refreshing to see such a frank admission that the advancing science makes an earlier claim not valid anymore. [Marcel Crok, The Netherlands]	Noted
2-157	2	3	53	3	54	This has to be demostrated: ¨ The most recent and most comprehensive analyses of river runoff do not support the AR4 conclusion that global runoff has increased during the 20th Century. ¨ [CRISTOBAL FELIX DIAZ MOREJON, Cuba]	Taken into account. See section 2.3.2
2-158	2	3	54	3	54	I prefer consistency in the centennial designations. "20th Century" in this line and "mid-twentieth Century" in line 33 are inconsistent. Either spell out the century number everywhere, or use numbers for the century everywhere. This is the last instance where I will point this out, by line number, but the comment applies throughout the report. [Richard Heim, U.S.A.]	Editorial

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2-159	2	3	54	3	54	Add a sentence "although the increase in the major Eurasian Arctic river runoff is statistically significant based on the gauged data". [Xiangdong Zhang, United States of America]	Rejected. Detailed assessment given in 2.3.2
2-160	2	3	56	3	56	Is "moistening" a preferred term to "humidity"? [Timothy Carter, Finland]	Noted. Both terms are used.
2-161	2	3	56	3	56	Add "near the surface" to distinguish this from upper-air moisture that is discussed in later sentences. [Melissa Free, USA]	Accepted
2-162	2	3	56	4	6	This discussion of atmospheric humidity changes is unnecessarily complex, uses too many different water vapor variables, and is unclear regarding the time periods of the reported changes. [Dian Seidel, USA]	Rejected. Time periods have been specified and wording reflects robust findings
2-163	2	3	57	4	2	This should perhaps specifically mention the surface when talking about the recent abatement in rising atmospheric moisture content/decreasing relative humidity over land. I don't think this is seen in the troposphere/total column water vapour. [Kate Willett, UK]	Accepted
2-164	2	3		4		Excellent summary hitting crucial point in an appropriate level of detail. [Larry Thomason, United States of America]	Noted
2-165	2	3		5		Very little is said about river runoff and in particular, information on regional variability is missing (as compared to what is included on trends in e.g. precipitation and temperature). Suggest to include for Europe: “A regionally coherent picture of annual streamflow trends has been found for Europe from 1962-2004, with negative trends (towards drier conditions) in south-eastern Europe, and generally positive trends (towards wetter conditions) elsewhere (Stahl et al., 2010).” Overall there ought to be a better balance between the emphasis given to the different components of the hydrological cycle (as defined in on page 2-31, line 37-38). [Lena M. Tallaksen, Norway]	Rejected. Regional and in particular sub-regional information is to be avoided in this summary.
2-166	2	3		5		This also holds for the paragraph on extreme events (refer comments on trends in high and low flow given in connection with Section 2.7.2) [Lena M. Tallaksen, Norway]	Rejected. See 2-165
2-167	2	4	1	4	1	This' is a bit vague. Suggest not to use 'this' here. [Zhaomin Wang, UK]	Accepted
2-168	2	4	2	4	2	delete comma after GPS [Peter Burt, UK]	Accepted
2-169	2	4	2	4	4	The statement: "observations from radiosnde, GPS, and satellite measurements indicate increases in troposheric water vapour at large spatial scales" is ambiguous because it is unclear whether this statement addresses absolute moistening, tropospheric specific humidity or troposheric relative humidity. It is suggested to limit the terms used in the whole paragraph to the necessary minimum and to define those terms in the glossary and explain the relationship to other expressions that might have been used in underlying publications. [Klaus Radunsky, Austria]	Rejected. Subsequent sentences make clear that discussion is on specific humidity first which is followed by relative humidity information.
2-170	2	4	2	4	6	It seems the IPCC want the AR5 to be accessible to a “wider community”.... This paragraph will be difficult to understand for a lay reader who doesn't know the difference between absolute and relative humidity. Because it's the summary, I would add something. Suggestion: “There is no contradiction between increase of absolute humidity and decrease of RH as, at constant absolute humidity, if the temperature increases, the RH decreases.” [Francois DANIS, France]	Taken into account. Wording includes relation with temperature increase. For more details readers will need to go to the glossary.
2-171	2	4	5	4	5	helpful to quantify 'large spatial scales' [Peter Burt, UK]	Accepted
2-172	2	4	10	2	11	This phrasing seems strange, implying satellite data are not observations. I'd suggest rephrasing. [Drew Shindell, USA]	Accepted
2-173	2	4	10	4	12	I fear the sentence "What trends do exist … data sets." will not be understood by most readers. [Dian Seidel, USA]	Accepted. Sentence deleted
2-174	2	4	13	4	14	Doesn't this belong in Chapter 8 on radiative forcing? [Timothy Carter, Finland]	Accepted. Only the observations of GHG belong in Ch2
2-175	2	4	13	4	14	Delete – comments about RF do not belong in observations chapter. P 6 says “RF is assessed in ch 8” [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Accepted. Only the observations of GHG belong in Ch2

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2-176	2	4	13	4	14	Why state this in the form of an attribution, rather than simply stating that both the atmospheric concentrations and radiative forcings increased? [Dian Seidel, USA]	Taken into account. See 2-174
2-177	2	4	13	4	14	Sentence should be rewritten as it can be misinterpreted (i.e. increases in non-CO2 gases are less certain). I think what it meant here is: Between 2005 to 2010 the CO2 increase contributed most to the increase in the radiative forcing of LLGHGs. Add this sentence and leave out 'especially CO2' in the first sentence. [Michiel van Weele, The Netherlands]	Rejected. See 2-174
2-178	2	4	16			to describe a background site as "undisturbed" seems inappropriate. The fact that there is a trend in ozone there means that it is indeed disturbed! I would suggest "remote background", perhaps. [Stephen Montzka, USA]	Accepted
2-179	2	4	17	4	18	This second sentence makes no sense, grammatically or logically. [Dian Seidel, USA]	Accepted
2-180	2	4	20	4	20	Aerosol Optical Depth → aerosol optical depth [Peter Burt, UK]	See 2-181
2-181	2	4	20	4	20	Need to define AOD: Satellite datasets indicate a continuing decrease of Aerosol Optical Depth (AOD) in the US, Europe, and Japan, [Richard Heim, U.S.A.]	Taken into account. Technical term avoided and the word aerosols used instead.
2-182	2	4	20	4	20	"Aerosol Optical Depth in the US" should be replaced by "Aerosol Optical Depth over the US" [Dale Hurst, United States of America]	Taken into account. See 2-181
2-183	2	4	20	4	22	I am not sure that a non-expert reader will be able to mentally convert immediately the AOD in a physical concept (e.g. concentration, visibility, ...), thus just a short explanation would be helpful. [Claudio Cassardo, Italy]	Taken into account. See 2-181
2-184	2	4	20	4	22	Most other paragraphs in this page provide some level of confidence (likely, virtually certain, etc.) except this paragraph. Is there a level of condiference that can be provided? [Norman Loeb, United States of America]	Accepted
2-185	2	4	20	4	22	About the sentence: "...Satellite datasets indicate a continuing decrease of Aerosol Optical Depth in the US, Europe, and Japan, and a continuing increase of AOD over Eastern and Southern Asia since the 1980s, which is consistent with long-term surface aerosol observations over North America and Europe." The consistency of the observations over North America and Europe can not be directly associated with the AOD behavior over Asia. Please, verify and eventually modify the sentence. [Rubén D Piacentini, Argentina]	Accepted
2-186	2	4	20	4	22	This makes no sense either. How can changes observed over Asia by satellite be consistent with (or inconsistent with) changes observed in situ over N. America and Europe? Is the consistency spatial or between the two observing methods? What period is meant by "long-term"? [Dian Seidel, USA]	Taken into account. Time period and consistency explained
2-187	2	4	20	4	22	Confidence level for long-term trend of aerosols should be provided, like other parameters. [Kaicun Wang, China]	Accepted
2-188	2	4	24	4	25	Is it really only "likely" that ozone and aerosols cause regional patterns of radiative forcing? Perhaps you mean it is likely that they have caused "substantial" (however that might be defined) region forcing? The statement would be more useful if expressed to indicate how strong an effect is likely. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Here the changes in patterns is highlighted.
2-189	2	4	24	4	25	Make clear whether you mean changes in stratospheric or tropospheric ozone, and also for aerosol. [Karen Rosenlof, United States of America]	Accepted
2-190	2	4	24	4	25	Could this possibly be vaguer? What direction are the changes, over what periods, and of what magnitude (compared with other aspects of radiative forcing)? [Dian Seidel, USA]	Rejected. This is for Ch8 to assess and a reference to this chapter has been added.
2-191	2	4	24	4	25	I wonder why this is only 'likely'? Is this not clearly the case, even if the magnitude is uncertain? The statement is so bland with no time or magnitude that it seems to be simply saying ozone and aerosols are inhomogeneous and cause forcing, so is that not much more clear than implied by 'likely'? [Drew Shindell, USA]	text re-written
2-192	2	4	24	4	25	Here ozone should read 'tropopsheric ozone' (to distinguish from stratospheric ozone) [Michiel van Weele, The	Accepted

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						Netherlands]	
2-193	2	4	27	4	26	Need to be more explained : ¨The quantification of the global mean energy balance as presented in earlier IPCC assessment reports requires substantial revision ¨. [CRISTOBAL FELIX DIAZ MOREJON, Cuba]	Rejected. Subsequent sentences give some clues as to the required changes and more details are in section 2.5
2-194	2	4	27	4	30	It would be helpful to be specific here about the sign of the fluxes. Suggest something like "The quantification of the global mean energy balance as presented in earlier IPCC assessment reports has undergone substantial revision. This revision affects a number of components, and in particular increases the magnitude of both downward thermal radiation and (upward) latent heat flux ....." [Elizabeth Kent, England]	Accepted
2-195	2	4	27	4	30	Re. the substantial revision of the global mean energy balance: make clear if the revision is due to (a) better data; (b) better models; or (c) a change from the pre-2005 state to the present state. If the latter, perhaps better to state that the energy balance has changed rather than has been revised. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Statement removed because not about changes over time
2-196	2	4	27	4	30	The way this is worded, it could be interpreted as a major "bombshell" statement. Is that the authors' intention? It suggests our understanding of the global energy balance was seriously flawed, which could make one question understanding of the whole greenhouse effect. Consider adding language to clarify whether the changed understanding has implications regarding our confidence in the greenhouse effect. Also, the adjective "downward" could be read as a modifier of "latent heat flux". Is that the intention? If not, simply put the latent heat flux before downward thermal radiation. [Dian Seidel, USA]	Taken into account. Text added which explains that confidence in our understanding of the greenhouse effect has not changed.
2-197	2	4	27	4	30	In the discussion of the quantification of earlier analyses of global mean energy balance, it is unclear how earlier IPCC assessment report treatment meshes with the current analysis. This sentence could be re-written to be more specific. [Jeffrey Taylor, United States of America]	Taken into account. See 2-196
2-198	2	4	27	4	42	The findings related to the global mean energy balance as well as to the decadal changes in surface solar radiation are important. However, it seems necessary to inform the reader about the imüplications of those findings with respect to policy relevant topics such as attribution. [Klaus Radunsky, Austria]	Rejected. Attribution in Chapter 10
2-199	2	4	29	4	29	Need to add "upward" before "latent heat flux" as it currently implies it is downward. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-200	2	4	33	4	33	Consider changing "expanded" to "lengthened". [Dian Seidel, USA]	Accepted
2-201	2	4	38	4	38	Delete "At the surface", which is redundant. [Dian Seidel, USA]	Accepted
2-202	2	4	41	4	42	"for increasing downward thermal and surface net radiation" is ambiguous. If both these fluxes (downward thermal and net) are at the surface, reword to make clear they are both at the surface not just the latter. If the former is not at the surface, state this and explain what level(s) it is increasing at. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Surface moved in sentence.
2-203	2	4	41	4	42	The phrase "also medium confidence indications" is a bit awkward -- please reword. [Jeffrey Taylor, United States of America]	Accepted
2-204	2	4	41			How do you define the difference between high medium and low thresholds? [Shouraseni Roy, USA]	Uncertainty language
2-205	2	4	44	4	52	Given the acknowledment of decadal variability in lines 44-48, and given the uncertainties outlined in the chapter regarding poleward migration of circulation features, what justifies "it is likely" is line 50? [Dian Seidel, USA]	Uncertainty language
2-206	2	4	44	4	52	Here a reference should be made to the simultaneous observed changes in the stratosphere given the attribution studies for many of these phenomena to stratospheric ozone depletion to some degree (Son et al., Science, 2008; Son et al., GRL, 2009; Kang et al., Science, 2011; Sigmond et al., GRL, 2011). Text suggestions: "Nevertheless, it is likely that, in a zonal mean sense, circulation features have moved poleward (widening of the tropical belt, poleward shift of storm tracks and jet streams, contraction of the polar vortex) since the 1970s and at least part of these changes in the Southern Hemisphere have been related to	Taken into account. Reference to Ch10 added without making an attribution statement here

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						stratospheric ozone depletion." [Michiel van Weele, The Netherlands]	
2-207	2	4	48	4	48	Magnitude of Walker circulation weakening since early 20th C has diminished in recent years (i.e., e.g., trend over 1901-2000 is larger than trend over 1901-2010) but the overall trend i.e. from 1901-2010 is still "weakening" (see Table 2.14). Hence term "reversed" as used here seems misleading. Perhaps "changes since the 1990s have largely offset trends evident in earlier records ". The fact remains that the Walker circulation weakened during the 20th Century, and suggest that this be stated so there is no confusion. [Scott Power, Australia]	Accepted
2-208	2	4	51	4	51	Change "…poleward shift of storm tracks and jet streams…" to "…poleward shift of storm tracks, jet streams, and centers of action of variability…" [Xiangdong Zhang, United States of America]	Rejected. No need to add additional technical term in sentence starting with e.g.
2-209	2	4	51			"contraction of the polar vortex"... I'm surprised as I thought it was weakening but not contraction.... I should read on. [Francois DANIS, France]	Noted
2-210	2	4	54	4	55	Mention the orders of magnitude provided on th last lines of pge 76 and ths first lines of page 77 [Michel Petit, France]	Accepted
2-211	2	4	54	4	56	For clarity, I would change this sentence, "It is very likely that the overall number of cold days and nights has decreased and the overall number of warm days and nights on the global scale has increased since 1950.", to read: "It is very likely that the overall number of cold days and nights has decreased and the overall number of warm days and nights has increased on the global scale since 1950." [Richard Heim, U.S.A.]	Accepted
2-212	2	4	54	4	56	There seems to be considerable uncertainty in the claim that recent analyses of extreme events generally support the AR4 conclusions by updating data. The AR4 conclusions were based, to a large extent, on 13 regional workshops conducted all over the globe. The regions were identified in AR4, Chapter 3, pp. 300. The difficulty of updating the data series of climate extreme indices was discussed several years ago by the Expert Team on Climate Change Detection and Indices (WMO, 2007). Referring to these workshops, this is what they said: "These workshops expanded from just gaining access to data, but also to developing analysis packages so indices were calculated in consistent ways across regions. Regarding to extremes, these data sets have given AR4 much to say than before (through a number of regional papers and one at the global-scale), but all need to be repeated and expanded for AR5 as there is still no way of simply updating extreme indices time series." There is no evidence to suggest that these workshops were repeated and expanded. Thus, the results of the global-scale paper referred to above, Alexander et al. (2006), have not been updated to include data after 2003 and are simply reproduced from AR4 in a different format (See Figure 2.41, WGI, AR5 FOD pp. 173). There is therefore a need to more clearly indicate how the results of previous studies supporting the AR4 conclusions were updated. Alexander, L.V. and Coauthors. 2006. Global observed changes in daily climate extremes of temperature and precipitation, J. Geophys. Res. 111, D05109, doi:10.1029/2005 JD006290. WMO, 2007. Joint CCL/CLIVAR/ Expert Team on Climate Change Detection and Indices. WCDMP-No. 64. WMO-TD No. 1402. ICPO Publication Series No. 115. Geneva, Switzerland. [Reynold Stone, Trinidad and Tobago]	Taken into account. The papers referenced in Section 2.7 indicate which updates have been included. The sentence here specifies that trends are for the period 1951-2003
2-213	2	4	54	5	13	These bulleted paragraphs for extremes would have greater impact if they appeared with other similarly-themed paragraphs instead of on their own at the end of this list. For example, move temperature extremes with the earlier bullets for temperature; precipitation extremes with the earlier bullets for precipitation. [Dale Hurst, United States of America]	Rejected. The underlying evidence on changes in extremes is in a separate section too.
2-214	2	4	56	5	1	The extention of this conclusion for large parts of Europe does not agree with the conclusions from the SREX report for the same region. In SREX it was concluded that there is medium confidence about increases in warm spells/heat waves for the regions West-Central Europe and North Europe and Europe as a whole (see for an overview Table 3.2 of chapter three of the SREX report and the referred references and underlying text in this SREX chapter). So, it seems a bit strange that the confidence has changed into high confidence with a	Accepted. Europe removed from summary because not different from other regions.

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						likely positive trend, given that in SREX references up to 2011 were included. Moreover the underlying text on which this conclusion is based is given on page 78 lines 37 - 39 of this chapter. However, this text does not provide an argumentation for the high confidence, nor the likely statement. It is also peculiar why there are no references to the same studies as in SREX. One could also refer to the SREX work, unless new studies merit a change in the confidence level, for which I do not see any evidence in the current text. [Bram (Abraham) Bregman, Netherlands]	
2-215	2	4	57	5	1	Add a comma after "20th Century", and clarify "this is the case". Is it the medium confidence? Is it the increase in length? The increase in number? [Dian Seidel, USA]	See 2-214
2-216	2	5	3	5	3	superscript 'th' [Peter Burt, UK]	Editorial
2-217	2	5	3	5	5	I have great doubts about this claim. The most complete analysis I have seen is one of Demetris Koutsoyiannis for the EGU 2011 conference. Unfortunately there is no peer reviewed publication yet. The analysis is available at http://itia.ntua.gr/getfile/1124/2/documents/2011EGU_DailyRainMaxima_Pres.pdf They analysed over 3000 time series with at least 100 years of data. Especially in the alleged anthropgenic era (since 1970) there is no trend at all. This is really the most global picture we have right now in my opinion. [Marcel Crok, The Netherlands]	Rejected. The peer reviewed literature indicates that the increase since 1951 is robust (year added to sentence). Also, the one in 20 events discussed here provide more robust statistics of change than the annual maxima analysed by Koutsoyiannis
2-218	2	5	4	5	5	About the sentence: " Confidence is highest for North America where the most consistent trends towards heavier precipitation events are found.", please explain if you consider all countries of North America (USA, Canadá and México), or only USA. [Rubén D Piacentini, Argentina]	Yes, all countries
2-219	2	5	7	5	8	How are floods defined, and should this information appear in this chapter or in WG II? [Timothy Carter, Finland]	Taken into account. See Section 2.7.2. Observations of flooding are part of this chapter.
2-220	2	5	7	5	8	Precipitation, and especially heavy precipitation, has a substantial convective component, that is normally local or at the mesoscale. Thus is not surprising to me that "a lack of evidence and thus low confidence regarding the sign of trend in the magnitude and/or frequency of floods on a global scale" exists. [Claudio Cassardo, Italy]	Noted. If a consistent pattern of increased local convection leads to increased flooding this should be noticed on larger scales too.
2-221	2	5	7	5	8	I would say there is high evidence that there is no trend [Marcel Crok, The Netherlands]	Rejected. The analysis allow identification of a trend yes or no. Not identification of no trend.
2-222	2	5	7	5	8	This statement should be further refined by indicating the main drivers of change in the magnitude/frequency of floods because climate change is not the only main driver. [Klaus Radunsky, Austria]	Rejected. Described are the observations; not the potential drivers for the change.
2-223	2	5	7	5	8	Avoid statements of this sort about a "lack of evidence", as they are very difficult to understand. Does this mean that there is a lack of data, or there are data but they show no trends. The language in lines 20-21 is much better. [Dian Seidel, USA]	Accepted. Sentence reworded.
2-224	2	5	7	5	13	There's a difference between "low confidence, but the best estimate is an increasing trend" and "low confidence, but the best estimate is no or small trend", and "there are insufficient observations to even make an estimate of a trend". It isn't clear which of these categories the flood and drought conclusions refer to. [Marcus Sarofim, USA]	Taken into account. An additional category is that there are no consistent trends at large spatial scales or globally, even though local trends exist.
2-225	2	5	9	5	14	Although this is not true at the global scale, there are several papers cited in the recent U.S. Bureau of Reclamation report describing increasing variability in annual runoff (Pagano and Garen, 2005; and Luce and Holden, 2009) [Charles Luce, United States of America]	Noted
2-226	2	5	10	5	13	Does this mean that the trends have changed since AR5, or that the data are less comprehensive? Are there no regional trends worthy of reporting. It was always presumed that some regions might experience increases (e.g. mid-latitude continental areas) and other regions decreases in drought frequency and intensity. Is this happening? [Timothy Carter, Finland]	Noted. Reanalysis of the data has shown that conclusions are less robust than assessed in the AR4.
2-227	2	5	10	5	13	The popular media continues to promote this erroneous finding from AR4, which certainly suggests that more priority be given this and other revisions/corrections to prior ARs. [Forrest Mims, USA]	Noted

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2-228	2	5	10	5	13	When talking of droughts and dryness it is not clear whether this means low humidity, lack of rain or low ground water supplies and having read the other bullets on rainfall and humidity there could be confusion. It might be better to state if possible whether this statement refers to purely hydrological droughts (lack of rainfall) and or other and avoid the term dryness unless being specific about what is dry – the soil? The air? Water supplies? [Kate Willett, UK]	Taken into account. Clarified that this is about hydrological drought / lack of rainfall.
2-229	2	5	15	5	16	The statement that "there is evidence of an increase in the most intense tropical cyclones since the 1970s" needs more context. Is the increase one that is unusual compared with expected levels of natural variability or not? While one may say that it is unusual in some statistical sense (i.e., the trend is assessed as significantly different from zero according to some test) my question is an important one which needs to be addressed head on, not with some vague language such as there is now, that is open to multiple interpretations. As an aside, our WMO expert team on TCs and climate change (Knutson et al. 2010, Nature Geoscience, concluded: "Therefore, it remains uncertain whether past changes in tropical cyclone activity have exceeded the variability expected from natural causes." We can't tell whether the draft statement in lines 15-16 agrees with this or not. The text needs to be clearer on this point. [Thomas Knutson, U.S.A.]	Rejected. This chapter describes the observed changes. Attribution to causes is the topic of chapter 10.
2-230	2	5	15	5	18	I'm a bit unhappy with the way this conclusion is phrased. The underlying text says that there is low confidence of robustness based on observations since 1970. See page 89, lines 49 - 56 The second sentence in this conclusion refers to destructiviness, which is a totally different quantity and does not follow the same line of the first sentence. It is more consistent to stick to the underlying text, and emphasizing that the data is not robust for the specific period of available data. A minor but somewhat disturbing issue is that 30- and 40-year periods (refering to availbale satellite data) are mixed up. SREX refers to a 30 year period of satellite-based observations on which tropical hurricane intensity is based (SREX chapter 3, para 3.4.4), while here the conclusion refers to a 40 year period. This has to be made consistent. [Bram (Abraham) Bregman, Netherlands]	Taken into account. Text revised and lack of robustness emphasised. New text is closer to Section 2.7.3 text
2-231	2	5	15	5	18	The second sentence of this paragraph does not seem consistent with the first. If there has been in increase in the number of the most intense tropical cyclones, then surely there has been in increase in their destructive potential since almost all tropical cyclone destruction is caused by storms of categories 3-5. In any event, the word “hurricane” should not be used unless referring specifically to Atlantic or eastern North Pacific storms. [Kerry Emanuel, United States of America]	Taken into account. Text revised. See 2-230
2-232	2	5	15	5	18	Need to mention in the summary that the record is too short to be reliable for saying the strongest storms have become more intense. It is mentioned this way on page 83, lines 12-13. [Philip Klotzbach, USA]	Taken into account. Text revised. See 2-230
2-233	2	5	15	5	18	The assertion about hurricane activity is not supported by more recent findings reported by Maue, R. N. (2011), Recent historically low global tropical cyclone activity, Geophys. Res. Lett., 38, LXXXXX, doi:10.1029/2011GL047711. See also comment 23, which is even more applicable to this correction to AR4. [Forrest Mims, USA]	TAKEN INTO ACCOUNT: The tropical storm section and associated executive summary in Ch 2 have been revised. Most of the discussion on this subject is now contained in Box 14.3 where the Maue 2011 results are assessed in context of the other peer-reviewed literature on tropical cyclone activity.
2-234	2	5	16	5	18	This is yet another example of new findings contradicting AR4 findings. There seem to be many of these, and one wonders if there has been a real reversal in the trends in some regions, or if the data quality and/or methods of analysis have changed. [Timothy Carter, Finland]	Noted. The latter is the most important reason for the new assessment results.
2-235	2	5	16	5	18	A reading of this chapter, on which this summary statement appears based, suggests a deficient review of this area of the science, with too much emphasis placed on the one group (GFDL and their direct collaborators), and only lip service paid to work of other experts such as Elsner, Kossin, Emanuel, and Holland which generally contradicts the conclusions drawn. [Michael Mann, USA]	TAKEN INTO ACCOUNT: The tropical storm section and associated executive summary in Ch 2 have been revised. Most of the discussion on this subject is now contained in Box 14.3. As you rightly point out there are multiple groups working in this area and the results

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							presented by those groups are diverse and sometimes contradictory. Our view is that we have assessed the peer-reviewed literature and reflected both our confidence and our uncertainty in our summary given this diversity.
2-236	2	5	20	5	20	helpful to quantify 'small-scale', or give an example of the phenomona [Peter Burt, UK]	Accepted. Hail mentioned
2-237	2	5	20	5	21	This finding deserves more prominent positioning in view of the many quotations in the media that assert anthropogenic climate change is responsible for severe weather events. [Forrest Mims, USA]	Noted
2-238	2	5	20	5	21	This statement should be further refined by indicating the main reasons for the still insufficient evidence and whether or not it can be expected that in the near future it could be possible to achieve greater clarity. [Klaus Radunsky, Austria]	Rejected. More details are in the underlying section 2.7.2
2-239	2	5	20	5	21	Small scale sever events such as..? [Kate Willett, UK]	Accepted. See 2-236
2-240	2	6	1	10	40	A bit odd to have 3 boxes right at the start with no text in between [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted.
2-241	2	6	1	10	40	Climate means have been changed for several times, such as selected by 1951-1980, 1961-1990, 1971-2000, now 1976-2005. Also, it is used for 1981-2100, 1901-1950, 1901-2000, or 1901-2010. This section should mention it and provide some contrasts for global mean surface air temperature anomalies changed with the vatious climate means as shown by the figures or tables. [Zong-Ci Zhao, China]	Taken into account. Sentence added on choice of normal period.
2-242	2	6	3	6	3	Overall I think this is an excellent chapter. Although it is lengthy, it concentrates primarily on progress since AR4 and succeeds in putting this into the context of the major observational challenges in climate change research. It also serves as an anchor for the discussions in many of the following chapters. [George Kiladis, USA]	Noted
2-243	2	6	3	6	5	I like the approach of AR5 which assesses the scientific literature since the AR4. [Richard Heim, U.S.A.]	Noted
2-244	2	6	4	6	4	"identified" instead of "indentified" [Florinda Artuso, Italy]	Accepted
2-245	2	6	4	6	4	"indentified" should be "identified" [Xuemei Wang, China]	Accepted
2-246	2	6	4	6	4	Replace "indentified" with "identified". [Robert Waterland, United States of America]	Accepted
2-247	2	6	7	6	8	written “As described in the AR4 (Trenberth et al., 2007), the climate varies over all spatial and temporal scales: from the diurnal cycle to interannual variability such as El Niño to multi-decadal and millennial variations”. It is necessary to consider that daily cycle behaves to weather scale not climate scale. [José Daniel Pabón-Caicedo, Colombia]	Rejected; weather and climate are not strictly separated in IPCC definitions.
2-248	2	6	7	6	10	In this paragraph there is a passage with two sentences that can cause confusion right at the beginning of the report: “As described in the AR4 (Trenberth et al., 2007), the climate varies over all spatial and temporal scales: from the diurnal cycle to interannual variability such as El Niño to multi-decadal and millennial variations. In this chapter, the changes are examined for the period with instrumental observations, since about 1800.” In summary, it says that the climate varies in several temporal scales and that the changes are examined for the period since 1800. In the way it is written, it implies that climate variations and climate change have the same meaning, are interchangeable expressions. Is this the intention? I could not find any glossary among the available material. Probably many scientists would not agree. As it seems that the IPCC main focus is on possible anthropogenic changes (and its effects on natural variability), the natural variability and anthopogenic changes should be separated. One or two sentences defining what is considered variation and what is deemed as change would help. Of course it is needed to describe the natural variability, but it should not be confused with climate change [Alice Grimm, Brazil]	Taken into account. Reference to climate change definition provided.

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2-249	2	6	9	6	9	Replace "the changes" with "climate variations". [Robert Waterland, United States of America]	Rejected. See 2-248
2-250	2	6	9	6	10	"are examined for the period with instrumental observations, since about 1800" is misleading. Many observations did not start until after 1900. Instead: "are examined for the period with instrumental observations, at the earliest starting about 1800." [Dale Hurst, United States of America]	Accepted
2-251	2	6	10	6	11	A series of linear fits to a nonlinear, and indeed nonmonotonic, time series is not a good way of describing the development. A more comparable approach would be piecewise linear fits. [Peter Guttorp, USA]	Rejected. Box 2.2 describes the choice for the trend method applied here.
2-252	2	6	10	6	12	There are many ways of displaying data: one way is to show trends from different starting dates (though even in that case, why not do a plot with all possible trend starting dates, rather than pick 5?): however, it would be interesting to compare to other means of assessing trends. For example, what 30 year periods have the highest trends in the dataset? What 50 year periods? (maybe non-overlapping?) [Marcus Sarofim, USA]	Rejected. Box 2.2 describes the choice for the trend method applied here.
2-253	2	6	12	6	12	Where written "...information becomes available in 1979." do you mean "… information becomes available for the 1979 to 2010 period"? [Benjamin R. Miller, United States of America]	Accepted.
2-254	2	6	12	6	12	Replace "becomes" with "became". [Robert Waterland, United States of America]	Taken into account. See 2-253
2-255	2	6	14	4	17	The statement about "improved monitoring capabilities" is both vague and too sanguine. Be clearer that the improvement is relative to the early 20th century. And be clear that just because we have satellite observations does not necessarily mean we are truly monitoring the climate system, because satellite protocols do not adhere to the UNFCCC climate monitoring principles. Consider adding a remark on the challenges of obtaining reliable information on climate change from the existing observational data archives. [Dian Seidel, USA]	Taken into account. Improvements are also relative to earlier centuries. Sentence on data challenges added.
2-256	2	6	17			"Dynamical reanalyses datasets" I am not certain what this means - it seems to be poor English. Perhaps"Dynamical reanalysis datasets" as used in Box 2.3? [Philip Lloyd, South Africa]	Accepted
2-257	2	6	19	6	19	The HadSLP gridded pressure dataset also starts in 1850. [Elizabeth Kent, England]	Rejected. Land surface temperature starts earlier now with Berkeley data product.
2-258	2	6	19	6	19	Consider changing "arise from" to "are" [Dian Seidel, USA]	Accepted
2-259	2	6	20	6	21	The statistical need to remove artifacts is not convinvcing. Rather, one ought to model the artifacts (which may, for example when an instrument is replaced, result in changes in variability, even if both instruments are well calibrated). Such changes are rarely, if ever, take into account. [Peter Guttorp, USA]	Taken into account. Modelling (using physical models) is not the objective of Ch2. Modelling (using statistical models) is done as the changes are described using the techniques from Box 2.2
2-260	2	6	20			"Artifact" spelling - elsewhere in this chapter (10x) "artefact" [Philip Lloyd, South Africa]	Accepted
2-261	2	6	21	6	21	Consider changing "associated with" to "have" [Dian Seidel, USA]	Rejected. Don't see the intention of this change.
2-262	2	6	21	6	23	The notion of global mean surface temperature has been criticised (in non-convincing way in my view). See Christopher Essex, Ross McKitrick, Bjarne Andresen 2007, "Does a Global Temperature Exist? "J. Non-Equilib. Thermodyn. 2007 Vol. 32 No. 1, DOI 10.1515/JNETDY.2007.001. The views expressed in this article contribute to the "doubt" some have about the IPCC conclusions, and the IPCC would provide a very useful service to the community by assessing this article in an objective way. [Jean-Pascal van Ypersele, Belgium]	Noted
2-263	2	6	22	6	22	"surface air temperature" [George Kiladis, USA]	Accepted
2-264	2	6	23	6	24	Better to use the more general definition of CS as response to forcing. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Rejected. The IPCC definition of CS is used here. See Glossary.
2-265	2	6	25	6	26	for clarity change one occurrence of "it" to "global mean temperature" [Dale Hurst, United States of America]	Accepted

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2-266	2	6	25	6	26	When talking about observtions that extend far back in time please specify whether only direct observtions are meant or if paleo records are also included. [Uwe Stoeber, Germany]	Accepted. Observational changed into Instrumental.
2-267	2	6	31	6	31	change "ocean" to "oceanic" [Dale Hurst, United States of America]	Rejected. See 2-268
2-268	2	6	32	6	32	cryospheric → cryosphere [Peter Burt, UK]	Accepted
2-269	2	6	32	6	32	a holistic → an holisitic [Peter Burt, UK]	Accepted
2-270	2	6	35	6	35	GHG →GHGs [Peter Burt, UK]	Accepted
2-271	2	6	35	6	38	A cross-reference to chapter 7 would be appropriate here as far as aerosols are concerned. [Olivier Boucher, France]	Accepted
2-272	2	6	35		38	the word "global" is needed in the first two sentences here. Global trends in GHGs (like those presented in this chapter) only allow for emissions verification on a global scale… [Stephen Montzka, USA]	Accepted
2-273	2	6	36	6	36	GHG →GHGs [Peter Burt, UK]	Accepted
2-274	2	6	37	6	37	GHG →GHGs [Peter Burt, UK]	Accepted
2-275	2	6	37	6	37	"play an important role in emissions verification if sinks and their strengths are well understood." [Dale Hurst, United States of America]	Rejected. They play an important role irrespective of understanding.
2-276	2	6	37	6	37	Replace "important" with "essential". [Robert Waterland, United States of America]	Rejected.
2-277	2	6	37	6	38	If radiative forcing issues are deferred to Chapter 8, why does the Executive Summary have two bullets on this topic (p 2-4 lines 13-14 and lines 24-25)? [Dian Seidel, USA]	Taken into account. First bullet has been changed removing RF and second has reference to Ch8
2-278	2	6	40	6	45	Cross refer to Chapter 14. Chapter 2 deals with past changes; Chapter 14 mostly with future changes in modes etc. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-279	2	6	47	6	49	Would it not be reasonable to mention SREX here? [Dian Seidel, USA]	Accepted
2-280	2	6	51	6	51	Replace "The" with "This". [Robert Waterland, United States of America]	Accepted
2-281	2	6	51	6	52	This statement is wishy-washy..are you saying trend means different things throughout the chapter? [Karen Rosenlof, United States of America]	Rejected. The text repeats the glossary definition of trends.
2-282	2	6	51	6	56	That paragraph seems to be here by mistake and should be close to the beginning of the introduction (section 2.1). Mind, the introduction of box2.2 is in this paragraph, if you follow my suggestion, box 2.2 may become box 2.1? [Francois DANIS, France]	Accepted. Paragraph is split in two parts and the first part on trend definition has moved up.
2-283	2	6	51			I like the description of trend very much. Too often the complexities of 'trends' while known to many are left without explanation. [Larry Thomason, United States of America]	Noted
2-284	2	6	52	6	52	An "equivalent linear trend" must be expressed in units that indicate the time period of the trend (e.g. per century). This is not consistently done. [Peter Guttorp, USA]	Taken into account
2-285	2	6	54	6	55	I would say 'examines' consistency, which helps to 'assess confidence' rather than what's written, which to me implies that looking at consistency always gives you greater confidence. [Drew Shindell, USA]	Accepted
2-286	2	6		10		Section 2.1: The introduction should include an explanation of what is considered anthropogenic climate change and climate natural variability (internal or external). As shown in the comments above, sometimes these expressions are used as if they were interchangeable (lines 7-10, page 2-6) and in other instances as if they meant different things (Page 2-7, lines 1-3). This separation is also important in view of the content of section 2.6. [Alice Grimm, Brazil]	Taken into account. The text in lines 7-10 on page 2-6 is changed and includes a definition of climate change used here which does not distinguish natural causes from anthropogenic causes.
2-287	2	7	1	7	3	What is defined as observed changes? What is the criterion to be considered an observed change and not a	See 2-286

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						natural variation? What is defined as natural internal climate variability? What would then be the natural external climate variability? If it is stated that there are observed changes, it is implied that they are not natural variability and thus a criterion to define these observed changes should exist. Is it the existence of a trend? For how long? [Alice Grimm, Brazil]	
2-288	2	7	1	7	3	It this technically correct? Doesn't the method that tests the significance of trends, used in this chapter, assume some level of internal variability in the data (albeit naively) - especially when an AR1 process is assumed? Other statements in this chapter do compare significance of trends against internal variability - e.g. P2-20 L13 to L14 [Gareth S Jones, UK]	Taken into account. Wording made consistent and trend detection only in statistical sense without forcing cause.
2-289	2	7	1	7	3	It would be very useful to have some indication of how far the observed trend is outside the range of observed variability. The formal detection and attribution of Chapter 10 relies heavily on the assumption that climate models correctly represent both the trend and variability. Having a purely observationally based estimate here would strengthen the case for significant trends even when the climate models are not reliable. [Geert Jan van Oldenborgh, Netherlands]	Taken into account. The reported trends show this. Sentence added to the introduction text.
2-290	2	7	1	7	7	It is not clear that this statement has been adhered to. For example, Tables 2.2, 2.4, 2.6, 2.9 include standard errors in the trends, but it is not clear that these standard errors include any information about observational uncertainty at all. The same is true for Box 2.2 Table 1. This quoted standard error is model dependent and either should not be included, or quoted separately from uncertainty in the trend arising from observational uncertainty e.g. data biases or other uncertainties discussed in Box 2.1. The observational uncertainties are most pertinent to this Chapter and have not been included. As pointed out in line 5 and 6 on page 7 the quoted standard errors are model dependent and would be more properly discussed in Chapter 10. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Sentence on trend significance added.
2-291	2	7	1	7	7	This paragraph tries to draw too sharp a distinction between measurement of change and detection of change (in the D&A sense). For instance, if -- hypothetically -- there were no statistically significant trends in any temperature data sets, it would be hard to report that fact and then say that this has no bearing on whether the observations lie within the range of natural variability or not. That just wouldn't make much sense. I think what you are trying to say is something like "This chapter will be concerned with measuring observed changes, reporting on their associated uncertainties (including statistical confidence intervals on observational model parameters like trend coefficients as well as measurement errors and so forth), and discussing the effects of urban heat islands and other non-climatic influences, but the question of whether observed changes are consistent with expected effects of GHG will be left to Chapter 10. That is to say, a large observed change is not necessarily evidence of anthropogenic GW, nor is a small observed change, in itself, evidence against AGW. Those questions typically cannot be answered by observations alone." [Ross McKitrick, Canada]	Taken into account. Text on meaning of trends in this chapter added and AGW mentioned
2-292	2	7	1	7	7	Is the bottom line in this paragraph that you are not attempting to determine statistical signficance to the changes in assorted parameters discussed in this chapter? [Karen Rosenlof, United States of America]	Taken into account. Text on meaning of trends in this chapter added.
2-293	2	7	2	7	2	insert 'of' after 'whether' [Peter Burt, UK]	Accepted
2-294	2	7	3	7	6	It is indefensible here to cite Cohn and Lins argument for the presence of long-range dependence (LRD) as evidence that trend determination depends on null hypothesis in the manner implied. If this is to be cited, then also cite this work demonstrating the fallability of statistical methods that attempt to impute the presence of LRD in climate time series: Mann, M.E., On Long Range Dependence in Global Surface Temperature Series, Climatic Change, 107, 267–276, 2011. [Michael Mann, USA]	Accepted. Reference added.
2-295	2	7	3	7	7	Cohn and Lin conclude (op cit pL23402)"But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence, and non-linearity of the climate system, it seems the answer might be yes. Finally, that reported trends are real yet insignificant indicates a worrisome possibility: natural climatic excursions may be much larger than we imagine. So large, perhaps, that they render insignificant the changes, human-induced or otherwise, observed during the past century." In other words, the decision to ignore Wu et al on the basis of Cohn and Lin should lead to ignoring ALL suggestions of trends in global temperatures. Alternatively, it is necessary to reject Cohn and Lin and include consideration of multidecadal oscillatory variations and/or secular trends. Certainly Boxes 2.1 and 2.2 MUST be rethought with Cohn and Lin's caveat in mind. [Philip Lloyd, South Africa]	Rejected. As explained in the text better now, the existence of multidecadal oscillations, persistence or secular trends in the observations alone will not "explain" observed change. For D/A we need models of our understanding of the climate system.

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2-296	2	7	4	7	6	I think AR4 Chapter 3 did do this to a limited extent. Although the emphasis of Chapter 2 should be a description of observed changes and trends, to make "no attempt" at such interpretation is going backwards from AR4 and reduces the impact of the text. An alternative is appropriate cross referral to such discussion in other chapters like chapter 14. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Noted. References have been added where appropriate, in particular to Chapter 10
2-297	2	7	5	7	5	Remove "either". [Robert Waterland, United States of America]	Rejected. This comes in addition to not attempting D/A as described in the previous sentence
2-298	2	7	6	7	6	chapter → Chapter [Peter Burt, UK]	Accepted
2-299	2	7	6	7	6	It is commendable that the authors mention Cohn and Lins, 2005. Unfortunately this is the only place in the entire report where this important paper is mentioned. In their conclusions Cohn and Lins write: “[With respect to] temperature data, there is overwhelming evidence that the planet has warmed during the past century. But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence and non-linearity of the climate system, it seems the answer might be yes…natural climatic excursions may be much larger than we imagine.” AR4 did not do a good job dealing with this topic, as is explained in McKitricks submission to the IAC: http://www.rossmckitrick.com/uploads/4/8/0/8/4808045/iac.ross_mckitrick.pdf (page 7-9) He mentions an email of Parker to Jones, who were responsible for the AR4 treatment of the Cohn and Lins paper. Parker wrote: "Maybe the biggest problem is Ross McKitrick and David Stephenson’s remarks on trends; we used only an AR-1 and they may be correct in advocating a more complex model. Our software for restricted maximum likelihood does not cope with ARMA(1,1) and may have to get John Kennedy to investigate new software using the cited references. This may be a big job but could be done after the LA3 meeting if we agree there what to do. Alternatively – as we have considered already – we could consider not citing linear trends, just overall changes of level from the smooth curves. This would save some space." In the end Parker and Jones did cite linear trends in chapter 3 claiming the warming was highly significant. In the second draft of AR4 IPCC wrote: "Determining the statistical significance of a trend line in geophysical data is difficult, and many oversimplified techniques will tend to overstate the significance. Zheng and Basher (1999), Cohn and Lins (2005) and others have used time series methods to show that failure to properly treat the pervasive forms of long-term persistence and autocorrelation in trend residuals can make erroneous detection of trends a typical outcome in climatic data analysis." This was a fair comment about Cohn and Lins. However after the second draft this text was removed and in appendix 3.a a much more disputatious text was introduced: "Nevertheless, the results depend on the statistical model used, and more complex models are not as transparent and often lack physical realism. Indeed, long-term persistence models (Cohn and Lins, 2005) have not been shown to provide a better fit to the data than simpler models." This was a completely ad hoc remark without any reference to the literature. A fair treatment of this topic in AR5 is much needed. I think this discussion should be dealt with in both Ch 2 and 10. [Marcel Crok, The Netherlands]	Taken into account. We have kept the reference to Cohn and Lins and further explained our assessment of curve fitting.
2-300	2	7	6	7	7	I am not sure this is correct. The observational uncertainty is often not used to test the robustness of a change in this chapter. For instance the significance of trends for CRUTEM4 in Table 2.2. is based solely on a linear model looking at the residuals of the fit and not on the observational error (which is available for CRUTEM4). [Gareth S Jones, UK]	Taken into account. The observational uncertainty is described in the text. The trend significance is calculated without this indeed.
2-301	2	7	9	7	9	sections → sections [Peter Burt, UK]	Accepted
2-302	2	7	9	7	9	insert comma after 'sections' [Peter Burt, UK]	Accepted
2-303	2	7	9	7	9	insert comma after 2.7 [Peter Burt, UK]	Accepted
2-304	2	7	9	7	13	This is a good plan for the chapter, but I don't think the chapter always follows this plan. Some sections give a lot more background than the reader might expect, given this statement of intention. [Dian Seidel, USA]	Taken into account. Detail removed from sections.
2-305	2	7	13	7	13	chapter → Chapter [Peter Burt, UK]	Accepted
2-306	2	7	18	8	17	The subject of this Box 2.1 is very important. The number of references that had been referred are not appropriate. I suggest to add some references for following more information by reader. [Fatemeh	Noted but there is limited space so no changes have been made here. Also, boxes should be reference

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						Rahimzadeh, Iran, Islamic Republic of]	light.
2-307	2	7	18	8	19	The language of Box 2.1 is too erudite and the text is too long. Especially because "Boxes" are supposed to explain important science concepts in general language, I'd suggest this be re-written in plainer English. [Dian Seidel, USA]	Taken into account. And other feedback has acted to provide some guidance but with no explicit guidance it is hard to absolutely know what the reviewer wanted changed here.
2-308	2	7	20	7	20	"historical and present" should be "historical and modern" [Dale Hurst, United States of America]	Accepted
2-309	2	7	20	7	20	does one "take" or "make" observations? [Dale Hurst, United States of America]	Noted. Rewritten text says 'made'
2-310	2	7	20	7	20	The inclusion of "and present" doesn't make sense here, if the verb is "were" in the past tense. Besides, the term "present weather" is actually meteorological jargon for part of the synoptic observation, so it could be misconstrued. [Dian Seidel, USA]	Taken into account. Combined with 2-308, 2-309. And modern is in parenthese now to show that the apprioach persists at present.
2-311	2	7	23	7	23	"changing their mean" should be "changing their values" [Dale Hurst, United States of America]	Rejected. The sentence is about mean and variability as the first two modes.
2-312	2	7	25	7	25	I'm not sure how you "measure physical atmospheric characteristics". Instead you "measure physical atmospheric quantities" [Dale Hurst, United States of America]	Accepted
2-313	2	7	25	7	31	The uncertainty in observational records should also include the measurement errors themselves. These can be either systematic [Klaas Folkert Boersma, Netherlands]	Comment is substantially cut off in the transmitted version and therefore cannot be interpreted or responded to.
2-314	2	7	26	7	26	The word "representivity" does not appear in my dictionary. Use "representation" instead? [Dale Hurst, United States of America]	Accepted
2-315	2	7	26	7	27	Is it correcct to call the list in parentheses after "representivity" errors? If not appropriately accounted for, they may produce an error in the trend analaysis, but they are not all an error in the measurement itself. (for example, just because temperature may be measured at different local times does not mean it is in error). [Karen Rosenlof, United States of America]	Rejected. They are errors in the raw data in that they do not faithfully represent the true long-term nature of the measurand.
2-316	2	7	27	7	27	I'm not sure what 'measurement geometry' means. [Kate Willett, UK]	Accepted. Example deleted.
2-317	2	7	27	7	27	Having something about the lack of metrological principles historically such as traceable calibrations back to known measurement standards might be relevant here. [Kate Willett, UK]	Noted. The metrological chain of tracability is discussed later in the revised paragraph and minor changes to that have been made.
2-318	2	7	29	7	31	This is a very poor example. I'd think of better example, or not bother putting one at all. This particular example can be shown to have hardly any effect at all. It also depends on how the spatially averaged series is produced and how big the region is. [Philip JONES, UK]	Accepted. Example text sentence removed.
2-319	2	7	33	7	34	Replace "There is no unique, unambiguous, solution to identify and account for non-climatic artefacts in the vast majority of records, which leads to a degree of uncertainty as to how the climate system changed." with "Since there is no unique, unambiguous, way to identify and account for non-climatic artefacts in the vast majority of records, there must be a degree of uncertainty as to how the climate system changed." [Robert Waterland, United States of America]	Accepted
2-320	2	7	33	7	39	The wording of this entire paragraph is deeply troubling. Implicit in the passage is the argument that most climate records, such as the surface temperature record, *cannot* "be considered an accurate record of the true changes in the measured quantity as sensed by the instrument". Yet such a claim is self-evidently false and is belied by the high levels of confidence that are attributed to many of these measurements in the chapter itself, e.g. the "virtual certainty" ascribed to the conclusion that "Globally averaged land surface air temperatures have increased since the late 19th Century and this warming has been particularly marked since the 1970s". How could virtual certainty be attained if, as implied by this current paragraph, the surface temperature record cannot "be considered an accurate record of the true changes"---a conclusion that the author of this paragraph has implied. This entire paragraph should either be stricken or rewritten in such a	Noted. We disagree with the reviewer's assertions here. Saying that there is no uncertainty or not outlining the sources of that uncertainty would do a disservice to our readers. Modifications to some aspects in response to other review comments may have helped but striking out the paragraph is disingenuous to the readers and the science.

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						way that it is defensible. The chapter authors need to do a better job in establishing some degree of consistency across this chapterin discussion of data reliability and associated levels of confidence and certainty. [Michael Mann, USA]	
2-321	2	7	33	7	39	The iconic Keeling CO2 record at Mauna Loa Observatory (MLO) since Mar 1958 is of superb quality and is nicely validated by the simultaneous NOAA CO2 record since May 1978. But these are not the only or the oldest very high quality data sets. The careful reader will want to know that the longest series of sunlight atmospheric transmission measurements in the world, the Ellis curve, was begun at MLO in Nov. 1957 (F. M. Mims, Hawaii's Mauna Loa Observatory: Fifty Years of Monitoring the Atmosphere, Univ. Hawaii Press, 226-228, 2012 (in print Nov 2011). Also highly significant are the Smithsonian APO measurements that provide 30 years of data on column ozone, column water vapour and optical depth. Various papers have carefully reviewed these findings, which show essentially no significant trends and which have long been due citation in an IPCC climate report such as this. One of various suitable citations is (free on AMS site): Roosen, Robert G., Ronald J. Angione, 1984: Atmospheric Transmission and Climate: Results from Smithsonian Measurements. Bull. Amer. Meteor. Soc., 65, 950–957. [Forrest Mims, USA]	Noted but there is limited space and the box is there to be short and informative so this material arguably does not belong here (see 2-307) and no changes have been made here. We havve explicitly called out the CO2 curve as being from MLO (see comment 2-326). Any changes suggested should be in the main body text instead in the relevant subsections rather than here.
2-322	2	7	35	7	35	change "exception is" to "exceptions are" [Dale Hurst, United States of America]	Accepted
2-323	2	7	35	7	37	"… that are directly tied to internationally recognized absolute measurement standards." [Dale Hurst, United States of America]	Accepted
2-324	2	7	36	7	36	insert comma after well-characterised [Peter Burt, UK]	Taken into account. Response to comment 2-323 has led to deletion of this excessive verbiage.
2-325	2	7	37	2	39	It is unclear to me what is meant in this part. [Klaas Folkert Boersma, Netherlands]	Noted. Revisions in response to other reviewer comments may have helped here but no specific changes were requested by the reviewer.
2-326	2	7	37	7	37	"including the CO2 record at Mauna Loa (Keeling, 1976)" [Dale Hurst, United States of America]	Accepted. Comment 2-321 also applies.
2-327	2	7	37	7	37	The reference to Keeling (1976) is specific to the measurements at Mauna Loa. This makes it unclear as to whether the entire measurement system is being referred to or just that one location. [Elizabeth Kent, England]	Taken into account. See response to 2-326
2-328	2	7	37	7	39	When discussing directly tracable measurements, I think the mention of "non-instrumental effects" should just be described as "other sources of uncertainty" so as to avoid ambiguity. [Jeffrey Taylor, United States of America]	Noted. Text has been deleted for brevity.
2-329	2	7	37			Such records,…, can be considered an an accurate record'. This sentece is awkward, because the word record is used twice. [Klaas Folkert Boersma, Netherlands]	Taken into account. Sentence deleted
2-330	2	7	39	2	39	This is very much the exception to the norm.' Does 'This' refer to non-instrumental effects? [Klaas Folkert Boersma, Netherlands]	Noted. We have deleted this sentence for clarity.
2-331	2	7	39	7	39	Not clear what 'This is very much...' refers to [Peter Burt, UK]	Taken into account. Combined with 2-330
2-332	2	7	41	7	51	There is an additional source of uncertainty which is relevant here, but not mentioned: the analysis or reconstruction uncertainty. This is relevant because some of the data sets used in the chapter are interpolated or reconstructed using statistical techniques. Each technique has a theoretical uncertainty model associated with it. The uncertainty estimated for each analysed value can help to explain differences between different analyses of the same variable. Structural uncertainty as determined from the current ensemble of opportunity of best estimates does not include this. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Rejected. We would maintain that this is implicit within the structural and parametric uncertainty classes already alluded to as it results from choices made in the analysis.
2-333	2	7	41	8	17	In the overall discussion of uncertainty and definition of approaches (as well as nomenclature), I urge the authors to consider adopting an accepted standrad for terminology (such as the ISO Guide to Uncertainty in Measurements). By utilizing different terminology and definitions, it obfuscates the message and makes it less translatable to other studies. [Jeffrey Taylor, United States of America]	Noted. While we agree in principle the reality is that many different terminologies in many different fields exist. The GUM is just one of several alternatives that could be used here. The key is reader clarity and in-field understandability and in this case would not be

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							well served by introduing the GUM languahe and adding the substantial amount of verbiage required to implement it and still be understandable.
2-334	2	7	42	7	45	Replace "Parametric uncertainty is the range of estimates that arises solely through varying a restricted subset of methodological choices for which no rigorous basis exists, e.g., when adjusting for an apparent break in a time series whether to use 2, 3 or 5 years of data either side to estimate this adjustment." with "Parametric uncertainty is uncertainty that arises solely from use of methodological choices for which no rigorous basis exists, e.g., when adjusting for a break in a time series by using 2, 3 or 5 years of data either side to estimate this adjustment." [Robert Waterland, United States of America]	Noted. Paragraph has been rewritten and example removed for brevity.
2-335	2	7	45	7	45	edit to avoid bad English '.. adjustment, but the ..' [Peter Burt, UK]	Noted. Paragraph has been entirely and extensively revised and this is no longer applicable.
2-336	2	7	45	7	45	Replace "But the overall methodological framework" with "The overall analytical approach". [Robert Waterland, United States of America]	Accepted
2-337	2	7	46	7	46	Put "structural uncertainty" into italics. [Robert Waterland, United States of America]	Rejected. Italics are reserved for uncertainty langauge and emphasis according to format guidelines.
2-338	2	7	46	7	48	This sounds overconfident about our ability to quantifying structural uncertainty -- "most easily ascertained from having multiple independent groups… etc.". Really should point out that some parametric and especially some structural uncertainty cannot be estimated/quantified even by these approaches, because the choice of methodological framework is limited in various ways, such as by the type and availability od observational data. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. The entire paragraph has been extensively rewritten.
2-339	2	7	48	7	48	"assess the same data using distinct approaches." [Dale Hurst, United States of America]	Noted. Redrafting of the paragraph has led to removal of this text.
2-340	2	7	48	7	48	Sometimes the approaches taken by independent groups are more alike than one might think. I would change "they will have" to "they will usually have". [Robert Waterland, United States of America]	Noted. Redrafting of the paragraph has led to removal of this text.
2-341	2	7	48	7	50	Can references be provided to support the statement of structural uncertainties almost always being larger than parametric uncertainties? [Colin Morice, UK]	Noted. Sentence has been removed for brevity.
2-342	2	7	48	7	50	Is structural uncertainty always more useful ? It could be couldn't it that previous estimates from multiple groups all contain common biases and that a new estimates with uncertainties that accounts for previous common biases is therefore a better estimate [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence deleted.
2-343	2	7	49	7	49	larger → greater [Peter Burt, UK]	Noted. Sentence has been removed.
2-344	2	7	50	7	50	insert comma after 'Therefore' [Peter Burt, UK]	Taken into account. Sentence has been deleted as per 2-342
2-345	2	7	50	7	51	This relates to one of the key GCOS Climate Monitoring Principles. Here, especially, and perhaps elsewhere, these should be referenced with the appropriate GCOS document. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected - GCOS literature is construed by author team to be grey literature and, sadly, unciteable. Sentence has been deleted in response to comment 2-342.
2-346	2	7	51	7	51	a holistic → an holistic [Peter Burt, UK]	Taken into account. Sentence deleted in response to 2-342.
2-347	2	7	51	7	51	remove "wherever possible" [Dale Hurst, United States of America]	Taken into account. Sentence deleted in response to 2-342.
2-348	2	7	51	7	51	I would suggest replacing the word 'parameter' with 'climatological variable', to avoid confusion with the concept of parametric uncertainty. [Colin Morice, UK]	Taken into account. Sentence deleted in response to 2-342.
2-349	2	7	53	7	54	This isn't true. This was recognized in 2007, it just wasn't stated as such. It is much more true for much earlier	Taken into account. Text now says that there are

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						(say pre 2000), but to say this has only been discovered since AR4 is stretching things a lot. There were numerous series in AR4 that had uncertainty ranges. [Philip JONES, UK]	more now than at the time of AR4.
2-350	2	7	55	7	55	Is it really the case that uncertainties have been assessed using a "broad range of approaches", for all the variables considered in the chapter? I think that may be the case for some variables, but for others uncertainty probably has not been assessed at all, or only minimally. [Dian Seidel, USA]	Rejected. The text does not suggest that all studies contain uncertainty estimates
2-351	2	7	56	2	56	What is a 'like-for-like comparison'? I suggest to use another term here, because I don't think many readers will know what this is. [Klaas Folkert Boersma, Netherlands]	Rejected. Like for like comparison is the best language we can consider for such a context.
2-352	2	7		8		Good explanation of the nature of 'data' [Larry Thomason, United States of America]	Noted
2-353	2	7				or random in nature. This is especially relevant to non-trivial measurements such as satellite retrievals. [Klaas Folkert Boersma, Netherlands]	Fragment comment with insufficient context for any action to be taken.
2-354	2	7				I can't find Box 2.1 anywhere -- it must have just gotten lost in the figure attachements. It sounds like it addresses an important topic (uncertainty), so we'll need to be sure to include it somewhere. [Jeffrey Taylor, United States of America]	Noted. TSU may wish to surround boxes with a black box outline next time around to avoid such confusion arising?
2-355	2	8	3	8	3	"should indicate a better product" [Dale Hurst, United States of America]	Accepted
2-356	2	8	3	8	7	Just to reiterate the previous comment, utilizing an accepted ISO metric, such as expanded uncertainty, would clarify this discussion and minimize ambiguity. [Jeffrey Taylor, United States of America]	Noted. See response to comment 2-333
2-357	2	8	6	8	34	This part of Box 2.2 compares OLS with splines. It is all very well to compare them, but no reason is then given for choosing OLS over splines for estimating the overall temperature changes. In Box 2.2, Table 1, you should definitely also include the OLS and spline results for the 1901-2005 and 1979-2005 periods because you specifically note (on lines 3-4 of this page) that OLS trends can easily change when new data are added, and because in the executive summary you compare the warming up to 2005 with the warming up to 2010 and it would be very useful to know if the impact of these extra five years is similar for both OLS and splines. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	This comment refers to a different section of the chapter than assigned so is not in my purview to respond to. Make sure Box 2.2 authors see and respond to it.
2-358	2	8	8	8	8	It may be worth briefly mentioning the concept of random" versus "systematic" error here, especially since these concepts are used later in the chapter, as in Section 2.2.2.1.1. [George Kiladis, USA]	Noted. See response to 2-333
2-359	2	8	11	8	11	insert comma after 'Therefore' [Peter Burt, UK]	Editorial
2-360	2	8	12	8	12	Sentence starting "Confidence can be built …" doesn't make sense [Elizabeth Kent, England]	Taken into account. See comment 2-362
2-361	2	8	12	8	13	Concepts … from one or more of redundancy in efforts to create products' - I did not understand this part. Suggestion: Confidence [Klaas Folkert Boersma, Netherlands]	This comment is incomplete (truncated) and therefore we are unable to respond to it.
2-362	2	8	12	8	13	Suggest "Confidence can be built from redundancy in efforts to create products,….or from cross-comparisons". Existing language is nonstandard and confusing. [Melissa Free, USA]	Accepted
2-363	2	8	12	8	13	I don't know what is meant by "Confidence can be built from one or more of redundancy in efforts to create products, from product heritage …" [Robert Waterland, United States of America]	Taken into account. See comment 2-362
2-364	2	8	14	8	14	Replace "co-vary" with "be correlated". [Robert Waterland, United States of America]	Rejected. Co-vary is more than simple correlation. It is a physical explanation rather than a statistical one. This has been clarified
2-365	2	8	16	8	16	chapter → Chapter [Peter Burt, UK]	Accepted
2-366	2	8	22	9	36	I was very optimistic after reading this box that consistent trend estimation methods might be used throughout the chapter, but my optimism was not warranted. The box misleads the reader, because the principles are applied only to a few variables in the chapter. I'd like to see all variables treated in the same way. If the authors chose not to do that, then at least be clear that the "proposed" (p 8 line 29) method is not actually applied in most of the chapter sections. [Dian Seidel, USA]	Accepted: in the SOD most trends in Chapter 2 are computed in a common way that is described in the Box 2.2.

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2-367	2	8	22			Box 2.2: AR(1) should be properly introduced. Sometimes AR1 is used that might be confused with an earlier Assessment Report. [Uwe Stoeber, Germany]	Accepted.
2-368	2	8	22			Box 2.2: In this discussion the authors should also comment on the issues raised by McShane & Wyner (2011). [Uwe Stoeber, Germany]	Rejected. Paper about last 1000yr reconstruction; not instrumental observations.
2-369	2	8	24	8	24	See comment above about Cohn and Lins, 2005. [Marcel Crok, The Netherlands]	Noted; see response above
2-370	2	8	24	9	36	The calculations do not obviously include observational uncertainty, only model uncertainty. Observational uncertainty is a key part of the chapter (see Box 2.1) and is not incorporated into this assessment. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. The box intends to introduce the trend method only giving an example of global temperature change. Moreover, distinctions between different kinds of data uncertainty is not particularly useful in the simple paradigm of linear trend: x = a*t + b + e, where e is the sum of all kinds of errors that make x deviate from the straight line a*t+b. Only statistics of the total e and not of its possible multitude of components matters for the estimation of parameters a and b.
2-371	2	8	26	9	13	I am pleased to see some description of the issues around calculating trends. In light of possible criticism from certain quarters, it might be helpful if there were a further expansion of how knowing a trend in data can be useful and what in practice is meant by a significant trend. Currently there is a danger that the method used to estimate significance in the trends is in effect a "detection" statement as there is an assumption that the residuals of the linear fit have an internal variability contribution. e.g. P2-20 L13 to L14 that talks about significance of trend with respect to internal climate variability. [Gareth S Jones, UK]	Rejected. The introduction explains the meaning of trends in relation to D/A and an additional sentence now explains what the meaning of significance is wrt the observed variability. In the lines 13-14 on P2-20 we were simply making an observations that areas where linear trends over last 30 years were not statistically significant are areas known for energetic interannual variability. These lines are excluded from SOD because of space limitations.
2-372	2	8	26	9	36	Suggesting that the alternative to a linear fit of the mean is only a non-linear fit of the mean is incomplete. Quantile regression tools allow for linear and non-linear fits to data where the variance is also non-stationary. It is commonly applied in economics (the tool used to show that the rich are getting richer in the US, for example) and have been applied to look at climate trends as well (see e.g. Luce, C., Holden, Z., 2009, doi:10.1029/2009GL039407 and citations therein). Refer to Figure 1.9 in chapter 1 of AR5 WG1. [Charles Luce, United States of America]	Rejected. The box is about changes in the mean and trend methods with regards to that. There are many additional statistical techniques to analyse climate data, but these are not discussed here.
2-373	2	8	29	8	36	It should be mentioned that the usual linear trend estimation is based on the least squares method (LSM) which requires Gaussian distributed data. In case of temperature this precondition is often justified but not in case of e.g. precipitation. So, it should be not neglected that methoids of trend analysis are available which avoid the precondition of Gaussian distributed datsa. Troemel and Schoenwiese using such an alternative method have shown for the example of precipitation trend patterns 1901-2000 observed in Germany that the LSM method tends to overestimations of trend values. Reference: S. Troemel and C.-D. Schoenwiese, 2008: Robust trend estimation of observed German precipitation. Theor. Appl. Climatol., 93, 107-115. [Christian-D. Schoenwiese, Germany]	Taken into account. The text does describe that LS is used and which assumptions are made. Many other alternatives exist but these are not included all.
2-374	2	8	32	8	52	The assumption of AR(1) errors is often overly simplistic. For example, most temperature series exhibit long term memory, and methods exist to deal with regression for such series (e.g. Smith, 1993, Stat, for the Env. 141-161, Wiley; Smith & Chen, 1996, Springer LN Stat v.115, 378-391; Craigmile et al, 2004, Environmetrics 15, 313-335,) [Peter Guttorp, USA]	Taken into account. The existence of many different methods has been acknowledged. Also see Introduction where it is stated that we will not attempt to further interpret the observed changes in terms of multidecadal oscillatory variations, (long-term) persistence etc.
2-375	2	8	32	8	52	The treatment of serial correlation here is too far behind the state of knowledge to be acceptable. Chapter 1 pages 5-6 explain that climatic processes operate with long, slow, lagged responses, which implies "long memory" in a statistical sense. Yet this text box asserts that an AR1 model is everywhere appropriate, irrespective of whether the data set is daily, monthly, annual etc., which implies response processes that can be modeled with a single parameter at all time scales. These 2 views are inconsistent. Obviously the AR1 view presented here is wrong and the treatment needs to be brought up to date. Surely you can't propose to use	Taken into account. The trends here are only used to describe the series. Disregarding the long-term memory in a statistical sense doesn't mean that we don't acknowledge its existence. As the introduction states, the climate varies on all time scales. This Chapter describes the changes in the mean using

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						the "effective dof" method employed in Santer et al. (2008). It was an approximation developed in the 1930s before computers were available, it is known to be inaccurate for higher-order AR processes and it is incorrect for the purpose of comparing trends among data sets even in the AR1 case. Also, REML, based on the Diggle book, is for longitudinal data sets and its application to time series analysis is not standard. I don't know why it keeps reappearing in IPCC chapters--this is the only place I ever see it used. For a recent survey of modern trend modeling issue see Mills (2009 -see cell 15) and for a good treatment of the comparison of trends across multiple autocorrelated data sets see Vogelsang, Timothy and Philip Hans Franses (2005) Testing for Common Deterministic Trend Slopes. Journal of Econometrics 126 (2005) 1—24. A standard, but somewhat dated, method would be ARMA(p,q) errors, which are available in any stats software. You could be easily use Newey-West (1987) standard errors (see cell 18) which are robust to any form of autocorrelation, and which are also available in most modern stats packages now. An even better, nonparametric & bandwidth-free approach is the Vogelsang & Franses (2005) estimator, applied to climatic data in McKitrick et al. (2010 - see ref. in cell 8). To adopt a 1930's era "effective degrees of freedom" approach risks discrediting the whole chapter. [Ross McKitrick, Canada]	linear trends. Linearity of the trend is an approximation and the AR(1) model for the deviations of the annual mean of a climate variabe from the straight line is an approximation too. However, these are well-established, traditional approximations. Applying significantly more complicated statistical models to relatively short climate timeseries in the routine statistical analyses (like those presented in this chapter) is ill-advised, as it might result in a poor estimation of model parameters (e.g., von Storch, H., and F. W. Zwiers, 1999: Statistical Analysis in Climate Research. Cambridge University Press, 484 pp) and therefore less reliable inferences than what a simpler model would give. Or, at best, "it is often difficult to establish a statistical superiority of one model over other models due to the relatively short time series" (Percival, Donald B., James E. Overland, Harold O. Mofjeld, 2001: Interpretation of North Pacific Variability as a Short- and Long-Memory Process*. J. Climate, 14, 4545–4559). More sophisticated analyses are available in the literature and assessed for special cases but none are immediately suitable to be consistently applied throughout all datasets here and have their results easily interpreted. As to the effective dof (or effective sample size) concept, it is really irrelevant when it was developed and was it before or after the availability of computers. It is a useful concept, still prominent in the statistical research (e.g. , Christel Faes, Geert Molenberghs, Marc Aerts, Geert Verbeke, Michael G. Kenward, The Effective Sample Size and an Alternative Small-Sample Degrees-of-Freedom Method, The American Statistician, Vol. 63, Iss. 4, 2009, and references therein). It is also immaterial that Diggle's book presents the REML estimation of trend for a longitudinal problem: mathematically it is an identical problem to the one about the trend in timeseries, and it has been treated as such (Wai-Kwong Cheang and Gregory C. Reinsel, Bias Reduction of Autoregressive Estimates in Time Series Regression Model through Restricted Maximum Likelihood Journal of the American Statistical Association , Vol. 95, No. 452 (Dec., 2000), pp. 1173-1184)
2-376	2	8	32	8	52	What does it mean to say that methods are "very effective" (line 50)? This is not a statistical term. We would normally say that methods yield coefficients that are unbiased, consistent, efficient, etc., or that test statistics based on certain variance estimators are powerful and/or size-robust. Unfortunately none of these things can be said about the methods used in this chapter. The term "effective" here is meaningless. [Ross McKitrick, Canada]	Taken into account. The sentence is re-worded.
2-377	2	8	32	8	52	Mills TC. 2010. Skinning a cat: alternative models of representing temperature trends. Climatic Change 101: 415–426. DOI 10.1007/s10584-010-9801-1. [Ross McKitrick, Canada]	Noted. Added to the references which support the subjective choices of modeling time series using statistical methods only.
2-378	2	8	32	8	52	Reference: Newey, W.K. & K.D. West (1987) “A simple, positive semi-definite, heteroskedasticity and	Noted.

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						autocorrelation consistent covariance matrix." Econometrica 55, 703–708 [Ross McKitrick, Canada]	
2-379	2	8	34	8	35	The "mean has changed linearly over time" is a tricky concept. If it's true, then the stationarity assumption is violated. And it requires conceiving of a moving window for the computation of the mean from the start to the end of the time series. Is there a simpler way of expressing the concept? [Dian Seidel, USA]	Rejected. One can talk about the mean for a particular year too. The stationarity assumption typically applies to the trend residuals, not to the total signal containing (possibly) non-zero trend.
2-380	2	8	34	8	36	The reader should be informed that the common use of linear trends in climate science, including by the IPCC, is often overly simplistic, sometimes misleading and primarily a convenience for expressing model results rather than displaying actual empirical findings. Linear trends should be replaced or at least accompanied by moving averages or other appropriate functions. The serious reader will appreciate being informed about the significance of using non-linear functions, including averaging, to better visualize the perturbations that occur in time series of data. I base this opinion on 25 years of monitoring many atmospheric parameters. (There is not time to ask about error bars and related topics.) [Forrest Mims, USA]	Taken into account. Better explanation of and motivation for the choice of trend method is provided. The adopted methods of linear trend calculation does include error bars on its slope estimate.
2-381	2	8	34	8	54	The use of linear trends is an unsatisfactory and irresponsible technique for assessing climate time series as it exaggerates the importance of the least realiable data and conceals irregular or periodic behaviour [VINCENT GRAY, NEW ZEALAND]	Taken into account. Better explanation of and motivation for the choice of trend method is provided. However, the reviewer's claim about exaggerating least reliable data by linear trends is unsubstantiated. Furthermore, a simplified description (e.g., by a linear trend) of any phenomenon always "conceals" something: this is the nature of any modeling.
2-382	2	8	38	8	52	I gave up trying to work out what this paragraph is trying to say, as it is so difficult to keep track of the information being presented. Also, I am not clear what an AR(1) process is. I thought at one stage you were referring to something in the First Assessment Report, but this may not be the case. In any case, the whole paragraph needs some careful review for clarity. [Peter Burt, UK]	Taken into account. The description is clarified.
2-383	2	8	38	8	52	This section should be reorganised and better put into context. Least Squares Estimators (LSE) are a very ad-hoc method without a statistical foundation. However, if the residuals can be assumed to be normally distributed, LSE becomes a maximum likelihood estimator (textbook knowledge) – this is the theoretical basis for the use of LSE in many practical applications. The fact that MLE performs well for long time series is not very surprising. MLE is asymptotically effient, i.e., for long time series no other estimator can have narrower confidence intervals (it approaches the Cramer Rao bound). The paragraph of course cannot go into so much detail, but it should stand on some better foundations. Maybe David Stephenson could look over it? [Douglas Maraun, Germany]	Taken into account. The description is clarified. However, it is absolutely wrong to say that the LSE is "a very ad-hoc method without a statistical foundation." Even without an assumption of normality on the regression residuals, the LSE method provides the so-called Best Linear Unbiased Estimator (BLUE) for regression coefficients. This property of the LSE is guaranteed by the Gauss-Markov theorem in mathematical statistics.
2-384	2	8	38	8	52	This section must say something more about the AR(1) assumption and its validity. Currently this just says that AR(1) is assumed. Can the validitiy of this assumption by tested? If not, say why not (e.g. difficulty in separating internal variability from climate change)? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Rejected. AR(1) for the trend residuals is a reasonably standard assumption in climatology for routine data analyses, therefore it is used here. It is beyond the scope of this report to reexamine or to prove it. Description of AR(1) processes intended for a wider audience is added.
2-385	2	8	38	8	52	The authors discuss several different methods to model time series data including non-parametric (e.g. linear trend based on Kendall's tau from Sen (1968)) and least squares based methods which assume the data could be represented by a linear trend line plus residuals that follow and AR(1) process. The blanket application of these methods to represent time series data is imprudent and ill-advised; one must first carefully examine and assess the data series before deciding on an appropriate statistical model. The reason for this is that linear regression methods are designed to detect a monotonically increasing or decreasing trend in a data series rather than an abrupt step change and, more importantly, cannot distinguish between the two. Thus, there is the real danger that these methods will indicate the presence of a linear trend when in fact a step change has taken place. [Reynold Stone, Trinidad and Tobago]	Taken into account. As described in the introduction, the report deals with the timeseries exhibiting generally non-linear evolution; trends for different periods provide highly-simplified, parsimonious summary of this evolution. Note that this chapter engages in the absolutely minimal analysis for describing change. No full statistical analysis is attempted.
2-386	2	8	38	8	52	It is important to note that the identification of a step change rather than a gradual monotonic trend is critically important because the implications of a step change are quite different from a gradual trend. The interpretation	Rejected. Generally, no such interpretations are intended. See also 2-385

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						of a gradual trend is that the trend is likely to continue into the future, whereas the interpretation of a step change is that the climate system has shifted to a new regime that will likely remain relatively constant until a new shift occurs (McCabe and Wolock, 2002). McCabe, G.J. and D.M. Wolock. 2002. A step increase in streamflow in the conterminous United States. Geophys. Res. Lett., 29,38-1 – 38-4, doi:10.1029/2000GL015999. [Reynold Stone, Trinidad and Tobago]	
2-387	2	8	38	8	52	To overcome this common statistical pitfall and to ensure selection of an appropriate model to represent the data, it is essential to test the time series for randomness before deciding on the choice of an appropriate statistical model. Harvey (1993) emphasized this point by stating: “Testing for randomness is a fundamental aspect of time series analysis”. Harvey, A.C. 1993. Time Series Models. Harvester Wheatsheaf, New York. Kendall(1976) also emphasized the need to test for randomness and provided the following advice on the choice of appropriate tests: “The choice of a test depends to some extent on what alternative hypothesis we have in mind. The work of Neyman and Pearson in hypothesis testing has driven home the message that one does not test a hypothesis all by itself, but only in comparison with other possible hypotheses.” Kendall, M. G. 1976. Time-Series. Charles Griffin and Company Ltd, London. [Reynold Stone, Trinidad and Tobago]	Taken into account, combined with 2-385. Note that this chapter strives to summarize large amounts of climate variability data in a uniform way. Simply fitting a straight line to a timeseries and reporting its slope is serving this purpose well. To put the calculated slope into the context of other kinds of variability present in the given timeseries, a null hypothesis of no linear trend is used throughout. It's tested under assumption that residuals are from the AR(1) process. In practice, the rejection of the null hypothesis usually points to significant changes (which in fact might occur in the form of a step change or some other non-linear shape) in the mean of the climate variable and thus might need some attention or explanation.
2-388	2	8	38	8	52	Three relevant tests of randomness that can assist in differentiating between linear trends and step changes are the turning point test, the difference sign test, and the Pettitt test. At the minimum, these three tests should be performed prior to linear regression analyses. There’s also a need to perform diagnostics checks on the residuals after a linear regression model is fitted. These procedures must be included in a robust methodology for reliable detection of patterns in climate series data. [Reynold Stone, Trinidad and Tobago]	Taken into account. Combined with 2-385 an 2-387.
2-389	2	8	41	8	41	Consider changing "most difficulty" to "greatest difficulty" or "greatest challenge". [Dian Seidel, USA]	Accepted
2-390	2	8	41	8	41	Replace "most" with "greatest". [Robert Waterland, United States of America]	Accepted
2-391	2	8	43	8	44	About the sentence:"Serial correlation has been dealt with by assuming that the residuals are an AR(1) process…", the statistical symbol AR was not defined previously. So, it could be included before it is used here or after (with an explanation in parenthesis). [Rubén D Piacentini, Argentina]	Accepted. AR(1) process is defined.
2-392	2	8	44	8	44	What is an AR(1) process? I doubt the reader will be familiar with that term at this stage. [Klaas Folkert Boersma, Netherlands]	Accepted. AR(1) process is defined.
2-393	2	8	44	8	44	I think that it is better to clarify what do you mean by AR(1) process [Celeste Saulo, Argentina]	Accepted. AR(1) process is defined.
2-394	2	8	44	8	45	I doubt many readers know what the terms "AR(1) process" or "prewhitening the data" mean. Point to the Appendix [Dale Hurst, United States of America]	Accepted. AR(1) process is defined.
2-395	2	8	50	8	52	This is not accurate. Trend values calculated by the current method of Chapter 2 are similar to the REML method used in AR4 etc, but uncertainties in the trends are markedly smaller than result from the version of REML used there. This is because uncertainties in annual values are now excluded from trend calculations. Comparison of Chapter 3 AR4 etc and this chapter confirms this. This must be commented on. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected. It is unclear what is the basis of the reviewer's claim: no specific contradictions between this chapter vs AR4 Ch.3 are pointed out. On the other hand, comparisons given in Appendix 2.A show the similarity of the results from the AR4 with those obtained by OLSdofrST (the method used in this chapter), including calculated uncertainties.
2-396	2	8	50	8	52	Despite the reference, I do not find anything about the method of Santer et al. (2008) in the Appendix. [Uwe Stoeber, Germany]	Accepted. Correction is made.
2-397	2	8	51	8	51	When describing reanalyses, it says they are "dynamically consistent". It should be clear that they are not consistent with the dynamics of the real world, only consistent with the dynamics of the NWP model, which is an approximation to the real world. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account on page 9, line 51.

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2-398	2	8	54			This section on non-linear trends reinforces Comment 32 above. [Forrest Mims, USA]	Noted
2-399	2	8	56	8	56	Put "a priori" into italics. [Robert Waterland, United States of America]	Accepted
2-400	2	8			9	Box 2.2 This discussion of fitting lines to data fails to adequately address the end effects issues. These are discussed in Appendix 3.A of AR4. [Kevin Trenberth, USA]	Rejected. The first paragraph under "Non-linear trends" subtitle mentions the effect of additional data points.
2-401	2	8				can be built from redundancy of efforts to generate products. [Klaas Folkert Boersma, Netherlands]	See 2-361
2-402	2	9	1	9	2	Missing full stop. And the following sentence is poorly constructed: better perhaps to reword to state that the residuals are not well represented as a simple … process [Elizabeth Kent, England]	Editorial: full stop added. Rejected: suggested changes to the sentence - they would alter the intended meaning in a way that is not useful.
2-403	2	9	1	9	4	The claim in Box 2-2 that the residuals from a linear trend "are often nonstationary and do not follow a simple ar or ma process…" contradicts the claim in Box 2-1 that the AR1 model is adequate for linear trend models! If the residuals in climatic data are often "nonstationary" it implies that you cannot estimate a trend coefficient, since any linear estimator is biased and inconsistent--in other words it never converges as the sample size grows. If you have nonstationary residuals you can only talk about trends in integer or fractional difference terms, if you can find an order of integration that yields stationary residuals. Did you really mean to suggest all this? If you want to discuss these issues then do a proper literature survey. But the contradiction with Box 2-1 will be pretty glaring. [Ross McKitrick, Canada]	Rejected. There is no claim in Box 2.1 that "the AR1 model is adequate for linear trend models." There is no such a claim in Box 2.2 either. Please see 2-387.
2-404	2	9	1	9	4	The nonlinear trend Box doesn't make a useful contribution to the discussion. While it is true that a smoothed curve has advantages for summarizing the data, it doesn't typically provide answers to questions of interest in the IPCC report, namely, does series A trend up at a statistically significant rate, or trend up faster than series B, or has the linear trend gone up in recent decades, etc. The Seidel and Lanzante (2004) paper is more useful in this respect, though it's not as useful as the Mills paper cited in cell 15, and the other papers summarized therein. It is noteworthy that the rest of the chapter, and the rest of the AR5 (as much as I read anyway) does not use nonlinear trend analysis. It would be more useful to add a few sentences to the end of Box 2-1 saying somethig like "Use of a linear trend model should take into account the time scale over which the linearity assumption remains reasonable. In some cases it can be useful to allow for a shift in the mean or trend parameters (Seidel and Lanzante 2004), but doing so requires the assumption that the break data is known. If the break date is unknown and is imposed empirically the critical values of the tests can change considerably (Saginsoy and Vogelsang 2011)." ref. in cell 19. [Ross McKitrick, Canada]	Rejected. Box 2.2 describes a uniform way in which linear trend slopes and their uncertinties are calculated for individual timeseries in Chapter 2. It does not deal, in particular, with comparison tests for trends from different timeseries.
2-405	2	9	1	9	4	Reference: Saginsoy, Ozgen and Timothy Vogelsang (2011). TESTING FOR A SHIFT IN TREND AT AN UNKNOWN DATE: A FIXED-B ANALYSIS OF HETEROSKEDASTICITY AUTOCORRELATION ROBUST OLS-BASED TESTS. Econometric Theory 27, 2011, 992–1025. [Ross McKitrick, Canada]	Noted
2-406	2	9	4	9	4	is → are [Peter Burt, UK]	Accepted
2-407	2	9	4	9	4	Change "data is" to "data are" and check the entire chapter for subject-verb agreement with the plural noun data. [Dian Seidel, USA]	Accepted
2-408	2	9	4			I was under the impression the word "data" is plural and therefore it should read as "data are." [Shouraseni Roy, USA]	Accepted
2-409	2	9	6	9	7	Another alternative approach is time series low-pass filtering. [Christian-D. Schoenwiese, Germany]	Noted
2-410	2	9	6	9	24	Why go into this much detail about a method that is not used in the rest of the chapter? [Melissa Free, USA]	Taken into account. It is planned to be used in SOD.
2-411	2	9	6			It's a very important part as many figures show non-linear trends and a lay reader may try to understand that paragraph. Without the references it's impossible to know what you are doing to get a smooth trend and lay reader wouldn't check the references (I didn't). The details are not important but a bit about how the trend is calculated could be useful: - is the average calculated with weight corresponding to confidence/uncertainty/etc.?	Rejected. No space for more detail and therefore references to the papers have been added.

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						- does each set of raw data have the same weight for the average? If many datasets use the same raw data are each of those sets weighted down? [Francois DANIS, France]	
2-412	2	9	7	9	7	General comment about Figure numbering: in Chapter 1, they are referred to as 1.x, except for the FAQ section, where the figure is referred as Figure 1. Then in Chapter 2, there is a Figure 1 for the BOX 2.2, so the figure numbering is confusing and will lead to figures with the same numbering in different sections-chapters. Same comment WRT Tables numbering. [Celeste Saulo, Argentina]	Editorial
2-413	2	9	8	9	8	HadCRUT4 is not presented before. Will this chapter have a list with acronyms? [Celeste Saulo, Argentina]	Taken into account. Dataset is explained in Section 2.2.3 (2.4.3 in SOD). Reference added.
2-414	2	9	12	9	13	Here and in the caption of Box 2.2, Figure 1, the smoothing window should be described. [Klaas Folkert Boersma, Netherlands]	Rejected. There is no explicit smoothing "window" in smoothing splines.
2-415	2	9	12	9	13	Yes smoothed time series' can be very useful. It might be useful to mention here the limitations of using trends. The methods are unable to distinguish between a "forced" climate response or one that is from "internal variability". [Gareth S Jones, UK]	Taken into account. See the introduction.
2-416	2	9	12			"This smoothed time series (Wood, 2006) is arguably a more justifiable alternative to a linear trend fit." However, see Cohn and Lin! [Philip Lloyd, South Africa]	Noted. Unclear what is suggested.
2-417	2	9	15	9	41	Working out the trends and uncertainties is fine, but you're using OLS. Why not additionally calculate the differences that AR4 did? These didn't assume linearity. You could compare with what AR4 did as well. Suggest a difference based on (2001-2011) minus (1850-1899) or (2007-2011) minus (1850-1899). This way the difference between the five years before AR5 can be compared with the 5 years before AR4. [Philip JONES, UK]	Taken into account.
2-418	2	9	16	9	18	"…both assuming AR1 errors." ?? These are least squares trends. The trend coefficient is not affected by the assumption of AR1 errors unless you are using a GLS estimator, but Box 2-1 said you are using the Santer method. Only the variances will be affected by the AR parameter, but you aren't presenting those estimates. [Ross McKitrick, Canada]	Accepted. Correction is made.
2-419	2	9	16	9	18	About the Figure 1 of Box 2.2: This is one of the most important figures of the AR5-WRI, so I consider that all the available information must be included in it. For example, the GISS/NASA (ref: http://data.giss.nasa.gov/gistemp/2011/. In particular: Figure 3. Global surface air temperature anomalies relative to 1951-1980 base period for (a) the 12-month running mean, and (b) the 60-month and 132-month running means). Also: i) Please, explain better the title: "(Top): HadCRUT4 global annual mean data from 1850 to 2010" as was done in the GISS/NASA case, adding "air temperature", between "global annual mean" and "data"; ii) include the unity °C in the vertical axis and put "Temperature" with all its word, due to the significance of the figure; iii) adds the marks (tips) in the opposite lines of each axis; iv) include in the legend to the figure the temperature value, with all its significant figures, that was used for the reference period and indicates this period, iv) include the error bars or at least indicates in the figure caption the absolute values of the uncertainties at the begining (1850) and the end (2010) of the period, since the expresion "also assuming AR1 errors" only inform on the way in which the error were determined but do not include specific numbers, v) incorporate another linear trend that corresponds to the last century, which is most easily compared with other periods than the 1901-2010 one. [Rubén D Piacentini, Argentina]	Taken into account. Apparently this example is mistaken with our key figure 2.7. Nevertheless, a more complete description has been added.
2-420	2	9	16	9	18	Suggest that the caption states exactly what has been plotted from the HadCRUT4 ensemble, to avoid later confusion. Readers may be interested to try and replicate the figure. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-421	2	9	16	9	34	Is the HadCRUT4 dataset publicly available? [Geert Jan van Oldenborgh, Netherlands]	Noted. Yes, from http://www.metoffice.gov.uk/hadobs/hadcrut4/
2-422	2	9	16	9	34	Assuming the HadCRUT4 dataset is constructed along similar lines as the HadCRUT3 dataset, it is well-known that he latter understimates the trend as it does not include the Arctic regions which have seen above-average warming rates (eg Simmons et al, 2010, Hansen et al, 2010). Please use a dataset here that also	Noted. HadCRUT4 has many more stations in this region.

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						includes an estimate of the temperature in the Arctic (and Antarctic) or mention explicitly that this dataset excludes this region to a large extent. [If HadCRUT4 does include this pleaes disregard this comment] [Geert Jan van Oldenborgh, Netherlands]	
2-423	2	9	16		18	Box 2.2 Fig 1 should have the result from the low pass filter used in AR4 added [Kevin Trenberth, USA]	Taken into account.
2-424	2	9	17	9	17	"AR1" should be "AR(1)" here and elsewhere, especially since at times AR1 is also refer to the IPCC First Assessment Report. [George Kiladis, USA]	Accepted
2-425	2	9	17	9	18	"AR1 errors" is the same as "AR(1) errors"? [Celeste Saulo, Argentina]	Taken into account. AR(1) consistently used now.
2-426	2	9	20	9	24	You refer to "95% uncertainty limits" in the text but "95% confidence limits" in the Figure caption. The IPCC does not use "uncertainty" and "confidence" to mean the same thing, so these terms are not interchangeable. Presumably you refer to confidence intervals. But there are no confidence intervals in the top figure, and in the bottom figure, far more than 5% of the data fall outside the boundaries. It's not clear what these graphs are therefore showing. [Ross McKitrick, Canada]	Taken into account. Confidence interval used consistently
2-427	2	9	20	9	24	The Santer method is not valid for comparing trends across different data sets, or sub-periods as in this case, even under the AR1 assumption. See McKitrick, McIntyre and Herman (2010) "Panel and Multivariate Methods for Tests of Trend Equivalence in Climate Data Sets". Atmospheric Science Letters, DOI: 10.1002/asl.290. (http://onlinelibrary.wiley.com/doi/10.1002/asl.290/abstract) [Ross McKitrick, Canada]	Noted. This chapter does not present a formal comparison of trends from different timeseries.
2-428	2	9	22	9	23	Actually the difference between the two trends do not show exactly the change in means between the early 20th Century and recent decades because the longest period contains the shortest one. To show the change in means between the early 20th Century and recent decades, two distinct period should have been used, better if of equivalent duration, as for instance 1901-33 and 1979-2010, or similar. [Claudio Cassardo, Italy]	Taken into account. It is not the intention here to show the change in means between these two periods by considering the different trend values. Sentence on this comparison is deleted.
2-429	2	9	22	9	24	In the discussion of statistical significance derived from a t-test, there should be some additional information given about the degrees of freedom used (for the sake of completeness). [Jeffrey Taylor, United States of America]	Taken into account. This comparison has been excluded.
2-430	2	9	23	9	23	shouldn't this be "statistically significant at the 99% confidence level" ? [Dale Hurst, United States of America]	Rejected. At the 1% level, as specified.
2-431	2	9	27	9	33	"The graphs are of "Mean Global Temperature anomalies" They are not global temperatures at all, but the result of multiple manipulation of many poorly charcterised and variable daily measurements of maximum and minimum temperature.. The errors must surely be much greater than the figures suggested [VINCENT GRAY, NEW ZEALAND]	Noted.
2-432	2	9	27	9	33	Misleading comparison of short and long trends. Longer-term trends are usually smaller. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Noted. The example indicates the difference between the two estimation methods. No intention to draw conclusions from the two different periods. Sentence on this is deleted.
2-433	2	9	28			Is it OK to define OLS for only time in a figure caption? [Larry Thomason, United States of America]	Accepted. Changed in LS which has been defined in the text
2-434	2	9	30	9	30	Delete "in order" and check all instances of "in order to", which usually can be shortened to "to" [Dian Seidel, USA]	Accepted. One other instance found.
2-435	2	9	30	9	34	.086 in the legend is not consistent with 0.084 in the table. Also, the legend should specify that the 0.086 (or 0.084) is for the mean change starting in 1901. [Marcus Sarofim, USA]	Rejected. These are different quantities.
2-436	2	9	33	9	33	Two questions: How do you extract a single trend value using the splines method? That is not explained in the Box. Also, what is the basis for deciding how many significant figures to report in trend estimates? Should it not be related to the native observational uncertainty? Throughout the chapter, there are many instances of trends and their confidence intervals are reported to precisions that seem unreasonably high. [Dian Seidel, USA]	Taken into account. (1) For both methods, "change estimates" are shown in the table. In case of the OLS method, it is the trend slope, but in case of splines, it is [s(t2)-s(t1)]/(t2-t1) where s(t) is the smooth spline estimate of temperature for year t. (2) Number of decimal places in trend estimates, as table formats in

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							general, are keeping with AR4, whenever practicable.
2-437	2	9	39	10	38	It is very questionable whether the reanalysis programmes can really claim to have extended weather into climate. Even some of the latest products (e.g. CFSR with which I am most familiar) does not claim to be able to smoothly cross from one period of reanalysis to the next even with a whole year of overlap between the two contiguous periods. They are indeed giving much greater reliability at 5day prediction; and are starting to show some clues as to seasonal swings. But they show no signs of decadal predictions and do not appear to have seen significant changes in weather patterns during the periods of training - they seem to have tracked the observed temperatures without establishing any correlation with global pressures patterns that the climate models predict. I am not even certain that the drafter of this section understands what is meant by 'assimilation' in this context - perhaps it needs to be spelled out. Yes, the programmes are doing a magnificent job of improving our understanding of weather; no, they are not giving us global temperature patterns at the level of accuracy that we need to track what is presently measured. The fact that reanalysis has been given a whole box to itself almost certainly overempahsizes its relevance to climate change, and there may well be merit in considering removing this box in its entirety. [Philip Lloyd, South Africa]	Noted - Reanalyses cannot be ignored as a large body of literature is basing on reanalysis data. This Box gives background information to the general suitability of reanalyses, further details on individual applications follow at the appropriate places. The Box is rewritten.
2-438	2	9	39	10	38	The inclusion of this box on reanalyses is a good idea. However, I feel it takes too sanguine a few towards their applicability for this assessment. See my comment # 6 above. [Dian Seidel, USA]	Taken into account - The box is rephrased.
2-439	2	9	41	10	36	One of the problems of each reanalysis dataset is the varying number of stations used, especially in regions poorly covered. This is true not only for the ERA-40 dataset. [Claudio Cassardo, Italy]	Taken into account - reworded.
2-440	2	9	43	9	43	Consider deleting "valuable and" as this is a judgment that is not held by a majority of experts. [Dian Seidel, USA]	Taken into account - reworded.
2-441	2	9	43	9	44	Replace "Dynamical reanalyses constitute an increasingly valuable and utilized resource for assessing weather and climate phenomena." with "Dynamical reanalyses are a valuable, and increasingly used, approach for assessing weather and climate phenomena." [Robert Waterland, United States of America]	Taken into account - reworded.
2-442	2	9	43	10	36	I suggest that, for completeness, a short acknowledgement of the effects of model biases on the output of reanalyses, particularly in the face of sparse observations, would be appropriate. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Taken into account -"moadel biases" is added.
2-443	2	9	44	9	46	Replace "Although used in previous assessments, their characteristics have not been clearly outlined. Given their more abundant use in this assessment their characteristics are outlined here." with "Although dynamical analyses were used in earlier IPCC assessments, their characteristics have not previously been clearly stated. Given their more abundant use in this assessment, we more completely define their characteristics here." [Robert Waterland, United States of America]	Editorial
2-444	2	9	45	9	45	Consider replacing "abundant" with "frequent" [Dian Seidel, USA]	Editorial
2-445	2	9	48	9	48	Replace "and complement" with ", but complement,". [Robert Waterland, United States of America]	Editorial
2-446	2	9	49	9	51	Replace "At the most basic level they use a modern day data assimilation scheme and weather forecasting model to integrate all historically available observations from multiple disparate sources and create a dynamically consistent estimate of the past atmospheric states." with "Rather, they aim to produce a detailed, continuous, reconstructed estimate of historical climate data that is dynamically consistent and also consistent with actual climate observations via a process know as data assimilation. This is accomplished by using a predictive model, such as a weather forecasting model, to integrate all historically available observations from multiple disparate sources." [Robert Waterland, United States of America]	Taken into account - reworded.
2-447	2	9	51	10	2	Should also add that care has to be taken if one ends up just comparing one model with another in areas with no "traditional" observations. See page 2-66 L45-48 for example of shortcoming of reanalysis. [Gareth S Jones, UK]	Noted but text no longer in draft.
2-448	2	9				Box 2.3: Too much emphasis on 'reanalysis'. Description is vague and woolly – eg what does 'dynamically consistent' mean? [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Rephrased

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2-449	2	9				Fig. 1 of Box 2.2: It would be useful to label the y-axis with units. I assume it is kelvins/degrees Celsius, but this should be stated. [David Pearson, United Kingdom]	noted
2-450	2	10	2	10	2	Without necessarily going into detail it would be fair to at least point out the potential dangers of "model verifying model" here. [George Kiladis, USA]	Noted but text no longer in draft.
2-451	2	10	6	10	6	Replace "reanalyses MERRA and ERA-Interim" with "MERRA and ERA-Interim reanalyses". [Robert Waterland, United States of America]	Editorial
2-452	2	10	7	10	8	"The NCEP/CFSR reanalysis uses a coupled ocean-atmosphere assimilation system (Saha et al., 2010)" This does not make sense as it stands.I think it would be better stated as "The NCEP/CFSR reanalysis uses a coupled ocean-atmosphere-land-sea ice system with assimilation of satellite radiances by the Grid-point Statistical Interpolation scheme (Saha et al., 2010)" [Philip Lloyd, South Africa]	Taken into account - reworded.
2-453	2	10	7		28	The reanalyses are evaluated from exactly these points in Trenberth et al 2011. line 28 Not just "early reanalyses" but all reanalyses. See Trenberth et al. 2011, Trenberth, K. E., J. T. Fasullo, and J. Mackaro, 2011: Atmospheric moisture transports from ocean to land and global energy flows in reanalyses. J. Climate, 24, 4907-4924. [Kevin Trenberth, USA]	Taken into account - reworded.
2-454	2	10	10	10	10	It is my understanding that C20R does not assimilate SST and sea ice; rather they provide a lower boundary forcing, as for an atmosphere-only GCM. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reworded.
2-455	2	10	11	10	12	"This variety of groups and approaches provides better estimates of uncertainties for any given application" – better than what? Presumably the use of a single reanalysis product? [Colin Morice, UK]	Taken into account - reworded.
2-456	2	10	11	10	12	This statement is problematic, as it assumes that more aproaches necessarily means a better uncertainty estimate. If a lot of approaches give similar estimates, but all are similarly wrong, the uncertainty will be judged to be smaller than it really is. [Dian Seidel, USA]	Taken into account - we use the term "robustness" rather than "uncertainty"
2-457	2	10	15	10	17	Box 2.3, Table 1: When referring to BAMS State of the Climate 2010 (or other years) can you specifically cite the section rather than the whole document both because it is a very long document and so finding the relevant section without a citation is difficult and because it is fair give credit to the authors of those sections. e.g. Here it should be Dee, D, P. Berrisford, M. G. Bosilovich, M. Chelliah, G. Compo, A. Ebita, P. D. Jones, S. Kobayashi, A. Kumar, G. Rutledge, S. Saha, H. Sato, A Simmons, C. Smith and R. Vose, 2011: [Global Climate] The use of reanalyses data for monitoring the state of the climate [in .State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S33-S35. [Kate Willett, UK]	Editorial
2-458	2	10	15	10	19	Shouldn't NOAA ESRL PSD be acknowledged for the 20th century reanalysis (as well as CIRES). The web page is http://www.esrl.noaa.gov/psd/data/20thC_Rean/ [Karen Rosenlof, United States of America]	Taken into account - reworded.
2-459	2	10	17	10	17	In this Table ERA-INTERIM starts in 1979. Another 10-years are available. [Philip JONES, UK]	Taken into account - reworded.
2-460	2	10	17	10	18	There does not appear to be any logic in this table! The material is not presented alphabetically by Institution, or re-analysis, or period, resolution or reference. [Peter Burt, UK]	Taken into account - The table is sorted by the start year of the reanalysis, which is now stated in the caption.
2-461	2	10	20	10	20	Process understanding? [Elizabeth Kent, England]	Taken into account - reworded.
2-462	2	10	20	10	26	Reanalyses have also been used to characterise and adjust observations, this could be mentioned here. [Elizabeth Kent, England]	Rejected - Materia is not substantial would lead too far.
2-463	2	10	20			This section is well written in terms of review of literature and existing datasets. I would think this would be very useful for potential researchers. [Shouraseni Roy, USA]	Thanks.
2-464	2	10	21	10	21	Consider changing "active" to "open" as a modifier of "question". [Dian Seidel, USA]	Taken into account - reworded.
2-465	2	10	25	10	25	Bosilovich et al? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-466	2	10	28	10	36	This paragraph doesn't give adequate justification for relying on the reanalyses later in the chapter, especially	Studies using reanalyses are reviewed in AR5. The

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						for upper-air variables near the poles. [Melissa Free, USA]	meaning of the box is not to give justification for the use of reanalyses, but should introduce the data sets and point to problems. It is repharsed to reflect that more caution is needed.
2-467	2	10	28	10	36	The use of Reanalyses for climate trends is properly cautioned. But this leads a reader to wonder: why use them at all? Some final statement on restricted use where their advantages outweigh their limitations is needed. [Bruce Wielicki, USA]	Studies using reanalyses are reviewed in AR5. The meaning of the box is not to give justification for the use of reanalyses, but should introduce the data sets and point to problems. It is repharsed to reflect that more caution is needed.
2-468	2	10	29	10	29	Products do not learn [Elizabeth Kent, England]	Taken into account - reworded.
2-469	2	10	29	10	29	"products have learned" makes no sense. People learn, animals too, but products? [Dian Seidel, USA]	Taken into account - reworded.
2-470	2	10	29	10	32	Replace "As subsequent products have learned from these pioneering efforts the ability to determine trends and quantify the uncertainties has improved. This has led to a more nuanced position whereby trend adequacy depends upon the variable under consideration, the time period and the region of interest." with "Subsequent products have improved capability and skill for determining trends and quantifying uncertainties. This has led to a more nuanced position where the quality of derived trends depends upon the variable under consideration, the time period and the region of interest." [Robert Waterland, United States of America]	Taken into account - reworded.
2-471	2	10	32	10	33	The assertion that ERA-40 performs well for surface air temperature and humidity would be more convincing if more studies could be cited, and if they were studies by investigators not directly involved with the project. [Dian Seidel, USA]	Taken into account - reworded.
2-472	2	10	32	10	36	I think it should refer to both ERA-40 and ERA-Interim (line 32). It is unclear whether humidity is being referred to is over both land and ocean (line 33). My reading of Simmons et al. (2010) is that the 2m analyses used in this study are derived directly from an optimal interpolation analysis of the screen-level data with the lowest model-level field used as a background [paragraph 9]. Screen level humidities are directly assimilated into the full variational analysis (for ERA-40 all hours data, for ERA-Interim daytime only) [paragraph 11]. From this I would argue that the observations of temperature and humidity are probably not "quasi-independent" from the reanalysis 2m fields. [Elizabeth Kent, England]	Taken into account - reworded.
2-473	2	10	33	10	33	– → , [Peter Burt, UK]	Editorial
2-474	2	10	34	10	35	Caused controversy statement would benefit from (succinct) elucidation [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reworded.
2-475	2	10				Box 2.3, Table 1: The ERA-Interim Reanalysis is now available since 1979. [Philip Klotzbach, USA]	Taken into account - reworded.
2-476	2	11	1	14	4	It is very pleased to see that there are rapid progresses in development of the global and regional/national temperature datasets, and many sections/ paragraphs are used to introduce these important datasets, which is very important to support main observation analysis results. We see global land dataset like CRUTEM,GHCN,GISS, Beckerley et al,maybe there are some new ones later, have been introduced in the AR5, but as to the regional/national datasets, most of the attentons are focused on the USHCN, and only two other datasets over Rurope, East Africa are mentioned. In fact, during AR4 to AR5, many regional/ larger-area national datasets are developped and used to studies on climate change. take China for example, CHHT1.0 (li et al, 2009; Li et al, 2010), and recently China National Meteorological Information Center is, developing the 2nd generation homoginized temperatue dataset , those are important contribution to the global dataset. So I hope there will be mentioned in the AR5. (Refereences for CHHT dataset: Li Q., Zhang H., Chen J., Li W., Liu X. and P. Jones, 2009, A mainland China Homogenized Historical Temperature Dataset of 1951-2004, Bull. Amer. Meteor. Soc., doi: 10.1175/2009BAMS2736.1; Li Q., Dong W., Li W., Gao X., Phil Jones, David Parker and John Kennedy, 2010, Assessment of the uncertainties in temperature change in China during the last century, Chin. Sci. Bull. Volume 55, Number 19, 1974-1982, DOI: 10.1007/s11434-010-3209-1) , [Qingxiang Li, China]	Noted. We have included several of these in the redrafted version. The original text was largely driven by CLA/LA knowledge which was incomplete and this and other suggestions to broaden the regional temperature efforts paragraph have been enacted upon.

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2-477	2	11	1			Most readers will find surprising that this section does not mention the very recent evolution of temperatures in the last 15 years, as this is one of the issues that are being more contentiously debated beyond the scientific quarters. If this IPCC report is meant to be a guidance for society as a whole, it should comment on the recent temperatures trends [Eduardo Zorita, Germany]	Noted. Recent changes are discussed at several points throughout the report including in this section and their discussion here has been modified.
2-478	2	11	3	11	3	Land-Surface Air Temperature (LSAT) [add acronym - this report is full of acronyms thus it could be better, the first time, explain its meaning] [Claudio Cassardo, Italy]	Noted. We would rather not place an acronym in the title so have rather added the acronym on the first mention in the main text section.
2-479	2	11	7	11	8	Misleading comparison of short and long trends. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Rejected. Here we are characterizing the AR4 conclusions so no changes can be made as it would then no longer be a faithful representation of the findings of AR4.
2-480	2	11	7	11	14	It is the mean global anomaly that has increased, It is not correct to assume that this implies an increase in mean global temperature because this cannot be measured [VINCENT GRAY, NEW ZEALAND]	Rejected. The global anomaly increase can only occur if the global mean temperature has also increased. To state otherwise cannot, logically, be correct.
2-481	2	11	7	11	14	Is this paragraph saying the global mean land-surface temperatures have experienced multi-decadal warming, or is it saying that land-surface temperatures throughout the globe have all experienced warming? Figure 2.8 shows that a small set of regions that have not warmed. [Robert Waterland, United States of America]	Noted. As made clear by references to global this is referring to changes at the global level. No changes made.
2-482	2	11	7	22	23	It is good to see discussion of observational uncertainties in this chapter, but very little use is made of the available estimates of these in these sections. Figure 2.6 is very useful in depicting the uncertainties in global average surface temperature anomaly on decadal scales. The uncertainties in the trends quoted in Tables 2.2, 2.4 and 2.6 do not include the effect of observational uncertainty and might give a misleading impression, although it is clearly stated that they do not include it. The type of information given about observational uncertainty is not consistent between data sets, which I realise makes it difficult to compare the results. Showing the spread between different estimates as in Figure 2.7 does go some way towards illustrating the structural uncertainty. Perhaps the authors could choose estimates for one or more data sets to use to illustrate the typical sizes of the other components of the uncertainties for, say the annual global average of the surface temperature parameters, and consider how the information might be conveyed? [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Noted. This is a vexed issue, as the reviewer points out. Our concern has been that introducing uncertainty estimates that are far from comparable will do more to obfuscate than it will do to help the reader. The issues surrounding uncertainty could fill a length longer than the current chapter and still remain contentious. We would prefer to retain the current approach despite its imperfections, recognizing that it is in reality the best of an imperfect set of solutions.
2-483	2	11	7	29	7	I would suggest re-arrange the sections by put current 2.2.4 Upper air temperature as 2.2.2, the later two sections (urban head island effects, and Sea surface and marine air temperature) read like sort of explanation of the air temperature [Xuemei Wang, China]	Noted. We prefer to keep the order as is currently placed as we believe a reader would expect to read about surface temperature changes first and then vertical profiles through to the highest level.
2-484	2	11	8	11	8	"past Century" is perhaps ambiguous. Last 100 years? Or 1900-1999? [Elizabeth Kent, England]	Accepted
2-485	2	11	8	11	10	The sentence mentions two very specific actions in production of revised datasets and increase in data density and then throw in a very general "new dataset efforts". This needs more specificity or possible removal. [Michael Brewer, United States of America]	Taken into account. Combined with 2-487
2-486	2	11	9	11	9	"digital data density" might need explaining [Elizabeth Kent, England]	Taken into account. Combined with 2-487
2-487	2	11	9	11	9	Consider changing "increase in available digital data density" to a clearer phrase. Is "more digital data records" what is meant? Or "digital data archives with better spatial resolution"? [Dian Seidel, USA]	Accepted. 'more digital data records'.
2-488	2	11	9	11	9	"revised" would it be better to say "improved"? [Xuemei Wang, China]	Noted. Improved implies a value judgement which would be hard to defend. While the dataset creators may maintain their new products are improved it does not follow logically, or at least cannot be objectively and undeniably proven, that they actually are better estimates of the single real-world trajectory of the climate system

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2-489	2	11	13	11	13	Is it worth stating that GISS is unaltered since AR4? There are newer analyses from NCDC and CRUTEM. Again here you say virtually certain when it should be unequivocal. [Philip JONES, UK]	Rejected. GISS is not unchanged since AR4. They have used the GHCNv3 product as a basis and with new nightlights urban adjustments so their product has changed substantially. Virtually certain is consistent with the uncertainty guidance whereas unequivocal is not. We are bound by the uncertainty guidance.
2-490	2	11	14	11	14	Multidecadal warming - add since about which year. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected. It is impossible to assign a single year or even approximate year given the divergence in behaviour seen in the early period of record between the available estimates.
2-491	2	11	16	11	16	Change "since the mid-1800s to early 1900s" to "starting in the mid-1800s or early 1900's" [Dian Seidel, USA]	Noted. Text has been moved to the appendix and substantially reworked and a figure added there. This additional space enables some expansion and addressing of concerns raised by the various reviewers.
2-492	2	11	16	11	16	Early 1900s ? Or 2000s ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-491
2-493	2	11	17	11	18	This was the excuse of CRU but they were unable to show any confidentiality agreements. Remove this sentence. For background see http://climateaudit.org/2012/01/08/nature-and-the-inundation-legend/ and http://climateaudit.org/2010/01/01/sent-loads-of-station-data-to-scott/ [Marcel Crok, The Netherlands]	Taken into account. Combined with 2-491
2-494	2	11	17	11	18	I didn't understand the sentence starting "Availability historically …." [Elizabeth Kent, England]	Taken into account. Combined with 2-491
2-495	2	11	17	11	18	Change "latencies" to "delays". [Dian Seidel, USA]	Taken into account. Combined with 2-491
2-496	2	11	18	11	18	Change "non-digital" to "paper" [Dian Seidel, USA]	Taken into account. Combined with 2-491
2-497	2	11	22	11	41	What do all the acronyms stand for? I think all data set names should be written out when they appear for the first time. [Uwe Stoeber, Germany]	Noted. Where possible this is done but in some cases the dataset names have no literal meaning so it makes little sense to try to expand except in those cases where expansion aids reader understanding.
2-498	2	11	22	11	43	A comparison of surface air temperature observations from flux sites in forested land and nearby grass field stations across N America showed that surface air temperature was lower in open land than in nearby forested land. The effect was 0.85+/-0.44K north of 45degN and 0.21+/-0.53K southwards. Below 35degN there was weak evidence that deforestation leads to warming. Night-time temp changes unrelated to changes in surface albedo were an important contributor to the overall cooling effect. This issue of appropriate data is also relevant to page 2-15, lines 35-42. [Beverly Law, USA]	This comment appears to be a partial comment continued in 2-499
2-499	2	11	22	11	43	It suggests open field met stations will not represent LSAT changes that occur due to deforestation. This isn't addressed in this section, but seems like an appropriate place to point out this uncertainty (Lee et al. 2011. Observed sensitivity of local climate to deforestation in mid- and high latitudes. Nature 479: 384-387. DOI:10.1038/nature10588.) [Beverly Law, USA]	Noted. After reading this manuscript it was clear that it was outside the chapter purview of describing the observed behavior rather than a physical interpretation and so it is not referenced. It would be more applicable to Chapter 10.
2-500	2	11	23	11	23	Perhaps some more specifics can be given on the new dataset by the Berkeley-group. Now it seems to come out of thin air. [Klaas Folkert Boersma, Netherlands]	Noted. With limited space only cursory mention of any dataset is possible. In general for full understanding of any of the datasets included the reader will need to resort to the peer reviewed literature. There is some additional discussion of datasets within the appendix.
2-501	2	11	23	11	23	Rhode et al., submitted - is there any improvement of this paper? [Claudio Cassardo, Italy]	Noted. It is not our place to comment on the 'quality' of submitted paper drafts but rather to assess the

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							science contained therein. We have read and incorporated the latest version in our next draft.
2-502	2	11	23	11	23	Rhode et al should be Rohde et al. (here and elsewhere). [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-503	2	11	23	11	23	Change "from a group" to "by a group". Change "at Berkeley" to a better description of the BEST team. I don't think they are all affiliated with the University of California, Berkeley, but if they are then use the formal name of the school. [Dian Seidel, USA]	Noted. The alternative is to call them BEST which is value laden and not something that an assessment report should be in the business of doing. We see this as the least worst solution. We have made some minor changes in response to this comment.
2-504	2	11	23	11	37	„a group in Berkeley“ sounds very sloppy. The name of the group should be given. [Douglas Maraun, Germany]	Taken into account. Combined with 2-503
2-505	2	11	23	11	37	This chapter includes some 37 "submitted" (or "Submitted") but as yet unpublished sources available only on request to the reviewers. The policy of leading journals of science is that such sources should be in print by the time a paper citing them appears. For example, the policy of SCIENCE (AAAS) is, "At the time of publication, all cited references must be published." [Forrest Mims, USA]	Noted. Our use of the submitted articles is congruent with accepted IPCC protocols as documented in several locations.
2-506	2	11	23	11	37	"Berkeley" doubling. [Christian-D. Schoenwiese, Germany]	Accepted. Have removed the sentence at the head of the paragraph.
2-507	2	11	23	80	26	There is inconsistency in style when citing papers submitted: both ‘submitted’ and ‘Submitted’ are used. The consensus across other chapters appears to be ‘submitted’, but I have not flagged these instances below. [Peter Burt, UK]	Editorial
2-508	2	11	23			(Rhode et al.) is also at line 37... maybe the sentence line 23 is not necessary? [Francois DANIS, France]	Taken into account. Combined with 2-506
2-509	2	11	23			Berkeley? [Larry Thomason, United States of America]	Taken into account. Combined with 2-503
2-510	2	11	24	11	24	I'm not sure that gross details is quite the right phrase. Perhaps "a basic description of the methods" would be a better substitute. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. This material is now in the appendix.
2-511	2	11	28	11	28	What are the "largest global scales"? Is there not just one global scale, with all others being smaller scales? Either clarify the scales in question or make "scales" singular. Add a comma after "scales". [Dian Seidel, USA]	Accepted. This sentence has been modified for clarity.
2-512	2	11	28	11	29	Version 3 and version 2? For the non-specialist reader, this is hard to follow. [Klaas Folkert Boersma, Netherlands]	Noted. It is going to be difficult to completely remove such issues but efforts will be made here and elsewhere to that end.
2-513	2	11	30	11	30	Change "night light based" to "night-light-based" [Dian Seidel, USA]	Editorial
2-514	2	11	30			What does this mean? 'additional night light based urban adjustments' [Larry Thomason, United States of America]	Noted. It means in addition to the adjustments applied to GHCN. The new text is hopefully less ambiguous in this regard.
2-515	2	11	32	11	32	What is meant by "previously existing stations"? The term suggests there are previously non-existent stations. [Dian Seidel, USA]	Accepted. Removed the qualifier 'previously existing'
2-516	2	11	34	11	34	This is not an assumption, this is a FACT! Every NMS will (not shouldm but will) have better metadata histories. [Philip JONES, UK]	Accepted. Sentence has been reordered to accommodate this. This discussion now resides in the appendix.
2-517	2	11	35	11	35	Insert comma after contrast [Peter Burt, UK]	Editorial
2-518	2	11	35	11	35	Analyses might be globally consistent, but they will be substandard for many countries. [Philip JONES, UK]	Noted. This is the intended implication of the text as the reviewer clearly artiuculates. Equally, they may well be better for other countries. Language is

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							retained as it proviides a balanced view. This discussion now resides in the appendix
2-519	2	11	35	11	35	Clarity point : By remaining do you mean all other than crutem4 or all other than the ones already discussed ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence has been modified to clarify this point. This discussion now resides in the appendix.
2-520	2	11	35	11	37	The sentence that spans these lines is ambiguous. Would it perhaps be better to say "The remaining stations were homogenized in a consistent manner (ref), although the input data may already have been processed at source." [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. Some changes to account for this comment have been made, in combination with a number of other comments on these lines. This discussion has been move to the appendix.
2-521	2	11	36	11	36	that → those [Peter Burt, UK]	Editorial
2-522	2	11	36	11	36	itself → themselves [Peter Burt, UK]	Editorial
2-523	2	11	36	11	36	Very good to have acknowledged that much so-called input data is not necessarily the raw (as measured) data. Every NMS does some measure of basic checking during each month and at the end. [Philip JONES, UK]	Noted. Text has been retained in this more in-depth discussion which has been moved to the appendix.
2-524	2	11	36	11	36	Change "itself" to "themselves" to agree with plural noun data. [Dian Seidel, USA]	Taken into account. Combined with 2-520
2-525	2	11	37	11	39	What is "kriging"? I hope this is explained somewhere - any upsides/downsides to the technique? [Gareth S Jones, UK]	Noted. It is impossible to go into that degree of detail and keep with length limits so we trust the interested reader would go to the reference and references therein for further detail.
2-526	2	11	37	11	41	The new group of Berkeley is mentioned two times in the same paragraph, line 23 and here - I suggest to mention it only one time comprehensively. [Claudio Cassardo, Italy]	Taken into account. Combined with 2-506
2-527	2	11	37			I don't care for this inexact reference: 'from a group at Berkeley' Is this the same product as mention in line 23? I assume you mean University of California at Berkeley. [Larry Thomason, United States of America]	Taken into account. Combined with 2-503
2-528	2	11	39	11	41	About the sentence: " This is substantially methodologically distinct from earlier efforts so helps to better span structural uncertainty in LSAT estimates." Since LSAT has not been defined before, I suggest to include a definition after "uncertainty in" and to put LSAT between brackets: "land-surface air temperature (LSAT) estimates.", if that is the meaning, as I suppose. [Rubén D Piacentini, Argentina]	Taken into account. Combined with 2-478
2-529	2	11	39			Here it vaguely refers to the "apparently homogenous segment" in Rhode et al but this is a big issue: not so much the segments but the way they sliced and diced the series probably into far too many segments. I believe this is an issue with these analyses yet to be proven as sound. [Kevin Trenberth, USA]	Noted. The reviewer may well be correct, but equally Williams et al., 2012 at least over the contiguous lower 48 united states provides gross validation of the implied break frequency arising from Rohde et al. No changes made as such criticism cannot be unambiguously supported given the current literature. We will keep a close watch on the literature in this regard.
2-530	2	11	40	40	11	"and so helps" [George Kiladis, USA]	Noted. This passage has been deleted.
2-531	2	11	41	11	41	This seems to be the first mention of LSAT. It would be useful to define it here. (I think it means "Land Surface Air Temperature"?) [David Pearson, United Kingdom]	Taken into account. Combined with 2-478
2-532	2	11	44	11	44	About Table 2.1: Actually (and since there is no numbers for the inner part of the table) in Page 12, line 3: Berkeley 1800 39028 / 7280 1, this last number 1 is a super-script that has the following associated text at the end of page 12:". There are two versions of the Berkeley product and the version in Figures and Tables in the FOD comes from the methods paper that uses the NCDC GHCNv3 raw data holdings." Please explain the meaning of FOD. Also, it would be better to put this super-script directly below Table 1, as in other cases, like in Annex II. [Rubén D Piacentini, Argentina]	Noted. We meant first order draft here. The table will be updated with the new Berkeley effort and the need for the footnote has been removed as a result. This table is now in the chapter appendix following various other comments on these tables and author team deliberations.

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2-533	2	11	44	11	46	In this Table the station counts are not that informative and give totally the wrong impression. What is needed is a diagram of coverage through time. This is hard to do as series are also full of gaps. I've looked through the Berkeley 39K data series and 10K of them are less than 10 years. It is not possible to assess the important aspects of homogeneity with such short records. Maybe there is a need to say that station counts tell you little about the density of coverage across the world through time. The Berkeley uncertainty estimates that use these numbers are likely far too low. They don't take into account the effective number of samples. [Philip JONES, UK]	Noted. Such a figure is now included within the appendix associated with a discussion of coverage aspects.
2-534	2	11	45	11	45	Start new sentence with "Further details…" [Dian Seidel, USA]	Accepted. This table now exists in the appendix rather than main text.
2-535	2	11	46	11	46	similar issue to Box 2.3 Table 1 [Peter Burt, UK]	Insufficient context to action
2-536	2	11	46	12	1	The explanations in the table column on "Quality Control… " are too technical and need to written more simply. [Dian Seidel, USA]	Taken into account. To make the text flow better these technical details have been moved to the appendix.
2-537	2	11		11		Table 2.1: it would be helpful to include in the first column one reference associated with these data sets, even if it is in the text. [Alice Grimm, Brazil]	Noted. We will do this for each of these tables. These now reside in the appendix.
2-538	2	11				Table 2.1: Gross methodological details, could perhaps be replaced by Basic methodological description. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. These tables now exist in the appendix
2-539	2	11				Section 2.2: there is no discussion about temperature change over the Arctic and Antarctic. Papers by Steig 2009 and O'Donnell 2010 have looked at surface temperature trends over the Antarctic. The lack of temperature measurements over the polar regions remains a key uncertainty in estimating global temperatures. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. We now address this briefly in the regional section of the text.
2-540	2	12	1	12	1	Table 2.1: the footnote about the two versions of the Berkeley product should make clear whether it is the 39028 or the 7280 station version that is used in the FOD figures and tables. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-532
2-541	2	12	3	12	3	What does "broad agreement" mean? Note that it agrees to within X degrees after 1900, bigger disagreements prior to 1900. [Karen Rosenlof, United States of America]	Noted. Some changes to this sentence have been made but adding a quantitative number seems overkill as the degree of agreement is readily quantifiable from the figure itself and the table.
2-542	2	12	3	12	3	The text refers to temperature time series but anomalies are shown. I consider that anomalies are better to depict the signal, but it should be mentioned anomalies with respect to which mean are showed. Also the text, figures and corresponding captions should be corrected consistently [Celeste Saulo, Argentina]	Noted. This has been taken into account in redrafting the figure and caption.
2-543	2	12	3	12	34	Sugestion is to give the correlation coefficients between four data sets for 1850-2010 and 1901-2010, as well as 1951-2010 . [Zong-Ci Zhao, China]	Rejected. Correlation is interesting but probably does not aid reader interpretation here.
2-544	2	12	7	12	7	Missing "the" and a comma in "... early period of the record, sampling ... [Benjamin R. Miller, United States of America]	Accepted
2-545	2	12	7	12	7	Here, my earlier comment about consideration of analysis uncertainty is relevant. "In the early period of record sampling is far from global so differences are larger". I would suggest this arises because some data sets considered are reconstructed or interpolated. When observations are sparse, values inferred by different statistical means can differ more markedly. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Noted. We have tried to explicitly tease this out in the redrafted text.
2-546	2	12	7	12	7	Change :"period of record" to "years" [Dian Seidel, USA]	Taken into account in redrafting text.
2-547	2	12	7			The sentence beginning "In the early period" there is an extra word. Either the beginning "In" or the"of" (5th word in the sentence) needs to be removed to make the sentence make sense. [Michael Brewer, United States of America]	Taken into account combined with 2-546
2-548	2	12	7			what are the actual numbers? [Shouraseni Roy, USA]	Noted. The numbers are readily apparent from the figure and the trends tables. Repetition here would

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							break the flow and is not undertaken.
2-549	2	12	8	12	8	I suggest that rather than using "when meaningful global coverage ceases", which implies going forwards in time, instead "when meaningful global coverage begins" might be clearer. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-550	2	12	8	12	8	Instead of "ceases", is "begins" more correct? [Dian Seidel, USA]	Taken into account. Combined with 2-549
2-551	2	12	9	12	9	increase in variance over time? [Elizabeth Kent, England]	Noted. This sentence has been deleted in redrafting.
2-552	2	12	12	12	13	The linear trend estimate is described as "highly significant" -- we should quantify this or define it better. [Jeffrey Taylor, United States of America]	Noted. The significance can be easily asceretained from the table which is directly below this text. No changes made to the text.
2-553	2	12	16	12	22	I don't think this form of Fig.2.1 (Fig on page 128) is very informative. These remarks apply to several other figures drawn this way in chapter 2. It would be much better, and take less room, to show all time series on the same graph, e.g. in thin coloured lines and their average in thicker black. The value of a format like this is shown by Baringer et al 2010, relating to FAQ 2.1, figure 2, page 144.These published multiple time series diagrams are widely used to communicate observed climate change, and work very well as they compactly show the impact of different analyses of the same factor relative to the mean signal. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted. Following deliberations we decided to implement a simpler representation of these timeseries throughout the temperature section as straight annual mean anomaly series with no smoothing applied.
2-554	2	12	16	12	22	About Fig. 2.1: i) Concerning the initial text: "Global land surface temperature timeseries evolution estimates from 1800 to present.", at the moment of publication of AR5-WGI (in 2013) the present time will not be the final data point of this figure. So, specify the last year for which temperature anomaly data are included; ii) the temperature anomaly must be in the upper part of the figure, since it is the principal one; iii) its dimension in the vertical axis must be at least double of the present figure 2.Bottom, due to its importance; iv) in the top figure, the vertical axis needs to be indicated in a different form than in the other figure, for example: Temperature anomaly offset (°C); v) for persons that are not specialists in this field, the "Anomaly" is a difficult and incomplete expression. It must be indicated as: Global mean temperature anomaly (or similar expression); vi) it will be important to include in the text, in the figure and/or in the figure caption, the absolute value of the Earth mean temperature that it is taken as a reference, vii) the period that was considered as a reference one must be explained in the figure caption; viii) in order to see more directly the fact that the anomaly offset is in general lower than the temperature anomaly, I suggest to use the same vertical scale for both figures, ix) it must be indicated if the data are presented as an "annually" mean, as it seems to be. [Rubén D Piacentini, Argentina]	Taken into account. See response to 2-553. These revisions will account for all comments pertaining to these figures.
2-555	2	12	17	12	17	I suggest "(availability varies prior to 1880)" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure and caption have been very substantively revised in redrafting.
2-556	2	12	20	12	20	timeseries → time series [Peter Burt, UK]	Editorial
2-557	2	12	22	12	22	Again, it is unclear to me what a "like-for-like comparison" is supposed to be. I propose to phrase it as 'a more consistent [Klaas Folkert Boersma, Netherlands]	Taken into account. See response to 2-555
2-558	2	12	24	12	26	That the areas of socioeconomic development have warmed by atmospheric circulation changes has been proved by observed [Klaas Folkert Boersma, Netherlands]	Comment is incomplete and does not obviously relate in any way to the identified section. No further action taken.
2-559	2	12	25	12	25	AR1 or AR(1)? [Celeste Saulo, Argentina]	Taken into account. See response to 2-555
2-560	2	12	25	12	32	Would it be handy to have somewhere when the data was retrieved from the providers. Especially in case of datasets constantly updated. i.e. version control? [Gareth S Jones, UK]	Noted. We plan to try to provide static data and code used to create the graphics in the final version of the report.
2-561	2	12	25	12	32	(Table 2.2 caption) It is helpful for a broader readership to specify the variable considered and to explain the acronyms and abbreviations in all Figure and Table captions. In this case in line 26 "land surface temperature dataset" should be added and "OLS" should be explained. [Christian-D. Schoenwiese, Germany]	Noted. With limited real estate available it is not possible to spell out acronyms again in the captions as well as the text. No changes made.

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2-562	2	12	25	12	33	How have the 'selected periods' been selected ? why all the selected periods end in 2010 ? It would instructive to show also the trends in other periods, 1900-1950, and 1950-2010 since one contended issue is the the warming observed in the first half of te 20th century would be as large, globally, as in the second half. [Eduardo Zorita, Germany]	Accepted. We will include 1900-1950 in all applicable tables.
2-563	2	12	30	12	31	GISS has a land-only dataset, but it appears here that you have used the version which has much less extrapolation than their basic one, which uses a 1200km decay distance. It would be useful to also provide the hemipsheric and global averages for the GISS version you're using as they will not be what GISS gives out as their land-only dataset. [Philip JONES, UK]	Noted. This is the version that GISS requested we use.
2-564	2	12	32			I believe the wider community may not read the text but may read legends of figures and tables... So, I think what can be understood from figures/tables should be in the legend. As this table is the first table of that kind, I would add, in the legend, that if the numbers/figures describing the slope increase from 1850-2010 to 1979-2010, it means that the increase in temperature is accelerating... [Francois DANIS, France]	Rejected. Table and Figure captions have been discussed within the author team in redrafting to SOD. This suggestion was not enacted.
2-565	2	12	35	12	36	The text should transparently state that the "theoretical challenges" are based on the vast number of stations that do not fully meet WMO and NWS/NOAA site guidelines. The following revised sentence is suggested: "Since AR2, the finding that more than 90 percent of weather stations do not meet WMO and NWS/NOAA site guidelines (Fall, et al., 2011) has raised various challenges (Pielke et al., 2007)...." [Forrest Mims, USA]	Rejected. Pielke et al paper raises a number of issues independent of the sampling so the suggested edit makes no sense. It is also unclear why the reviewer is suddenly invoking the second assessment report here when it is not discussed in the text.
2-566	2	12	35	13	19	(and also in the previous page). I note that there is a frequent use of papers just submitted. I do not complain here the quality of papers (which in any case I do not have read yet, because they are not yet published). I also understand that the report aims to contain all the most recent findings, and I know that, perhaps, these papers will be published before the finalization of the report. However I would have preferred to base the considerations of the report mainly on already published papers. [Claudio Cassardo, Italy]	Noted. Submitted literature citation is congruent with IPCC accepted procedures.
2-567	2	12	35	14	54	I found Sections 2.2.1.1 and 2.2.1.2 rather overlapping and inconsistent in their discussion of land station siting effects. Perhaps the sections could be more clearly delineated to avoid this? [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reassignment between and restructuring within sections hopefully mitigates these concerns.
2-568	2	12	36	12	36	Move "since AR4" to the beginning of the sentence - it is a dangling modifier here. [Dian Seidel, USA]	Accepted
2-569	2	12	37	12	39	The statement "Subsequent research concludes these concerns to be largely unimportant in characterizing global-mean scale changes and serve to reinforce confidence in the reality of the reported time series behaviour" is not substantiated in the ensuing discussion, and is the kind of throwaway line that gives an impression of a head-in-the-sand attitude on the part of the IPCC. [Ross McKitrick, Canada]	Noted. This sentence has been modified to be more balanced.
2-570	2	12	38	12	38	This claim is unfair and doesn't do justice to the ongoing debate in the field. Many of the unresolved issues in Pielke 2007 are still unresolved. See the comments on this paper by Peterson et al and the reply by Pielke et al, Parker, D. E., P. Jones, T. C. Peterson, and J. Kennedy, 2009: Comment on Unresolved issues with the assessment of multidecadal global land surface temperature trends. by Roger A. Pielke Sr. et al.,J. Geophys. Res., 114, D05104, doi:10.1029/2008JD010450. Pielke Sr., R.A., C. Davey, D. Niyogi, S. Fall, J. Steinweg-Woods, K. Hubbard, X. Lin, M. Cai, Y.-K. Lim, H. Li, J. Nielsen-Gammon, K. Gallo, R. Hale, R. Mahmood, S. Foster, R.T. McNider, and P. Blanken, 2009: Reply to comment by David E. Parker, Phil Jones, Thomas C. Peterson, and John Kennedy on “Unresolved issues with the assessment of multi-decadal global land surface temperature trends. J. Geophys. Res., 114, D05105, doi:10.1029/2008JD010938. Pielke et al conclude: "However, the analyses performed by Parker et al. [2009] do little to improve confidence in the global surface temperature record. In particular, we reaffirm the statement of Pielke et al. [2007a] that nearby changes in LULC may be influencing the temperature trends observed at surface climate observing stations. We further continue to emphasize the lack of data independence in the global surface temperature analyses (including that of Parker et al. [2009]). We do agree with Parker et al. [2009] that data sparseness makes temperature trend estimates less robust over many parts of the globe, and join their call for improved data collection, metadata, and data rescue." [Marcel Crok, The Netherlands]	Taken into account. Combined with 2-569. Much of the comment focusses microscopically on a single exchange but the paragraph is about the literature as a whole, not one single exchange in one journal.
2-571	2	12	38	12	39	"Subsequent research concludes these concerns to be largely unimportant in characterizing global-mean	Taken into account. Combined with 2-569.

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						scale changes and serves to reinforce confidence in the reality of the reported time series behaviour" This MUST be referenced - or else omitted. [Philip Lloyd, South Africa]	
2-572	2	12	40	12	40	The results of Fall et al should be given. So far this is the only attempt to document all the stations in a large area with photographs and site information. Very few of the sitings were good and still USHCN is regarded as maybe the 'best' network in the world. The main conlusion of Fall et al is: "Temperature trend estimates vary according to site classification, with poor siting leading to an overestimate of minimum temperature trends and an underestimate of maximum temperature trends, resulting in particular in a substantial difference in estimates of the diurnal temperature range trends." [Marcel Crok, The Netherlands]	Rejected. Fall et al. results are already cited appropriately and in a depth proportionate to the discussion of other results. As the text rightly states for USHCN the siting quality is highly correlated with instrument type and the conclusions from fall et al are consistent with well understood instrument type biases as noted in e.g. Williams et al., 2012.
2-573	2	12	40			The Web URL for this unprecedented citizen science effort is http://www.surfacestations.org. This URL should be provided so readers can judge for themselves any significance it might have. [Forrest Mims, USA]	Rejected. Guidance is not to include URLs.
2-574	2	12	48	12	50	The sentence "Benchmarking … error structure' was unintelligble for me. I strongly suggest that this be rephrased in more clear [Klaas Folkert Boersma, Netherlands]	Comment is mis-placed. Perhaps relates to p.13 instead in which case it is covered by responses there.
2-575	2	12		12		Table 2.1: Paragraph 9 of Hansen et al., 2010 implies that the GISS analysis averages the surface temperature bins within latitude zones (which their paragraph 130 suggests are 90°S–25°S, 25°S–0°S, 0°N–25°N, and 25°N–90°N), then the global mean is computed as the average of these four zones with each zone weighted by its area. This differs slightly from a simple “Average of the bins”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Added text 'with areal weighting' to account for this. Table now resides in the appendix.
2-576	2	12				comparison.' [Klaas Folkert Boersma, Netherlands]	Comment is unintelligible fragment. No action taken
2-577	2	12				increases in air temperature well above the ground? I think some more substance to the statement would be useful here. [Klaas Folkert Boersma, Netherlands]	Comment is unintelligible fragment. No action taken
2-578	2	12				terms. [Klaas Folkert Boersma, Netherlands]	Comment is unintelligible fragment. No action taken
2-579	2	12				Table 2.1, column 4, row 4: The word "independently" should be "separately". The analysis does not assume spatial independence. [Peter Guttorp, USA]	Accepted. This table now resides within the appendix.
2-580	2	12				Table 2.2 GHCNv3.0.0 contained coding errors (GHCN Technical report GHCNM-12-01, Nov 2011). It was replaced by v3.1.0, which also appears to contain errors, and again in Feb 2012 by v3.1.1. Also, this table again uses the misleading trick of comparing long and short trends. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Noted. We use the latest version of all datasets. The use of differing trend lengths is to be commensurate with and comparable to AR4 and is discussed at length in Box 2.2.
2-581	2	12				Figure 2.1: the caption states that the lower panel "shows the mean" while it clearly shows anomalies. Please describe how these anomalies are calculated. [Jeffrey Taylor, United States of America]	Taken into account. These figures have been substantively revised and this comment no longer pertains..
2-582	2	12				Table 2.2: In the title block of the table, it is stated that "two SE range" is used -- please clarify this by avoiding the abbreviation. [Jeffrey Taylor, United States of America]	Noted. The nomenclature has been changed in response to author team discussions so this is now no longer relevant.
2-583	2	13	1	13	1	change "optimal modern siting" to "optimal siting". [Karen Rosenlof, United States of America]	Rejected. Siting quality is not a static property of the site. We have clarified what we mean by modern siting explicitly in response to this.
2-584	2	13	1	13	1	Consider changing "far from optimal" to "poor". Clarify "may be expected" or re-write this. Do you mean expected in the future, or probably occurred in the past? [Dian Seidel, USA]	Accepted
2-585	2	13	1	13	2	The reader will be much better informed by the numerical explanation of "many." Fall, et al. clearly describe empirical findings from visual inspections and photographs that only 1.2% of 1,007 US weather stations fully meet NWS site guidelines. This or related information certainly deserve mention in a section devoted to highly publicized questions and answers related to the instrumental temperature record. [Forrest Mims, USA]	Noted. Changed many to most but addition of numbers would be too much detail compared to the treatment of remaining papers and results. Also,, the siting criteria are a continuum and more than the

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							reviewer stated proportion are in the combined 'satisfactory siting' or above categories.
2-586	2	13	1	13	2	It states that many sites may be expected to suffer large biases. Make it clear whether these are large biases in absolute temperature or in the temperature change over time, and note in the text that the former have no influence on most of the data products. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. We have added explicit caveat about the large biases being in the absolute temperature in an attempt to clarify this point.
2-587	2	13	2	13	2	same comment as above…unless you're going to explain what is different between "modern" siting and old fashioned siting, I suggest leaving the qualifier out. [Karen Rosenlof, United States of America]	Taken into account. Combined with 2-583
2-588	2	13	2	13	2	What is "modern siting"? [Karen Rosenlof, United States of America]	Taken into account. Combined with 2-583
2-589	2	13	2	13	3	This poor siting is likely in the Berkeley data as they have so many short series. You point out later that the Fall et al (2011) is just for the USA and is also negligible when it comes to mean temperatures. Fall et al (2011) is mainly concerned with problems with Tx and Tn and the fact that these are generally opposite in sign in the magnitudes of the problems. [Philip JONES, UK]	Noted. But here we are not discussing explicitly the Berkeley dataset method so no changes made to this text. The reviewer's remaining concerns are articulated in the discussions of the US record elsewhere.
2-590	2	13	2	13	4	"Within the USA modern siting quality is very highly correlated with instrument type and the biases for the network as a whole have been documented to be largely dominated by instrument type, rather than siting, biases" This is NOT substantiated by Fall et al 2011 "This initial study examines temperature differences among different levels of siting quality without controlling for other factors such as instrument type. Temperature trend estimates vary according to site classification, with poor siting leading to an overestimate of minimum temperature trends and an underestimate of maximum temperature trends, resulting in particular in a substantial difference in estimates of the diurnal temperature range trends." [emphasis added) There is therefore a discrepancy between Fall et al 2011 and Menne et al 2011 and the quoted statement from the FOD does not reflect this discrepancy - instead, it in effect discards Fall et al. even though Fall et al postdates Menne. The quote from the FOD is not balanced. [Philip Lloyd, South Africa]	Rejected. Careful reading of Fall et al. results section substantiates the correlation between siting quality and instrumentation as does the metadata of instrument type by station class readily calculable from their metadata and associated fields publicly available from NCDC.
2-591	2	13	4	13	6	"Regardless, homogenization procedures (Menne and Williams, 2009; Rhode et al., submitted) remove most, if not all, of the impacts (Fall et al., 2011; Menne et al., 2010; Muller et al., submitted; Williams et al., Submitted)." To the contrary, Fall et al document the impact of poor siting particularly on the trends in the homogenized data for maximum and minimum temperatures. The FOD statement is wrong. [Philip Lloyd, South Africa]	Rejected. The FOD text is explicitly discussing mean temperatures here. The discussion of max and min is returned to later on when the implications of the Fall et al. analysis for DTR trends is explicitly discussed.
2-592	2	13	4			Regardless, homogenization procedures (Menne and Williams, 2009; Rhode et al., submitted) remove most, if not all, of the impacts (Fall et al., 2011; Menne et al., 2010; Muller et al., submitted; Williams et al., Submitted). [Philip Lloyd, South Africa]	Comment makes no sense as it is a cut and paste of the submitted draft text. No action requested or taken.
2-593	2	13	6	13	6	"of biases" might be added to the end of the sentence to clarify what is causing the impacts. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-594	2	13	6	13	7	A paragraph break here might be useful to emphasise the shift from the discussion of USHCN to the globe. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. This whole section has been reordered to aid reader comprehension.
2-595	2	13	7	13	11	"Sampling and methodological independence has been assessed through sub-sampling, which shows very little sensitivity to use of entirely independent samples (Jones et al., Submitted; Parker et al., 2009), and creation of an entirely new and structurally distinct product (Rhode et al., submitted) and a complete reprocessing of the GHCN product (Lawrimore et al., 2011). None of these yielded more than minor perturbations to the records." This may be true, but it is irrelevant to the question of whether siting is appropriate - yet is given in apparent substantiation of the previous statement which, as I have noted, is incorrect. [Philip Lloyd, South Africa]	Rejected. Such studies are directly relevant to siting. The context of the paragraph is also broader than just the effects of siting. See response to 2-594. Reordering of the text has hopefully acted to increase comprehensibility of the discussions here.
2-596	2	13	9	13	11	The urban heat island contribution to regional warming is much greater than suggested here. (1) Consider this finding from a 2011 paper: "On average, [from 1954 to 2008] the total temperature increase over South Korea was about 1.37 °C; the amount of increase caused by the greenhouse effect is approximately 0.60 °C, and the amount caused by urban warming is approximately 0.77 °C." (Kim Maeng-Ki And Kim Seonae (2011). Quantitative estimates of warming by urbanization in South Korea over the past 55 years (1954—2008).	Misplaced comment. We are not discussing the urban heat island in the referred to section. Assuming that this instead refers to the UHI / LULC section this has been substantively redrafted in the latest draft.

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						Atmospheric Environment 45. 5778-5783.) (2) Also consider this 2011 paper: Chow, Winston T. L., Bohumil M. Svoma, 2011: Analyses of Nocturnal Temperature Cooling-Rate Response to Historical Local-Scale Urban Land-Use/Land Cover Change. J. Appl. Meteor. Climatol., 50, 1872–1883. (3) Consider this very well known study of Phoenix, Arizona (USA): Baker, L.A., A.J. Brazel, N. Selover, C. Martin, N. McIntyre, F.R. Steiner, A. Nelson, and L. Musacchio. 2002. Urbanization and warming of Phoenix (Arizona, USA): Impacts, feedbacks and mitigation. Urban Ecosystems, 6: 183–203. (4) Consider many urban heat island studies of US cities, including Atlanta, Houston, Phoenix, etc., including this: C.P. Lo and Dale A. Quattrochi (2003). Land-Use and Land-Cover Change, Urban Heat Island Phenomenon, and Health Implications: A Remote Sensing Approach. Photogrammetric Engineering & Remote Sensing 69, No. 9, pp. 1053–1063. (5) Many more studies (including even high school science fair projects) could be cited, but my time is gone. [Forrest Mims, USA]	
2-597	2	13	10	13	11	More than minor perturbations" What exactly is being referred to here ? The regional mean or ? Presumably it does make noticeable differences at some (small) spatial scale ? paragraph starts talking about global mean changes and then talks about US data only which is where confusion arises as to what is being discussed here. Would be helpful to clarify what is meant by minor perturbatoins. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. Added text 'global LSAT' and the splitting off into a new paragraph (2-594) and reordering of the section also helps to address this concern.
2-598	2	13	11	13	11	After "perturbations to the records" add "at global or hemispheric scales", since the effects at local scales can be much larger. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-597
2-599	2	13	11	13	11	Consider starting a new paragraph at "Willett…" [Dian Seidel, USA]	Noted. We prefer to start the new paragraph a little earlier with the change in spatial domain as suggested in 2-594.
2-600	2	13	11	13	12	"changes in specific humidity" [George Kiladis, USA]	Noted. It is actually across the papers both specific and relative humidity. To avoid confusion we have modified the sentence to make this point explicitly.
2-601	2	13	13	13	15	Why is it interesting that re-analyses are in agreement with actual observations? Would hope this is the case in areas where there are observations... but wouldn't know if there was an agreement in areas with no observations. [Gareth S Jones, UK]	Noted. We will make clearer that these reanalyses do not ingest the surface temperature observations so they are an independent verification through addition of discussion. This discussion resides in the UHI section where it is more appropriate.
2-602	2	13	13	13	15	"Various investigators (Parker, 2011; Simmons et al., 2010; Vose et al., Submitted-a) showed that temperature trends and time series from modern reanalyses were in very good agreement with observed products." As the reanalyses included sea surface temperatures and as the area of the sea is ~70% of the globe, it would be most surprising if the reanalysis did not reproduce the observed temperature trends. Simmons et al 2010 noted ~80% agreement. Other reanlayses have multiple temperature inputs - CFSR for instance has 16 different temperature data inputs, primarily atmospheric, but including soil and sea temperatures. Systems that include temperature data as inputs cannot be used to prove that temperature measurements are correct! [Philip Lloyd, South Africa]	Taken into account. Combined with 2-601. The reviewer seems to believe that if the reanalysis assimilates any temperature data from any source it can't then be compared to temperature data from an independent source. This is incorrect. The non-observed field will be a simulation of the measurand independent of the measurements and hence act as a reasonable and physically consistent check.
2-603	2	13	14			Simmons et al show convincingly in fact that the HADCRU record underestimates trends owing to missing data in the Arctic. It is only good agreement where data exist and the missing data is a real issue. This is a case where reanalyses do better. This section does not do a good enough job on changes in sampling over time. This is touched on on p 20 l 41. [Kevin Trenberth, USA]	Noted. No changes made here but a section has been added to the appendix on sampling over time and it is also more intuitively called out in the new global trends map.
2-604	2	13	15	13	15	"Jones et al. (Submitted)" - there is no citation in the references that seems to match this. [Gareth S Jones, UK]	Noted. This is the CRUTEM4 paper which was in the references.
2-605	2	13	17	13	17	summertime → summer time [Peter Burt, UK]	Editorial
2-606	2	13	17	13	19	When talking about the bias adjustments for very early records, it would be more informative to be explicit about the time period and to give example values to indicate the magnitude of the adjustments and to give an indication of the effect the adjustments could have on hemispheric/global scale records. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. We have made clear the timeframe but as there is no single number or good estimate of an areal average magnitude in the literature we do not quantify explicitly as suggested.

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2-607	2	13	18	13	18	insert comma after 'approaches' [Peter Burt, UK]	Editorial
2-608	2	13	18	13	18	and elsewhere. Gross or grossly is used frequently in the chapter - I would prefer them to be replace with words such as "broad(ly)", "general" or "largely" as appropriate [Elizabeth Kent, England]	Editorial
2-609	2	13	18	13	18	What is meant by "grossly confirmed"? [Dian Seidel, USA]	Noted. Text has been redrafted substantively in response to reviewers.
2-610	2	13	21	13	22	Change "trend fields" to "trends". [Dian Seidel, USA]	Editorial
2-611	2	13	21	13	36	The notion that AR4 did not provide explicit evidence of their criticism of MM2004 and DLM2006 is correct. Furthermore, counter to the claims here it has been shown in subsequent studies that indicators of circulation changes do not provide any explanation for the found relation between temperatures and socio-economic parameters. In addition, the results of DLM2006 are still misrepresented in Schmidt [2009]. First of all, Schmidt [2009] primarily focusses on MM2004, not DLM2006. Schmidt [2009] claims that DLM2006 did not consider processes like tropospheric ozone or aerosols, when in fact these were mentioned as possible explanations for the temperature trend – socio-economic factors correlation. Furthermore, Schmidt [2009] shows that for period roughly between 1940 and 1970 there is a similar but negative correlation (larger cooling for larger economic activity), and therefor interprets these results as spurious. However, there exists another possible but amazingly completely overlooked explanation. The positive correlation may reflect actual physical changes in the climate system, as regions of high economic activity are also regions of high emissions, in particular sulfur. Changes in these emissions have been attributed to changes in radiation, the so called global brightening or dimming, but amazingly never been linked to temperature trends as far as I am aware of. Yet given the positive correlation between temperature trends and socio-economic factors after 1980 and according to Schmidt [2009] a negative correlation roughly between 1940 and 1970 it appears possible that these relations reflect changes in aerosols (increase in aerosols between 1940 and 1970, decreases in aerosols after 1980 due to worries about acid rain. Such changes have been documented and are actually used to reproduce 20th century temperature changes in climate model simulations. It is thus conceivable that these patterns actually can be interpreted as fingerprints of global warming and thus in agreement with, rather than being in conflict with or contradicting, ‘global warming’. [Marcel Crok, The Netherlands]	Noted. The effect of dimming / brightening on temperatures is discussed elsewhere in the chapter. The causal aspects would need to be discussed in modelling chapters 9 and 10 and are out of scope for this chapter. This paragraph has been moved to a more appropriate location and redrafted in entirety.
2-612	2	13	21	13	36	In general, as AR4 did such a bad job on this topic it is very important that AR5 does a much better job. The discussion didn't end with Schmidt (2009). McKitrick and Nierenberg showed that the claim of Schmidt (2009) was incorrect. The story behind this paper is not a pretty one. The 'team' did everything they could to block the paper in IJOC, which they succeeded in. For the story see http://climateaudit.org/2010/12/15/mckitrick-and-nierenberg-2010-rebuts-another-team-article/ IPCC reports are supposed to be objective and comprehensive. Now this is the moment to show that IPCC can be objective and comprehensive. The paper eventually was published in an economics journal: McKitrick, R. and N. Nierenberg, 2011. Socioeconomic Patterns in Climate Data, Journal of Economic and Social Measurement 35(3-4): 149-175. Parts of their conclusion say: "We have examined the question of whether spatial trend patterns in surface temperature data can be explained in part by non-climatic, socioeconomic processes of the kind that are supposed to have been filtered out of the gridded data products. We have shown that a coefficient pattern connecting temperature trends to indicators of industrialization is robust across a wide range of data configurations in the surface and lower troposphere, but is absent in climate model-generated data. The failure to reproduce this pattern in models indicates that it is not a natural feature of the climate system nor a response to greenhouse gas-induced forcing." (...) Therefore, our overall finding is that the strong explanatory influence of socioeconomic effects on the pattern of climatic trends over land cannot be explained away as spurious effects due to spatial autocorrelation, data selection or fluke correspondence with known atmospheric circulation patterns. In the absence of any alternative explanation we conclude with some confidence that the temperature data being used for most modern analysis of climate change is inadequately filtered to remove known contamination patterns related to urbanization and other socioeconomic influences. The counterfactual experiments in Table 6 indicate that the contamination yields an overall warm bias over land. [Marcel Crok, The Netherlands]	Noted. We requested explicit guidance from TSU over what constituted peer-reviewed literature as these are not searchable on ISI WoK and therefore we believed they did not constitute the official guidance for peer reviewed literature which it was our explicit charge to assess. Based upon their guidance these newer papers are now considered in the SOD.
2-613	2	13	21	13	36	Another paper that is highly relevant in this context is Klotzbach (2009): Klotzbach, P.J., R.A. Pielke Sr., R.A. Pielke Jr., J.R. Christy, and R.T. McNider, 2009: An alternative explanation for differential temperature trends at the surface and in the lower troposphere. J. Geophys. Res., 114, D21102, doi:10.1029/2009JD011841	Noted. This paper is not relevant in the context of the specific discussion being critiqued as it makes no attempt to differentiate trends beyond land / ocean

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						in which they conclude: ”This paper investigates surface and satellite temperature trends over the period from 1979 to 2008. Surface temperature data sets from the National Climate Data Center and the Hadley Center show larger trends over the 30-year period than the lower-tropospheric data from the University of Alabama in Huntsville and Remote Sensing Systems data sets. The differences between trends observed in the surface and lower-tropospheric satellite data sets are statistically significant in most comparisons, with much greater differences over land areas than over ocean areas. These findings strongly suggest that there remain important inconsistencies between surface and satellite records.” In summary, the papers of Michaels/McKitrick/Nierenberg, De Laat/Maurellis and Klotzbach et al provide strong evidence that there still is a warm bias in the surface temperature record. The central claim that UHI has contributed for only 10% to the long term trend is not warranted. [Marcel Crok, The Netherlands]	whereas this discussion is about apprent gradients in LSAT.
2-614	2	13	21	13	36	This sentence is unclear to me: “According to the AR4, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant if one takes into account the fact that the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes.” I did not understand it, because the contrary seems to be true. As a matter of fact the whole intention of this paragraph is unclear. Those biases mentioned in lines 22 and 32 should be better explained, and it should also become clearer what the strongest warming in the reanalysis proves and why significantly less warming in the reanalyses would support the claims of the studies referred to in the beginning of the paragraph. Is the intention of this paragraph to say that global warming is not due to urbanization and related land surface changes but to increasing GHG? Then it should be said in a clearer way. [Alice Grimm, Brazil]	Noted. The entire section has been redarfted and moved to a more appropriate section and these concerns and those raised by others (which are often contradictory) have been taken into account to the extent possible in so doing.
2-615	2	13	21	13	36	This is a crucial paragraph. If possible, the flaws in the McKitrick & Michaels study should be worked out more clearly, of course in a balanced and objective way. Currently, their findings are only indirectly rebutted. One should avoid providing quotes for climate sceptics („both studies concluded that...“). [Douglas Maraun, Germany]	Taken into account. See response to 2-614
2-616	2	13	21	13	36	In order to properly survey this topic you need to add 3 references: McKitrick, Ross R. and Nicolas Nierenberg (2010) Socioeconomic Patterns in Climate Data. Journal of Economic and Social Measurement, 35(3,4) pp. 149-175. DOI 10.3233/JEM-2010-0336.; McKitrick, Ross R. (2010) Atmospheric Oscillations do not Explain the Temperature-Industrialization Correlation. Statistics, Politics and Policy, Vol 1 No. 1, July 2010.; McKitrick, Ross R. (2012) Encompassing Tests of Socioeconomic Signals in Surface Climate Data. submitted (http://www.uoguelph.ca/economics/sites/uoguelph.ca.economics/files/2012-02.pdf). [Ross McKitrick, Canada]	Taken into account. Combined with 2-612
2-617	2	13	21	13	36	Don't need to spend as much space as this on this topic, could easily cut by 30%. Also it would be better moved to 2.2.1.2, which could be expanded in scope to cover "Land cover and urban heat island effects". [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. The section has been moved as requested. See also response to 2-614
2-618	2	13	21	13	36	This is a key result since it addresses climate warming claim objections. Well done. [Larry Thomason, United States of America]	Noted
2-619	2	13	21	21	36	I can't tell what the actual point being made in this paragraph is. [Karen Rosenlof, United States of America]	Taken into account. See response to 2-614
2-620	2	13	24	13	27	"According to the AR4, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant if one takes into account the fact that the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes. AR4 provided no explicit evidence for this overall assessment result." If AR4 indeed gave no explicit evidence for its claim, then it is entirely inappropriate to cite it in evidence against two refereed publications (McKitrick and Michaels (2004) and de Laat and Maurellis (2006)). [Philip Lloyd, South Africa]	Rejected. We need to provide the context. We also are not using it as evidence to refute their claims, but rather to provide a context in which to place subsequent developments appropriately.
2-621	2	13	24	13	27	These sentences make a start at acknowledging the fabrication of evidence that took place in the AR4. I commend you for bringing the issue forward. However, you use a passive voice, insert the qualifier "explicit" as if to suggest there was "implicit" evidence, refer to "the fact that" warming locations and the effects of circulation changes overlap even though that was equally an unsupported statement, call the claim an "overall assessment" as if to suggest there was some evidential basis for it (and ignoring the fact that the text in question was not inserted until after the close of peer review, making it impossible for it to have been "assessed"), and you do not cite the one paper on the subject that specifically tested and refuted the IPCC	Rejected. The suggested replacement text is perogative in nature which is entirely inappropriate in the context of scene setting. We are not trying to critique what was done in AR4 here, but rather provide a context in which the reader can then appropriately digest the more recent developments and evaluate our new assessment.

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						claim. A more accurate summary would be: "In the AR4, Trenberth et al. (2007) dismissed evidence of socioeconomic contamination of the surface temperature record by claiming that the correlation of warming with industrial and socioeconomic development would cease to be statistically significant if one took into account a conjectured overlap between the locations of greatest socioeconomic development and those associated with atmospheric circulation changes. The claim was made with no supporting evidence and was shown in McKitrick (2010) to be untrue." [Ross McKitrick, Canada]	
2-622	2	13	26	13	28	The text in this paragraph needs much work! This is giving weight to some poor papers. MM2007 claim to take circulation into account, but they are using annual average data. In most places around the world circulation influences from wind direction are generally opposite in winter compared to summer. Therefore, attempting to extract circulation influences using annual averages is a flawed procedure. As the summer circulation influence is generally weaker,it might be possible to use winter half year temperatures, but this has not been tried in the context here. [Philip JONES, UK]	Taken into account. See response to 2-614
2-623	2	13	27	13	27	I really applaud this admission by the authors. This was one of the worst examples of bias from the authors in AR4. [Marcel Crok, The Netherlands]	Noted
2-624	2	13	28	13	28	"McKitrick and Michaels (2007) corroborated their earlier findings" there is no need to add "claimed to". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-625	2	13	28	13	30	Biased wording - “McKitrick claimed”, Schmidt “showed”. Use neutral wording to describe results of papers. In fact McKitrick's work is based on observed data while Schmidt uses model simulations, so the former is likely to be more reliable. McKitrick has two 2010 papers that are relevant. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-624
2-626	2	13	28	13	32	The language here is indicative of bias, perhaps unintentional, when you say that McKitrick and Michaels "claimed to" corroborate their earlier findings, whereas Schmidt "showed" our work was not robust. The use of slanted verbs would be improper in any case, but in this case is especially unjustified since Schmidt didn't "show" anything, he only "claimed" things, on which see the next cell. [Ross McKitrick, Canada]	Taken into account. Combined with 2-624
2-627	2	13	28	13	32	You have not mentioned McKitrick and Nierenberg (2010) (See ref. in cell 23) which was a full response to Schmidt's paper. Schmidt made 3 claims about McKitrick and Michaels (2007): That the results could not be replicated on other data sets (in particular using the RSS series rather than UAH); that spatial autocorrelation (SAC) rendered the findings insgnificant; and that similar correlations could be found in GCM output where no surface contamination occurs, making the apparently observed effects spurious. But he only claimed these things, he did not show them, paceyour statement. MN2010 replicated the McKitrick and Michaels results on 8 different surface-troposphere data configurations, including RSS (see their Table 2). Schmidt showed some variograms of the dependent variable, but that is not an SAC test. MN2010 presented robust LM tests for SAC in the dependent variables and -- crucially -- the residuals, and showed that in most cases the McKitrick and Michaels model did not yield spatially autocorrelated residuals, so the SAC correction was not needed (see their Table 3). However, even adding an SAC treatment did not undermine any of the original conclusions (see their Table 4). They also showed that SAC is present in the residuals of Schmidt's own regression using GCM data but since he didn't test for it he didn't notice it. Correcting it eliminated all the apparent correlations between socioeconomic variables and regional trends in GCM data not only in Schmidt's GISS analysis but also in a similar analysis on the average across all 23 GCMs used in the AR4 (see MN2010 Table 4). Consequently Schmidt's basis for claiming that the McKitrick and Michaels results were spurious was refuted. M&N2010 showed that there are significant correlations between the spatial pattern of warming and the spatial pattern of socioeconomic development, they are robust to many specification tests and robustness corrections including SAC, and they are not predicted by climate models as a feature of the spatial pattern of GHG-induced warming, indeed they tend to run opposite to the GCM predictions. [Ross McKitrick, Canada]	Taken into account. Combined with 2-612
2-628	2	13	28	13	32	Hence it would be more accurate to summarize this dispute by saying: "In subsequent analysis McKitrick and Michaels (2007) corroborated their earlier findings and estimated that between one-third and one-half of the 1980-2002 trend over land could be due to local land-surface changes and faults in the observations. Schmidt (2009) suggested that the spatial response to GHG forcing might yield a spurious match between the spatial pattern of greenhouse warming and socioeconomic development, and that the significance of the correlations	Taken into account. See response to 2-614

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						might have been overstated due to spatial dependence. But McKitrick and Nierenberg (2010) showed that correcting for spatial autocorrelation did not change the earlier conclusions, and that GCM simulations of greenhouse warming do not predict a spatial pattern in which regional warming correlates to the spatial distribution of socioeconomic development." [Ross McKitrick, Canada]	
2-629	2	13	30	13	30	The warming trend was particularly enhanced, in the boreal cold season (Nov. to Mar.) over semi-arid regions, showing a temperature increase of 1.53°C as compared to the global annual mean temperature increase of 1.13°C over land. In mid-latitude semi-arid areas of Europe, Asia, and North America, temperatures in the cold season increased by 1.41, 2.42, and1.5°C, respectively. The semi-arid regions contribute 44.46% to global annual-mean land-surface temperature trend (Huang et al, ACPD, 2012). Reference: Huang, J., Guan, X., and Ji, F. 2012: Enhanced cold-season warming in semi-arid regions, Atmos. Chem. Phys. Discuss., 12, 4627-4653, doi:10.5194/acpd-12-4627-2012. [Jianping Huang, China]	This comment appears to be misplaced. It has no obvious relationship to the text identified. No changes made.
2-630	2	13	30	13	32	“In contrast, Schmidt (2009), showed that much of the reported correlation between warming and socio-economic indicators likely arose due to naturally occurring climate variability and model over-fitting and was not robust”. This appears to be a misreading of Schmidt, who concludes op cit "Thus, though this study cannot prove that the global temperature record is unbiased, there is no compelling evidence from these correlations of any large-scale contamination" If the author concedes that his study does cannot disprove McKitrick and Michaels claim of bias, who would disagree with him? There must be a suspicion that there remains some bias in the temperature signal, and this needs to be reflected in this document - at present, it is not. [Philip Lloyd, South Africa]	Taken into account. See response to 2-614
2-631	2	13	32	13	32	for clarity it might help to say "Further, it is impossible to reconcile such gross biases as those claimed by McKitrick and Michaels with the very good…" [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-614
2-632	2	13	32	13	35	"Further, it is impossible to reconcile such gross biases with the very good agreement seen between the methodologically diverse set of modern reanalysis products, none of which directly assimilate land-surface air temperatures, and the land-surface air temperature records at global and regional levels" As noted previously, because the reanalyses have temperatures as their inputs, they cannot be used to detect bias in the records. The drafter seems really confused on the meaning of "assimilate" in this context. My understanding of the word is that observations are combined with subsystem models within the overall system and the subsystems run until such time as they reproduce the observations with a reasonable degree of faithfulness, whereafter the other outputs of the subsystems are employed in other parts of the system. Thus the fact that the system 'assimilates' temperature data means that it will, if required, generate the observations reasonably well. This certainly is the sense in which it is used in, for instance, Rogers, E. et al Changes to the NCEP Meso Eta Analysis and Forecast System: Increase in resolution, new cloud microphysics, modified precipitation assimilation, modified 3DVAR analysis http://www.emc.ncep.noaa.gov/mmb/mmbpll/eta12tpb/ which has one of the best descriptions of the process I know of. [Philip Lloyd, South Africa]	Rejected. None of the reanalyses directly assimilate the surface temperatures so the current text is correct. The implication of the reanalyses is that the adjusted data are consistent with the sum totality of the rest of the observing system and therefore the warming is real. This is an argument made either explicitly or implictly in the papers that are cited here.
2-633	2	13	32	13	36	This part of the paragraph doesn't constitute an argument. You seem to be suggesting that if results from reanalysis products don't agree with the analyses of the surface observational data in the McKitrick and Michaels work, the reanalysis data must be correct. In that case, why do you use, much less rely on, the surface observational data in the rest of the chapter, and why present detection and attribution results in other chapters that use the land surface record? You can't have it both ways. The IPCC uses the surface data to measure warming and to detect the effects of greenhouse gases. The literature you summarize has presented evidence of significant contamination of the surface record from socioeconomic development. All the counterarguments you have cited, including the fabrications in the AR4 and the conjectures in Schmidt (2009), were examined and refuted. None of the teams using reanalysis data have looked at whether it yields spatial correlations with socioeconomic development indicators, so those data products cannot be brought in at this stage of the argument to provide yet another unsupported basis for dismissing the problem. [Ross McKitrick, Canada]	Taken into account. See response to 2-632
2-634	2	13	32	13	36	In addition, you cite a "very good agreement" between reanalysis trends and surface data. But you do not explain anything about that agreement. It sounds as if you are referring to agreement at the level of the univariate trend in the global average; but on page 10 lines 20-21 you disparage the reliability of long term	Taken into account. See response to 2-632. The reviewer also mis-understands the text on page 9 which does not claim that reanalyses products are

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						global trends in reanalysis data sets. Equally at issue is the gridded pattern of warming and cooling, which is essential for evaluating whether surface data sets are suitable for detection and attribution work, ie fingerprinting, but on page 9 line 49 you say that reanalysis products do not provide gridded outputs. If the agreement you refer to is in the global average surface trend, then explain why the agreement matters in light of the serious quality problems mentioned on age 10, and quote some test statistics on the strength of the agreement. If the studies you cite have looked at the spatial pattern of trends at the surface then explain how well those match; otherwise they're not on point. You also say they do not "directly" assimilate surface observations. You need to explain the term more precisely, since if they "indirectly" assimilate surface observations then any resulting match doesn't mean much. [Ross McKitrick, Canada]	anything other than available as gridded products.
2-635	2	13	32	13	36	I suggest the paragraph would be more accurate if the sentences beginning at line 32 with the word "Further..." were deleted and the following added instead. "At this point the disputes about whether global surface climate data products are affected by large-scale non-climatic biases arising from socioeconomic development and measurement problems are unresolved. There do appear to be significant correlations between the recent spatial pattern of warming trends over land and the spatial patterns of socioeconomic development, and this pattern is not predicted by climate models as a response either to natural variations or greenhouse gases. The conjectured grounds for setting this concern aside in previous Assessment Reports have subsequently been invalidated. As a result, global-scale contamination of land surface climate data must be considered a possibility. Any such contamination has no effect over the oceans, however, where observed trends are one-half to one-third the size of those in the land data sets (see Table 2.4). The bias implied by McKitrick and Michaels (2007) is about 0.1 degrees C per decade over land, or about one-third of the post-1979 trend over land. This explains only about 0.03 degrees C per decade globally, or one-fifth of the post-1979 global trend reported in Box 2.2, Table 1. Of potentially more significance, however, is that detection and attribution studies rely on the assumption that all non-climatic effects in the land surface data are removed during the homogenization process, and our confidence on this point is diminished in comparison to previous assessments." [Ross McKitrick, Canada]	Rejected. Suggested replacement text is not a balanced assessment of the literature as a whole, which is what is being strived for, and over-reaches chapter remit.
2-636	2	13	35	13	36	The final sentence here is a crucial one. It would be useful comparing reanalyses (say ERA-INTERIM) with land-based temperatures from say CRUTEM4 or NCDC. This would show that urbanization influences are negligible and as stated the Reanalyses warm slightly more. [Philip JONES, UK]	Taken into account. See response to 2-632
2-637	2	13	36	13	35	Probably helpful to explicitly state referring to global mean warming here, [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Rejected. These studies are actually considering the full spatio-temporal field.
2-638	2	13	36			need to say why the trend is more and that it is better because it properly depicts the Arctic changes [Kevin Trenberth, USA]	Noted. As this is about LSAT and the arctic is largely ocean no changes have been made specifically here.
2-639	2	13	38	13	38	"LSAT" is not defined here or elsewhere [George Kiladis, USA]	Noted. LSAT now defined on first usage and used more broadly throughout the section.
2-640	2	13	38	13	39	Sentence starting "The US national …" doesn't really make sense, and probably doesn't say much either [Elizabeth Kent, England]	Accepted. Sentence deleted.
2-641	2	13	38	13	54	There is an overemphasis in this paragraph on the USA. This region is only 2% of the Earth's land surface. [Philip JONES, UK]	Rejected. We are charged with assessing the literature and far more has been done and said about the US data than the RoW. Changes in our understanding here are also more substantial than apparent from the literature for elsewhere. We cannot do anything other than reflect the balance of the literature. Comments have suggested a number of leads for the following pargraph which is now more substantial and as a result we have somewhat better balance.
2-642	2	13	38	14	4	The regional observations of section 2.2.1.1 have a strong local bias towards the US. There are only a coule of lines about Europe and nothing at all about the rest of the world, although I can not image that temperature is only measured in the US and Europe. [Uwe Stoeber, Germany]	Taken into account. Combined with 2-641.

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2-643	2	13	42	13	54	I found this discussion hard to follow. [Dian Seidel, USA]	Noted. Without specific suggestions it is hard to make exact changes although changes suggested elsewhere may help.
2-644	2	13	44	13	46	"Fall et al. (2010) found that the North American Regional Reanalysis generated overall surface air temperature trends for 1979–2003 similar to the USHCN" In essence, it had to, because it used historical temperature data in the system. "Following the 25-yr period of 1979–2003, the NARR is being continued in near–real time as the R-CDAS. As specified in more detail below, this is done with a maximum effort to minimize changes in R-CDAS compared to the retrospective NARR system." Mesinger F et al North American Regional Reanalysis. Am. Meteorological Socy. March 2006 pp 343-360. DOI:10.1175/BAMS-87-3-343. Tables 1 and 2 in Mesinger op cit list the historical data information incorporated in the system, which includes temperature data. Thus the statement gives entirely the wrong impression, that the NARR confirmed in some way the USHCN. [Philip Lloyd, South Africa]	Rejected. The NARR reanalysis does not ingest LSAT and therefore retains independence despite the reviewer's assertions to the contrary.
2-645	2	13	45	13	45	LSAT? Or does "overall" refer to land+ocean? [Elizabeth Kent, England]	Noted. LSAT is clarified in revisions.
2-646	2	13	47	13	47	Hansen et al. (2001)'s →Hansen et al.'s (2001) [Peter Burt, UK]	Editorial
2-647	2	13	47	13	50	There is a parenthesis out of place and a punctuation problem ("…in the presence of network-wide systematic biases; with the extent being dependent upon…" should be "…in the presence of network-wide systematic biases, with the extent being dependent upon…"). [Alice Grimm, Brazil]	Editorial
2-648	2	13	48	13	54	Passage starting "Benchmarking unveiled …." to the end of the paragraph is confusing and ambiguous [Elizabeth Kent, England]	Noted. We have tried to simplify and shorten.
2-649	2	13	51	13	51	"When applied to…" -- it is unclear from previous sentences exactly WHAT has been applied. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. Changes to language have been made in an attempt to clarify intended meaning here.
2-650	2	13	51	13	52	"When applied to the observations both minimum and maximum temperatures warm." This is at variance with Fall et al 2011. [Philip Lloyd, South Africa]	Rejected. Fall et al., 2011 looked at solely a subset of the US period of record. There is no conflict between the two studies other than timescale. Some changes have been made to call this out more explicitly.
2-651	2	13	51	13	52	I suggest "When the biases are removed from the observations both minimum and maximum temperature trends increase." [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-652	2	13	52	13	54	General assertions about warming or cooling should meet the IPCC transparency guideline. While the lower 48 States may have warmed as a whole, large regions within their boundaries have not warmed or have even cooled. For example, from Jan 1895 to Nov 2011, Central Texas (NOAA State Code 41, Division 7) cooled by slightly more than 0.1 degree F (based on the means for all months and the IPCC preferred linear trend). Susan Solomon et al., and others have shown that temperatures measured by Thomas Jefferson and by the Lewis and Clark Expedition closely track measurements made for those respective locations today. The air and sea temperatures measured at Hilo, Hawaii, during winter 1840-41, by the US Exploring Expedition were very close to modern measurements. Many other examples can be cited that strongly suggest warming, cooling and stasis over time are spatially variable parameters. [Forrest Mims, USA]	Rejected. It is clear that we are discussing US average temperatures and not local temperatures. So, while the reviewer might be correct it is impossible to discuss such regional detail here. Regional details are shown in maps later in the chapter so are not hidden. They and discussion thereof do not belong here.
2-653	2	13	52	13	54	References for Comment 39: (1) Solomon, Susan S., et al., 2007. American Scientist 95. 430-437. (2) Solomon, Susan S. and John Daniel, 2004. Bull. Am. Met. Soc. 1273-1288. (3) Mims, Forrest, 2012. Hawaii's Mauna Loa Observatory Fifty Years of Monitoring the Atmosphere, Univ Hawaii Press, 43-44. [Forrest Mims, USA]	Noted. See response to comment 2-652
2-654	2	13	53	13	53	What does this mean: "…the lower 48 states…" ? [Alice Grimm, Brazil]	Taken into account. Combined with 2-656
2-655	2	13	53	13	53	The term "lower 48 states" is a bit dated. Although in popular usage, it seems to have made the most sense during the brief period when the US had 49 states, with Alaska not contiguous with the other 48. Hawaii is "lower" (in latitude) than all of the others. [Dian Seidel, USA]	Taken into account. Combined with 2-656
2-656	2	13	53	13	54	"the lower 48 states as a whole have been warming on multi-decadal timescales is unequivocal." Why is it	Accepted

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						necessary to even make such a statement. It adds nothing to the findings, does nothing to provide clarity on issues that ARE equivocal, and is ultimately meaningless. This sentence could readily be deleted. [Philip Lloyd, South Africa]	
2-657	2	13	54	13	54	You coul;d also quote the agreement with SST datasets that presumably are unaffected by urban heat effects,(although this is to some extnt implcit in the agreement with reanalyis datasets that are forced by SST). [Geert Jan van Oldenborgh, Netherlands]	Taken into account. This agreement is mentioned as a possible approach elsewhere. Sentence has been removed in response to 2-656
2-658	2	13	56	13	57	For national assessments, please add Tietäväinen, H, Tuomenvirta, H, Venäläinen, A., 2010. Annual and seasonal mean temperatures in Finland during the last 160 years based on gridded temperature data. Int. J. Climatol.30: 15, 2247-2256. http://onlinelibrary.wiley.com/doi/10.1002/joc.2046/abstract [Kirsti Jylhä, Finland]	Accepted
2-659	2	13	56	13	57	For regional assessment: The tundra climate zone shrank by about 30% the period 1950-78 to 1979-2006. See Jylhä, K., Tuomenvirta, H., Ruosteenoja, K., Niemi-Hugaerts, H., Keisu, K. and Karhu, J.A., 2010. Observed and projected future shifts of climatic zones in Europe, and their use to visualize climate change information. Weather, Climate, and Society, 2:2, 148-67. And references therein. http://journals.ametsoc.org/doi/abs/10.1175/2010WCAS1010.1 [Kirsti Jylhä, Finland]	Noted. The cryosphere is outside of the remit of this chapter so no changes have been made here to avoid chapter cross-talk issues.
2-660	2	13	56	14	4	Here the conclusion about the reliability of the global estimated is based upon the evidence that, for some sub-areas (USA, Europe and East Africa, plus others not detailed), the sign of the long-term changes in mean temperatures equals the one of the global estimates. This conclusion appears to me weak, especially because the mentioned regions are just a small portion of the land (that comprises, for instance, the largest continent: Asia).. [Claudio Cassardo, Italy]	Noted. Additional references have been suggested by other reviewers so this paragraph is now longer and more comprehensive.
2-661	2	13	56	14	4	A review of the rainfall and temperature trends for India is available in the paper: http://cs-test.ias.ac.in/cs/Volumes/102/01/0037.pdf [Sharad K Jain, India]	Accepted.
2-662	2	13	56	14	4	This shorter paragraph is looking at the rest of the world's land areas (98%) but only gets a third of the lines! There has been some excellent work undertaken in Canada that would be worth referencing (by Lucie Vincent). A comparison of Lucie Vincent's adjusted Canadian with what is got from the MW2009 approach would be very useful. Why not produce a Canadian national average frojm both. [Philip JONES, UK]	Noted. We have undertaken to increase the completeness of this paragraph in response to reviewer comments. We do not believe it appropriate to undertake substantive new research in an assessment report so do not undertake an intercomparison as suggested here.
2-663	2	13	57	14	1	Please consider including more references on the long term temperature record and trend in China. A number of studies have been carried out to establish a homogeneous temperature record in China in order to assess the long term temperature variations in China over the last 100 years. References : - Ding, Y.H., G.Y. Ren, G.Y. Shi, P.Gong, X.H. Zheng. P.M. Zhai, D. Zhang, Z.C. Zhao, S.W. Wang, H.J. Wang, Y. Luo, D.L. Chen, X.J. Gao, and X.S. Dai, 2007: China's National Assessment Report on Climate Change (I) : Climate change in China and the future trend, Advances Climate Change Research, 3 (suppl.) 1-5. - Li, Z. and Z.W. Yan, 2009 : Homogenized daily mean/maximum/minimum temperature series for China from 1960-2008, Atmospheric and oceanic science letter, 2(4), 237-243. - Tang, G. L., Y.H. Ding, S.W. Wang, G.Y. Ren, H.B. Liu, and Z. Li, 2010 : Comparative Analysis of China Surface Air Temperature Series for the Past 100 Years, Adv. Clim. Change Res., 1(1), 11-19. [Tsz-cheung Lee, Hong Kong]	Noted. We now include a subset of these in the revised section text.
2-664	2	14	1	14	1	Change "available to the global analyses" to "included in the global analyses", because these data and metadata probably are available to the global analyses if those people doing the global analyses had the resources to make use of them. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Rejected. The reality is that most of these metadata are not available outside the country or even to people withn a given country beyond the NMSs or other data curators. Metadata sharing is far less advanced than data sharing.
2-665	2	14	6	14	54	As to UHI effect on China regional warming, AR5 cited results from Ren et al (2008) and Yang et al(2011) and believe urban warming may account for upwards of 20% in some regions in China. But as concluded in Table 1 of Yang (2011), there are many different kinds of research results on this topic. To my opnion, we can only detect out larger UHI effect in local and relatively small region, but not for a large region or all over China. Of	Noted. We had made substantial efforts to synthesize the papers noted in the original paragraph. Given space restrictions it is not possible to go into the degree of detail requested by the reviewer.

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						them, we'd like to mention Li et al(2010), in this study, Northeast of China is focused, and detected a UHI warming of about 0.027 °C decade-1(8.2%), which is similar with many other studies. Further more, It pointed out that on an annual basis, that Northeast China underwent an abrupt warming of about 1.1 C around 1990, instead of an gradual warming like UHI effect causes.(Reference: Li Q X, Li W, Si P, et al. Assessment of surface air warming in northeast China, with emphasis on the impacts of urbanization [J]. Theor. Appl. Climatol., 2010. 99(3): 469-478.) [Qingxiang Li, China]	
2-666	2	14	6	14	54	Why is no mention made of Irrigation Cooling Effects? Increased irrigation has cooled many areas of the world, the effect is potentially just as important as Urban Heat Island Effects but is not discussed at all. [Geert Jan van Oldenborgh, Netherlands]	Noted. We have tried to recast this section around more holistic LULC issues than just UHI. Unless there is literature we can cite this specific suggestion cannot be included.
2-667	2	14	6		15	I suggest to present the result such as 10% and 20%.....after the assessment of artilce. Because you have reached these values after assessing the artilce. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Noted. This style has been changed throughout the chapter following author team discussions.
2-668	2	14	6			How about the effects of agricultural activities in the form of irrigation on near surface air temperatures? [Shouraseni Roy, USA]	Taken into account combined with 2-666
2-669	2	14	8	14	8	Suggest defining Urban Heat Island here (UHI) on first usage [Peter Burt, UK]	Accepted
2-670	2	14	8	14	8	About the initial part of the sentence: "In AR4 Urban Heat Island effects..", I suggest to include UHI here since it will be used next, in the following way: "In AR4 Urban Heat Island (UHI) effects". [Rubén D Piacentini, Argentina]	Taken into account combined with 2-669
2-671	2	14	8	14	8	Insert "(UHI)" after "Urban Heat Island" and remove it in page 2-14, line 16. [Robert Waterland, United States of America]	Taken into account combined with 2-669
2-672	2	14	8	14	14	There are several statements in this paragraph, but no references to support them. [Alice Grimm, Brazil]	Taken into account. See response to 2-667
2-673	2	14	8	14	20	References should be added. [Douglas Maraun, Germany]	Taken into account. See response to 2-667
2-674	2	14	8	14	54	This section is on UHI effects. It is mostly good, but the paragraph on the previous page (lines 21-36) is also about urban effects, or claimed urban effects. The section on p14 argues that they are mostly small except in a few areas, but the poor papers on p13 argue the effect is ridiculously high. As said the reanalyses get the same surface trends over land areas. [Philip JONES, UK]	Noted. We have pulled this paragraph over to this section and revised this section to cover more than just UHI impacts.
2-675	2	14	8	15	42	For the discussion of the urban heat island effect, it would help to clarify that data estimates of the UHI have not considered the effects of feedbacks of urban surface changes to global climate and have not isolated temperature changes due to urban surfaces from other causes of urban temperature change, such as heat from combustion or carbon dioxide domes over cities. A recent paper used a model to examine the global climate response of urban surfaces at their actual resolution and found that such surfaces contributed to 2-4% of gross global warming (warming before cooling effects are subtracted out): Jacobson, M.Z., and J.E. Ten Hoeve, Effects of urban surfaces and white roofs on global and regional climate, J. Climate, 25, 1028-1044, doi:10.1175/JCLI-D-11-00032.1, 2012. [Mark Z. Jacobson, U.S.A.]	Noted. This strays beyond chapter remit per se in quantifying contributions of LULC to the global totality of climate change, so no changes made to avoid this.
2-676	2	14	8	15	42	The section on urban heat islands would benefit from a short discussion of warming in cities due to carbon dioixde domes over cities Both Jacobson, M.Z., The enhancement of local air pollution by urban CO2 domes, Environ. Sci. Technol., 44, 2497-2502, doi:10.1021/es903018m, 2010 and Balling Jr., R.C.; Cerveny, R.S.; Idso, C.D. Does the urban CO2 dome of Phoenix, Arizona contribute to its heat island. Geophys. Res. Letters 2001, 28, 4599-4601 found a peak contribution over cites themselves of about 0.1 K due to CO2 domes. [Mark Z. Jacobson, U.S.A.]	Taken into account. See response to 2-675
2-677	2	14	9	14	9	You cannot accrue analysis. 'The results of substantial additional analysis have been accrued,....' [Peter Burt, UK]	Sentence has been removed.
2-678	2	14	9	14	9	quantify 'substantial' [Peter Burt, UK]	Taken into account. Combined with 2-677
2-679	2	14	11	14	11	insert comma after 'time' [Peter Burt, UK]	Editorial

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2-680	2	14	12	14	12	urban heat island → UHI [Peter Burt, UK]	Editorial
2-681	2	14	12	14	14	"It is concluded with high confidence that urban heat island effects are real and have real impacts on urban populations but that their impact on the current global Land Surface Air Temperature analyses trends is small compared to the multi-decadal warming signal (less than 10%)." As this is completely unreferenced, then there is no basis for the statement. It is, of course, possible that there will subsequently in this section be support for the view - in which case the conclusion should come AFTER the basis has been presented, not before. Presenting a conclusion before the basis has been given must lead to questioning the motive for making the statement prematurely. [Philip Lloyd, South Africa]	Noted. The text has been moved and redrafted in response to author style guide decisions and this and various other feedbacks.
2-682	2	14	12	14	14	The claim that land surface changes add no more than10% to the global trend over land is made up out of thin air, from whence it goes into the chapter summary and then inevitably into the SPM. Either present some actual evidence or take it out. You discuss the McKitrick and Michaels work which actually presents some empirical computations to support a claim of one-third to one-half of the observed trend being non-climatic in origing. In the past the 10% number was attributed to the Jones et al. 1990 paper, which, aside from being over 20 years old, was unfounded anyway since the claim appeared in the conclusion to that paper with no derivation in the data itself. [Ross McKitrick, Canada]	Taken into account combined with 2-681 and 2-684
2-683	2	14	13	14	14	Land Surface Air Temperature → land surface air temperature [Peter Burt, UK]	Editorial
2-684	2	14	14	14	14	Where does that 10% come from? Is this the same as an order of magnitude smaller? [Philip JONES, UK]	Noted. We have removed this (10%) as it clearly did not help in interpretation as evidenced by this and other comments.
2-685	2	14	14	14	53	It should be clarified what the percentages refer to: amplitude of the trend? Explained variance by the trend? [Douglas Maraun, Germany]	Taken into account. Combined with 2-684
2-686	2	14	16	14	16	delete Urban Heat Islands [Peter Burt, UK]	Sentence has been redrafted and this comment is no longer applicable.
2-687	2	14	16	14	16	UHI → UHIs [Peter Burt, UK]	Editorial
2-688	2	14	16	14	16	About the sentence: "Urban Heat Islands (UHI) form because the modified surface affects the storage and transfer of heat", please eliminates (UHI) from this sentence, since it was introduced before (page14, line 8). [Rubén D Piacentini, Argentina]	Editorial
2-689	2	14	16	14	16	Replace "Urban Heat Islands (UHI) form because the modified surface affects the storage and transfer of heat." with "Urban Heat Islands form because land surface modifications that accompany economic development affect the storage and transfer of heat." [Robert Waterland, United States of America]	Rejected. Urbanisation need not always be accompanied by economic development, and for us to state so may leave us looking naïve.
2-690	2	14	16	14	19	Reference Chapter 10 discussion about climate changes from land use changes 10.7.2.2? [Gareth S Jones, UK]	Noted. At this time it seems a little dangerous to refer to sections of other chapters which themselves are still evolving in structure and content. Perhaps there is a role for TSU / RE's in suggesting such cross-linkages in the next version which will be closer to finalized?
2-691	2	14	16			the sentence “Urban Heat Islands (UHI) form because the modified surface affects the storage and transfer of heat” would be written “Urban Heat Islands (UHI) form mainly because the modified surface affects the storage and transfer of heat”. [taking into the account that in the formation of UHI participate several factors such as the gases emissions and the home, industrial and transport processes]. [José Daniel Pabón-Caicedo, Colombia]	Accepted
2-692	2	14	17	14	17	Consider changing "metropolis" to "metropolitan" [Dian Seidel, USA]	Noted. Text has been removed in editing.
2-693	2	14	18	14	18	insert 'the' after 'of' [Peter Burt, UK]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-694	2	14	19	14	20	“Many of these sites have potentially experienced increasing UHI effects not representative of broader regions.” I do not know what was intended with the inclusion of this sentence, but it seems to me that it is at odds with what was stated in the previous lines and previous paragraph: that the UHI effect has a little effect of the global warming signal. If many of the sites with temperature observations have experienced increasing UHI effects not representative of broader regions, and if these observations are used to calculate a global average, it seems logical to conclude that there is a significant contribution of the UHI effect to the estimated global warming. [Alice Grimm, Brazil]	Noted. The text has been substantially redrafted for clarity.
2-695	2	14	22	14	42	This paragraph references comparisons between station records and reanalyses. It might be instructive to include a mention of any relevant uncertainties in the reanalyses here to put the differences into context. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Noted. There is a box on reanalyses earlier in the chapter which is intended to provide precisely this context. Some discussion of renalayses is also made in this revised section now.
2-696	2	14	22	14	42	Have you considered the issue of representativeness of measurement within green spaces and parks in cities? See Dousset et al. (2011). [Dousset, B., F., Gourmelon, K. Laaidi, A. Zeghnoun, E. Giraudet, P. Bretin, E. Mauri, S. Vandentorren, "Satellite monitoring of summer heat waves in the Paris metropolitan area", Int. J. Climatol. 31: 313-323 (2011)] [Dian Seidel, USA]	Noted. This paper was considered in redrafting but ultimately it was decided not to cite it.
2-697	2	14	23	14	23	change rather than changes [Elizabeth Kent, England]	Editorial
2-698	2	14	26	14	26	The statement "could be upwards of 20%" is not very useful. "Could be" means that it might not be. Hence it is saying that the value could be more or less than 20%, which covers any value at all. Why not give a range (perhaps a "likely" range) of possibilities? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have used more precise language here but a range would be hard to garner from the range of assessments and place any confidence in that range being meaningful
2-699	2	14	27	14	28	I think "Observations-minus-reanalysis trends" needs some explanation. [Robert Waterland, United States of America]	Noted. These are covered in more detail elsewhere in the chapter / appendix.
2-700	2	14	27	14	30	2 sentences with the same meaning to me... only one would be enough? [Francois DANIS, France]	Accepted. Second sentence has been removed.
2-701	2	14	28	14	28	Lim et al., 2008 is a worldwide study, not just for China. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. This discussion has been removed in the redraft.
2-702	2	14	29	14	30	I strongly suggest avoiding this construct (i.e., the use of parentheses to distinguish opposite effects). It is confusing. See discussion of the issue by Robock (EOS, vol. 91, no. 56, Nov. 9, 2010) [Dian Seidel, USA]	Taken into account. See response to 2-700
2-703	2	14	30	14	30	The word “respectively” is superfluous. [Alice Grimm, Brazil]	Taken into account. See response to 2-700
2-704	2	14	33	14	33	superscript 'th' [Peter Burt, UK]	Editorial
2-705	2	14	33	14	35	This remark about China seems to be conjecture, since no reference is given. [Dian Seidel, USA]	Accepted. Sentence deleted.
2-706	2	14	37	14	37	urban heat island → UHI [Peter Burt, UK]	Editorial
2-707	2	14	39	14	39	van der Schrier et al (2011, Clim. Past, doi:10.5194/cp-7-527-2011) show that the movement of stations from city centres to airports in the early 1950s caused an UHI-related spurious cooling trend in the Netherlands that should be corrected for. [Geert Jan van Oldenborgh, Netherlands]	Taken into account. Paper is now cited in the regional paragraph. In the context of the redrafted section text it was hard to see where to include this specific suggestion.
2-708	2	14	44	14	45	How can you say that "Estimates of large-scale temperature change have tended either to avoid urban observing sites…" when NOAA, GISS and CRU are all heavily dependent on the GHCN, and 67% of Southern Hemisphere GHCN data come from urban airports? [Ross McKitrick, Canada]	Noted. The reviewer neglects to mention the second half of the sentence which addresses their own criticism. Where there is no option but to use urban sites they should be used appropriately.
2-709	2	14	44	14	54	The problem with these analyses is that they split the sample based on a rural/urban criterion and then look for a difference of trends. But such results, regardless of what they find, can be consistent with the presence or absence of contaminating effects on the surface record due to land surface disruption. For instance, if the UHI effect is linear in population size, observing the same trend in rural and urban data would either indicate that	Rejected. UHI impacts only become important in the context of large scale trends if the UHI grows / shrinks and therefore a non-representative trend bias is imparted into the data. If a station is always biased

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						there is no UHI effect, or that it is the dominant pattern in the data. The only way to test these things is to construct a statistical model that encompasses rival explanations and can credibly discriminate between them. McKitrick (2012) gives examples of such models. [Ross McKitrick, Canada]	10K relative to a pristine environment and this bias is stable through time then, when considered as anomalies, it has no impact.
2-710	2	14	44	14	54	McKitrick, Ross R. (2012) "Encompassing Tests of Socioeconomic Signals in Surface Climate Data." submitted. [Ross McKitrick, Canada]	Noted. Paper is now included.
2-711	2	14	44			"Estimates of large-scale temperature change have tended either to avoid urban observing sites" - but surely this is at variance with the findings of Fall et al., 2011? That study found almost the reverse - the majority of the stations in the UHSCN network were urban and less than ideal according to NOAA criteria. Granted, this only applies to the US, but given the intensity of study there, it leads to questioning of all sites. [Philip Lloyd, South Africa]	Rejected. This is a statement as to how the cited studies have variously tried to account for the sampling issues. This context is made clearer in the revised text.
2-712	2	14	46			Hansen et al. should not be in parentheses, only the year. [Michael Brewer, United States of America]	Editorial. Final copy-editing will need to address all such cases.
2-713	2	14	47	14	47	Last word of this line "lit" is unclear [Sharad K Jain, India]	Noted. Text has been edited for brevity.
2-714	2	14	47		48	There are ssome strong national differences in night lights (a map of Europe at night is quite revealing) but may not reflect urbanization. Is this accounted for? [Larry Thomason, United States of America]	Noted. The nightlights dataset is nationally invariant so it won't be. Given the lack of a peer reviewed basis to the reviewer's comment or its effect on the data processing it is not possible to make any changes here. This is an assessment of the literature so if such an effect were documented therein we would be able to consider citing and discussing it. We are unaware of such a paper.
2-715	2	14	49	14	49	(Wickham et al., submitted) → Wickham et al. (submitted) [Peter Burt, UK]	Editorial. Final copy-editing will need to address all such cases.
2-716	2	14	50	14	50	Is "resolution" a simpler word than "granularity"? [Dian Seidel, USA]	Accepted
2-717	2	14	52	14	52	Space before Efthymiadis [Peter Burt, UK]	Editorial
2-718	2	14	52	14	52	Is this upper limit of the urban influence meant to be for local or global trends? [Dian Seidel, USA]	Accepted. Global added.
2-719	2	14	52	14	54	About the sentence: "Efthymiadis and Jones (2010) estimated an absolute upper limit on urban influence of 0.02°C per decade, or ~15% of the total trends, in 1951–2009 from trends of coastal land and sea surface temperature but argued on physical theoretical grounds that the true value was likely to be lower than this". Could the Authors explain in more detail which are the physical theoretical grounds and which is the reduction of the percentage total trend? [Rubén D Piacentini, Argentina]	Noted. This detail is in the paper but space constraints mean that it is not possible to pull through this reasoning to the front here.
2-720	2	14	52			Space needed between 2010. and Efthymiadis [Michael Brewer, United States of America]	Editorial
2-721	2	14	56	15	42	This chapter explain with many references and links the complexity of the datum "DTR", but at the end it does not draw a conclusion. [Claudio Cassardo, Italy]	Noted. The conclusion drawn reflects the state of the literature on the issue which is that it is highly uncertain.
2-722	2	14				2.2.1.3 Need to reinforce view in AR4 that UHI effects are real and so very locally warming is often greater than the global trends. [Kevin Trenberth, USA]	Accepted. Have nuanced the second paragraph to this effect.
2-723	2	15	1	15	1	Diurnal Temperature Range → diurnal temperature range [Peter Burt, UK]	Rejected. Capitalization is used to define the acronym that follows.
2-724	2	15	1	15	1	Replace "minimum increasing" with "minimum daily temperatures increasing". [Robert Waterland, United States of America]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-725	2	15	8	15	8	(Vose et al., 2005a) → that of Vose et al. (2005a) [Peter Burt, UK]	Editorial. Final copy-editing will need to address all such cases.
2-726	2	15	8	15	8	"No global analysis of DTR has been" [George Kiladis, USA]	Editorial
2-727	2	15	9	15	9	(Makowski et al., 2009) find → Makowski et al. (2009) found [Peter Burt, UK]	Editorial. Final copy-editing will need to address all such cases.
2-728	2	15	9	15	9	Replace "this time" with "AR4". Replace "find" with "found". [Robert Waterland, United States of America]	Editorial
2-729	2	15	11	15	11	and 1980s → and in the 1980s [Peter Burt, UK]	Editorial
2-730	2	15	13	15	14	so the diurnal tempertaure range has continued to decrease? If so, say so (and give the changes in temperature)! [Peter Burt, UK]	Noted. This is now addressed elsewhere in this sub-section.
2-731	2	15	16	15	20	Add a short explanation why the studies show a greater DTR on the weekends [Birgit Hassler, USA]	Taken into account. See response to 2-732
2-732	2	15	16	15	20	Is this weekend effect mentioned in AR4 worth discussing again? [Philip JONES, UK]	Accepted. Paragraph has been deleted
2-733	2	15	16	15	20	Could delete the short paragraph aobut the weekend effect on DTR. It seems of little significance. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-732
2-734	2	15	17	15	17	delete comma after Japan [Peter Burt, UK]	Taken into account. See response to 2-732
2-735	2	15	19	15	20	Should this discussion of DTR differences between weekends and weekdays be related to discussion of DTR trends? [Dian Seidel, USA]	Taken into account. See response to 2-732
2-736	2	15	22	15	23	Replace "Various investigators (e.g., Christy et al., 2009; Pielke et al., 2007) have raised on theoretical grounds and for specific areas doubts over the interpretation of minimum temperature trends." with "On theoretical grounds, various investigators (e.g., Christy et al., 2009; Pielke et al., 2007) have raised doubts about the interpretation of minimum temperature trends." [Robert Waterland, United States of America]	Editorial
2-737	2	15	22	15	33	is this true when the study related to the extreme events such as th cold spell ? , please see box 2.5 table 1 page 2-75 chapter 2 in this report. [ALI GEATH ELJADID, LIBYA]	Noted. It is unclear what specific changes the reviewer is requesting so none can be made here.
2-738	2	15	22	15	33	It is of interest to note here that part of the original conclusions by Parker(2004) were recently confirmed by a detailed model study of the stable boundary layer (Steeneveld et al, 2011). In fact Parker (2004) showed similar long-term trends for calm and windy conditions at night, and on basis of this it was suggested that the possible effect of urban heat effects on long-term temperature trends are small. Later on a simplified analytic model study by Pielke and Matsui (2005, PM05) suggested that at night the resultant longterm temperature trends over land should depend on height and strongly on wind speed. In the paper by Steeneveld et al (including Roger Pielke as co-author), the PM05 study is revisited using a validated atmospheric boundary layer model with more elaborated atmospheric physics as compared to PM05. As such the response of the SBL over land to a change in radiative forcing is explored and it is find that the screen level temperature response is surprisingly constant for a rather broad range of both geostrophic wind speed (5–15 m s−1) and 10 m wind (2–4.0 m s−1). This is mostly due to land surface-vegetation-atmosphere feedbacks taken into account in the new study which were not considered by PM05. Reference: Steeneveld, G. J., A. A. M. Holtslag, R. T. McNider, and R. A. Pielke Sr. (2011), Screen level temperature increase due to higher atmospheric carbon dioxide in calm and windy nights revisited, J. Geophys. Res., 116, D02122, doi:10.1029/2010JD014612. [Albert A.M. Holtslag, Netherlands]	Accepted. The paper and appropriate words will be included in the redraft.
2-739	2	15	22	15	33	When discussing temperature in the context and max and min temps, it is worth pointing out that most countries (i.e. more than 50%) don't use max and min temps in their calculation of mean temp. Again there is a US or an Anglo bias in discussion of how monthly mean temperature is calculated. [Philip JONES, UK]	Noted. Have removed the end words of the paragraph.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-740	2	15	22	15	33	The text says "Parker (2006) used the difference between calm and windy nights to address issues with minimum temperature trends and found no difference between trends for calm and windy nights on a global average basis. If the data were fundamentally affected, as posited, then a difference would be expected." But McKitrick (2012 -- ref in cell 32) showed on a sample of Canadian cities that, even if the absence of a UHI implies trends will be the same under calm and windy conditions, it does not follow that a similarity of trends between calm and windy conditions implies the absence of a UHI, since such similarity was observed on data that were shown on other grounds to be affected by urbanization. [Ross McKitrick, Canada]	Rejected. The question is not the presence of an UHI but the changing UHI influence imparting a spurious bias. Presence of a UHI effect that is stationary has no impact upon inferred trends. It is only in the limit that the effect is non-stationary that it matters for characterizing trends.
2-741	2	15	22	15	33	This paragraph opens by attempting to address the claim that minimum temperature trends should be neglected. Although there is some discussion on this topic in the paragraph, it does not actually speak to minimum temperature trends -- this should be directly discussed. [Jeffrey Taylor, United States of America]	Noted. Length restrictions mean this is not possible.
2-742	2	15	26	15	30	Steeneveld, G. J., A. A. M. Holtslag, R. T. McNider, and R. A. Pielke Sr. (2011), Screen level temperature increase due to higher atmospheric carbon dioxide in calm and windy nights revisited, J. Geophys. Res., 116, D02122, doi:10.1029/2010JD014612. looked in more detail on the issues raised by Parker. They confirm the results of Parker (i.e. that trends are the same with different wind speeds) but also show that trends are still dependent on the height of the measurement. [Marcel Crok, The Netherlands]	Taken into account. See respomnse to 2-738
2-743	2	15	30	15	30	Specifically, this would in most cases act to reduce nighttime temperature, which should be mentioned [George Kiladis, USA]	Noted. This text has been removed.
2-744	2	15	30	15	33	It depends. I agree of course that we all want to understand both the trends in Tmax as in Tmin. However so far models do a very poor job in this, see eg this presentation by McNider at ECMWF: http://www.ecmwf.int/newsevents/meetings/workshops/2011/GABLS/presentations/McNider.pdf where he shows that the trend in Tmin is more than double that of Tmax. Models capture only 20% of this trend difference. As explained in Walters, J. T., R. T. McNider, X. Shi, W. B Norris, and J. R. Christy (2007): Positive surface temperature feedback in the stable nocturnal boundary layer, Geophys. Res. Lett., 34, L12709, doi:10.1029/2007GL029505 higher trends in Tmin are very likely the result of nightly turbulent mixing. This can also be caused by downward radiation of CO2 or water vapor. As McNider comments in his ECMWF presentation: "Thus, this acts as a positive climate feedback in that slight changes in downward radiation can lead to large changes in temperature. However, it is not a net energetic increase but only a redistribution of heat!" This is a major issue as in later chapters Taverage is used as a climatological metric for the accumulation of heat. It would be very instructive if you could show the large trend differences in a figure as well. Comparison with the models should be done in chapter 9 and in chapter 10 they should give the reader a warning that T2m is maybe/probably not representative for a larger area. [Marcel Crok, The Netherlands]	Noted. In the space available it is not possible to explore further. Much of comment also pertains to other sections as the reviewer themselves alludes to. We are also charged with assessing the literature and not presentations.
2-745	2	15	33	15	33	are necessary to include → should be included [Peter Burt, UK]	Editorial
2-746	2	15	33	15	33	Change "are necessary to include." to "must be included." [Robert Waterland, United States of America]	Editorial
2-747	2	15	40	15	42	With adjusted data the difference in trends is still there, see the ECMWF presentation of McNider: http://www.ecmwf.int/newsevents/meetings/workshops/2011/GABLS/presentations/McNider.pdf [Marcel Crok, The Netherlands]	Noted. As this is not in the literature (to our knowledge) it cannot be factored into our analysis
2-748	2	15	44	15	55	The historical sea surface temperature record is not fully considered. For example, the published air and sea temperatures measured at Hilo, Hawaii, during winter 1840-41, by the US Exploring Expedition were very close to modern values. These and numerous other SST measurements during the US ExEx voyage around the world were carefully made with calibrated thermometers, often under dangerous conditions, and it would be inappropriate to fail to note that SST at Hilo well before the IPCC cutoff ("late 19th Century") was very close to modern values. (I have not checked other locations.) Sample data comparisons and references to the published Smithsonian data can be found at Mims, Forrest, 2012. Hawaii's Mauna Loa Observatory Fifty Years of Monitoring the Atmosphere, Univ. Hawaii Press, 43-44. [Forrest Mims, USA]	Noted. The present assessment is based only on the research that has been published in peer-revied literature as per explicit guidance from the TSU.
2-749	2	15	44	20	24	Section 2.2.2 is much too long and contains too much detail. Identify what is new since AR4 and focus on that. [Dian Seidel, USA]	Accepted. The section has been re-edited, with a large portion of the material excluded or relegated to the Appendix.

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2-750	2	15	46	15	55	Has the issue with ship call signs been resolved? These have been removed after 2006. Can IPCC make a recommendation that they be reinstated? [Philip JONES, UK]	Rejected. Outside of the charge of this chapter. IPCC has to be policy neutral and this is not a policy neutral statement.
2-751	2	15	50	15	50	insert 'the' after 'in' [Peter Burt, UK]	Editorial
2-752	2	15	50	15	50	Was the "step change in availability" actually an "increase"? If so, say that. [Dian Seidel, USA]	Accepted
2-753	2	15	50	15	50	Do you mean "data" rather than "metadata"? [Robert Waterland, United States of America]	Rejected. Written as intended.
2-754	2	15	50			is this "steep" [Shouraseni Roy, USA]	Taken into account. Combined with 2-752
2-755	2	15	50			"a step change" could be a step up (improvement) or step down (less capable): need to use a clearer adjective: "major improvement"? [Bruce Wielicki, USA]	Taken into account. Combined with 2-752
2-756	2	15	54	15	54	It might be clearer to write "do no alter the conclusion that, with very high confidence, global SSTs have increased" otherwise it sounds like the conclusion is being altered albeit with little confidence. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-757	2	15	54	15	54	ambigous as to what the confidence is related to. Are you confident that the conclusion has not changed or that global SSTs have increased? [Elizabeth Kent, England]	Taken into account. Combined with 2-756
2-758	2	15	54	15	54	Replace "do not alter the conclusion with very high confidence that global" with "do not alter the conclusion that, with very high confidence, global" [Robert Waterland, United States of America]	Taken into account. Combined with 2-756
2-759	2	15	55	15	55	superscript 'th' [Peter Burt, UK]	Accepted
2-760	2	15				Section 2.2.2: in sea-surface temperature, sea-surface ought to be hyphenated consistently. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-761	2	16	3	16	16	Is the table 2.3 reference appropriate here/ [Karen Rosenlof, United States of America]	Rejected. Table 2.3 provides details of ICOADS relevant to this sentence.
2-762	2	16	4	16	5	Replace "Most historical SST observations arise from ships, with buoy measurements and satellite data becoming a significant contribution in the 1980s." with "Historically, most SST observations were obtained from moving ships. Buoy measurements and satellite data comprise a significant fraction of SST measurements from the 1980s onwards." [Robert Waterland, United States of America]	Editorial
2-763	2	16	10	16	12	When discussing medians, should standard deviations be used? What about median average deviation? [Jeffrey Taylor, United States of America]	Rejected. Median average deviations are not included into ICOADS summaries.
2-764	2	16	11	16	11	means or medians of data which pass QC are representative of QC'd data not reported data [Elizabeth Kent, England]	Accepted. Changes have been made to clarify
2-765	2	16	13	16	13	Replace "possibly" with "in some cases". It isn't a case of possibility: some are, some are not. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-766	2	16	14	16	16	Perhaps mention of the 1840-41 Hilo measurements could be cited here to illustrate why more study of US ExEx and other very early SST records is badly needed. Detailed US ExEx and modern Hilo references sent on request (or see Mims in Comment 41). [Forrest Mims, USA]	Noted. See response to 2-748
2-767	2	16	15	16	15	Earth → Earth's [Peter Burt, UK]	Accepted
2-768	2	16	22	16	25	Should explicitly state here that all the adjustments have some link to NMAT [Elizabeth Kent, England]	Taken into account, sentence rewritten.
2-769	2	16	25	16	25	Night Marine Air Temperatures →night marine air temperatures [Peter Burt, UK]	Rejected. This is the first use of the term and afterwards it is referred to by the acronym so the capitalization is appropriate here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-770	2	16	25	16	28	the reason that bias adjustments were not applied to earlier analyses was that there was no clear evidence that they were needed (see Folland and Parker 95, Smith and Reynolds 2002). This led to the hypothesis that after 1941 there was a more homogeneous mixture of measurement methods including better insulated buckets and engine intake measurements. When the number of observations in the underlying database was increased, the mix changed and those assumptions needed to be revisited. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted
2-771	2	16	25	16	28	Adjustments were not applied after 1941, but the SST analysis produced by Smith et al. included an extra uncertainty term that allowed for some error due to uncompensated biases in the SST record. (Smith, Thomas M., Richard W. Reynolds, (2003): Extended Reconstruction of Global Sea Surface Temperatures Based on COADS Data (1854-1997). J. Climate, 16, 1495-1510. and Smith, Thomas M., Richard W. Reynolds, (2005): A Global Merged Land-Air-Sea Surface Temperature Reconstruction Based on Historical Observations (1880-1997). J. Climate, 18, 2021-2036. doi: 10.1175/JCLI3362.1) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted.
2-772	2	16	27	16	28	Replace "more advanced measurement methods as well as buckets of improved design became popular in the later period; these exhibited smaller systematic biases (Figure 2.2, top)." with "more advanced, less biased, measurement methods as well as buckets of improved design became popular in the later period (Figure 2.2, top)." [Robert Waterland, United States of America]	Editorial
2-773	2	16	30	16	30	bad English, and scientific, expression. I suggest deleting 'warm' [Peter Burt, UK]	Editorial. Suggestion to delete "warm" rejected.
2-774	2	16	31	20	48	It is necessary that all temperature units be in celsius degree (°C), not some in Kelvin degree [Michel Boko, Benin]	Editorial
2-775	2	16	42	16	42	This figure does not appear in Kennedy et al 2011b [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference corrected to Kennedy et al 2011c
2-776	2	16	44	16	46	Suggest replacing "… has been assuming … inversely proportionally to …" with "has been to assume ….. In inverse proportion to .." [Elizabeth Kent, England]	Accepted
2-777	2	16	44	16	51	The use of the word platform is confusing here. Platform is often used generically to mean 'all ships' or 'all drifting buoys'. It is equally often used to refer to a single ship, or buoy. The text needs to be changed to make it even more clear that each individual ship and each individual buoy is considered to have an associated - unknown - bias. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-778	2	16	47	16	47	to allow → allowing [Peter Burt, UK]	Accepted
2-779	2	16	50	16	50	"error estimates" should be "uncertainty estimates". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-780	2	16	50	16	50	Change 2011c to 2011b. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-781	2	16	53	16	53	"Although noisier than SSTs…" -- be clearer about what you mean. Less accurate? More (real) short-term variations? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-782	2	16	53	16	53	Replace "MATs" with "Marine Air Temperatures (MATs)" [Robert Waterland, United States of America]	Accepted
2-783	2	16	53	16	55	The claim that MAT and SST are constrained to track each other needs to be supported. The 2 papers cited actually show evidence against the claim. It would be more accurate to say "It is typically assumed that MATs are physically constrained to track SST variability, though in the few places where this has been tested it has not been found to be a reliable assumption." [Ross McKitrick, Canada]	Taken into account. The sentence is re-written (and moved to the Appendix).
2-784	2	16	53	17	2	Another use of NMAT data is to constrain early SST estimates. This could be mentioned here or in the later section on bias adjustment. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Mentioned in another paragraph.
2-785	2	16	55	16	55	Replace "Thus they provide an independent measure of marine temperature change." with "MATs provide an independent measure of SSTs." [Robert Waterland, United States of America]	Rejected: MAT is not a "measure of SST"

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2-786	2	16	57	16	57	nineteenth → 19th [Peter Burt, UK]	Accepted
2-787	2	16	58	16	58	Please provide the meaning of NMATs. [Claudio Cassardo, Italy]	Rejected. NMATs have already been defined on first usage.
2-788	2	16		20		Sections 2.2.2.1 and 2.2.2.2 are very good and helpful. [Alice Grimm, Brazil]	Noted.
2-789	2	16				Figure 2.2: the term "Global annual average SST anomaly" should be better explained -- what do these anomalies represent? [Jeffrey Taylor, United States of America]	Accepted. Precise definition provided.
2-790	2	17	4			Section 2.2.2.1.2: The ATSR time series is referenced, but there are published global temperature series from AVHRR which could be cited (with the caveat that they are not completely independent of in situ data) including, Good, S. A., G. K. Corlett, J. J. Remedios, E. J. Noyes, D. T. Llewellyn-Jones, 2007: The Global Trend in Sea Surface Temperature from 20 Years of Advanced Very High Resolution Radiometer Data. J. Climate, 20, 1255–1264. and Lawrence, S. P., D. T. Llewellyn-Jones, and S. J. Smith (2004), The measurement of climate change using data from the Advanced Very High Resolution and Along Track Scanning Radiometers, J. Geophys. Res., 109, C08017, doi:10.1029/2003JC002104. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected. Global mean time series from satellite (ATSR) SSTs are only presented in this section to illustrate the improvements in historical climate data sets, not as standalone descriptions of global mean SST variability.
2-791	2	17	10	17	37	in situ is, correctly, italicised here, but not in the rest of the Chapter. [Peter Burt, UK]	Editorial
2-792	2	17	12	17	12	Rather than "usually through reference to in-situ measurements" but increasingly for climate through "physical modelling of radiative transfer independent of such references". This is a major point of the ATSR series. A reference would be Llewellyn-Jones and Remedios, The Advanced Along Track Scanning Radiometer (AATSR) and its predecessors ATSR-1 and ATSR-2: An introduction to the special issue, Remote Sensing of the Environment,116, 1-3, 2012 as a placeholder. I suspect there will be a full ATSR description paper shortly which would be cited instead and further referenced in the ATSR paragraph [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Noted. The point about the independence of ATSR series from in situ data has been made further in the text; different references have been used.
2-793	2	17	15	17	15	Replace "The majority of satellite SST data arise from operational meteorological sensors." with "The majority of satellite SST data are collected by sensors primarily designed for meterological purposes." [Robert Waterland, United States of America]	Editorial.
2-794	2	17	15	17	16	Replace "The principal IR sensor is the Advanced Very High Resolution Radiometer (AVHRR) series." with "The principal IR sensors are the Advanced Very High Resolution Radiometer (AVHRR) instruments carried upon a number of polar orbiting satellites." You should also mention the microwave sensors (AMSU A1 and AMSU A2). [Robert Waterland, United States of America]	Editorial. Suggestion to specify MW sensors in more detail is rejected, since no difference between them is drawn in the text (IR sensors are described in more detail b/c of the need to distinguish between AVHRR and (A)ATSR time series).
2-795	2	17	15	17	34	In this section, AVHRR and ATSR SST data records are introduced and subsequently in Figure 2.3 ATSR data is shown. But why [Klaas Folkert Boersma, Netherlands]	Rejected, being taken as a suggestion to exclude the description of satellite SST records, since the latter provided a foundation for the recent progress in the development of historical SST data sets and estimates of their uncertainties.
2-796	2	17	25	17	34	There is at least one factor associated with the ATSR data that need to be flagged. There were problems with the first of the satellites, ATSR1, such that observations from it are less reliable than those from later satellietes due to failure of one of the IR channels and problems with the cooling systems. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected. This level of detail cannot be sustained in the AR5; the shortcomings of the ATSR are described in the references cited in this paragraph.
2-797	2	17	25	17	34	Its implicit in this text that AATSR and ATSR2 are part of the ATSR series. Given the detailed description of uncertainties that follows on the next page it might help to list them explicitly and give the years of operation. [Elizabeth Kent, England]	Accepted.
2-798	2	17	25	17	34	You need to be careful to be clear on which AATSR data is being quoted (operational-like, V2.0 archive, ARC V1.1 etc). You also need to be careful of the word "accurate". Are they more accurate, precise or more "temporally consistent". How well can we establish abolute biases? Probably not to better than 0.1-0.2 K	Accepted, regarding the specification of the AATSR product. Rejected, regarding detailed description what "accurate" means here: this is explained in the cited

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						although we have very good confidence in relative bias of homogenised data. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	reference.
2-799	2	17	25	17	34	Veal et al, 2011, A time series of global mean skin-SST anomaly using Version 2.0 operational data from ATSR-2 and AATSR (K. L. Veal, G. K. Corlett, D. Ghent, D. T. Llewellyn-Jones and J. J. Remedios). Abstract: A time series of global mean, skin-SST anomaly has been calculated for 1995 to 2011, based on the first consistently processed data set for the Along Track Scanning Radiometer(s) or ATSRs. The ATSR sensors are accurate and stable instruments, with similar orbits and equator crossing times, specifically designed to obtain global sea surface temperature (SST) for climate research. Radiometric calibration and atmospheric correction techniques allow ATSR SST values to be derived independently of in situ data. The analysis procedure in this study utilises the ATSR multi-mission Version 2.0 (V2) archive to derive level 3 averaged skin-SSTs for ATSR-2 and AATSR consistently for both sensors using a standard operational-like processor. Data are then gridded, aligned between sensors and a final anomaly time series derived relative to a climatological average calculated from the standard ATSR data for the same period; the process strongly reduces global mean and spatially varying biases. The ATSR-2 to AATSR SST bias is obtained by analysing the overlap period (July 2002 to June 2003) between the two sensors. Before alignment, the average bias between V2 SST data sets from the two instruments is +0.03 K for the dual-view, three channel SST, with a small but linear relation to water vapour in the differences between ATSR-2 and AATSR. Post-alignment, the residual average bias is 0.007 K. The sampled area for the final time series using the V2 data extends from 60°N to 65°S; it is recommended that sampling areas are always provided for time series of data for climate. The uncertainty on the monthly mean global SST anomaly is estimated to be 0.035 K for ATSR-2 and 0.025 K for AATSR which are comparable to the uncertainties on monthly global average in situ data sets in the equivalent time period. Hence the ATSR data sets provide an excellent and independent reference for testing in situ data sets and climate models over a period in excess of 15 years of data. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Rejected. See 2-790
2-800	2	17	25	17	39	The detailed comparison of ATSR and HadSST3 is good, but I wish that the authors would take a step back from the details and address the broader question that jumps out from Fig 2.3, namely, what are the implications for Earth's energy budget of a short-term (2-year), 0.3 ºC increase in SST around 1998? That implies a lot of additional energy in the global system that had to come from somewhere quickly and go to somewhere else quickly. Where are those "somewheres", and are there data from those reservoirs that corroborate the SST data? [JOHN OGREN, USA]	Rejected. Outside of the scope of this section: observation-based energy balance is discussed in Chapter 3.
2-801	2	17	27	17	28	Kennedy et al. used AATSR SST retrievals to estimates biases in in situ observations, not ATSR. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-802	2	17	30	17	30	I would say somthing like "Since AR4, ATSR observations have been reprocessed consistently in the V2.0 archive (Veal et al, 2011) and with new estimation techniques in the ATSR Re-analysis for Climate or ARC project (Embury and Merchant, 2011). The new AATSR SSTs seem to be more accurate than many in situ observations (Embury et al., 2011) and to have comparable uncertainties on global monthly mean SST anomaly fields to the combined in situ records (Veal et al, 2011)." I'm not sure in what way "the more accurate" is meant. If in a global monthly sense, then it should be "comparable to (Veal) or less than the in situ (Embury)", else clarify in what you mean more accurate bearing in mind previous comment [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Rejected. The meaning is as cited: for local estimates of SST, not for the global means.
2-803	2	17	30	17	30	A phrase like "seem to be" should not be used in this assessment, as it lacks rigor. [Dian Seidel, USA]	Rejected.This is the phraseology used in the cited paper
2-804	2	17	30	17	34	I would say strongly recommend " ATSR SSTs are derived by techniques which are independent of in situ data, and although having marginal dependence on in situ observations through auxiliary data used for ARC processing, the ATSR SST time series of global monthly mean SST anomaly is highly coherent with that obtained using only in situ observations (represented in Figure 2.3 by the HadSST3 ensemble)." Note the V2 archive of SST data is most likely less accurate in an absolute sense per measurement than ARC, but is similar after homogenisation and is independent of in situ data. The coherence with in situ data is similar, although the extra benefit of ARC is a properly calculated sub-skin SST. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Editorial, regarding the sentence re-writing. Noted, regarding V2 vs ARC comparison for ATSR archives. See 2-790.

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2-805	2	17	32	17	33	instead of saying that the ATSR SSTs are "highly coherent" with the in situ obs, would be more informative to say what the correlation coefficient is (or some other statistcal measure, such as RMSE or better still the RE). [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Comparison of ATSR and in situ SST is discussed quantitatively elsewhere in the section and in the Appendix.
2-806	2	17	33	17	33	Characterizing the two time series as "coherent" is vague and does not explain the ~0.1 K offset between them seen in Fig. 2.3. [Dian Seidel, USA]	Accepted. The sentence will be re-written and Figure 2.3 replaced by the one that illustrates it better.
2-807	2	17	33	17	34	A brief statement explaining why HadSST3 data set has 100 ensemble members would be necessary [Celeste Saulo, Argentina]	Accepted: calrifications added.
2-808	2	17	33			figure 2.3. As a lay reader, I would be interested in knowing why, up to 1996, there is a difference between satellite and in-situ measurements... A systematic offset from the satellite? No time to check but, a comment would be welcomed; especially as, line 12, you say that satellites are "calibrated" against in-situ measurements. [Francois DANIS, France]	Taken into account. Combined with 2-806
2-809	2	17	34	17	34	Suggest referencing Kennedy et al 2011b and c here after mention of HadSST3 [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-810	2	17	36			Figure 2.3. There is a large difference between ARC and HadSST3 SST anomalies prior to 1996: this is not mentioned or discussed in the text. [Bruce Wielicki, USA]	Taken into account. Combined with 2-806
2-811	2	17	36			Figure 2.3. This figure claims to show "global" (title) "co-located" (subtitle) SST from satellite and in-situ data. Is this plot really only using a small subset of satellite data that is "co-located" with the in-situ data? And if so: what time/space matching criteria were used for a match? [Bruce Wielicki, USA]	Taken into account. Combined with 2-806
2-812	2	17	37	17	39	About Figure 2.3: "….(bottom) Global annual average SST anomaly based on different kinds of co-located data: ERI and hull contact sensor (green), bucket (blue), buoy (red), and all (black). Adapted from Kennedy et al., (2011b)". i) Please explain what "all (black)" means, since it is not the average of the previous figures (from a visual analysis of them, it can be verified that in the time interval 1910-1940, it is the lowest curve); ii) the black line must be put in front of the other lines, since there are parts of this black line that are not well represented, for example around the years: 1996,1998, 2000. [Rubén D Piacentini, Argentina]	Rejected. The comment makes no sense. The reviewer seems to have attributed to Fig 2.3 the large part of the caption for Figure 2.2.
2-813	2	17	41	17	41	Should "error" be plural? [Dian Seidel, USA]	Accepted
2-814	2	17	41	18	2	Is all this basic material really needed? [Dian Seidel, USA]	Taken into account. The paragraph has been significantly shortened, partly relegated to the Appendix.
2-815	2	17	41	18	2	Does ocean color play a role in this phenomena? [Larry Thomason, United States of America]	Rejected - not supported by the peer-reviewed published literature.
2-816	2	17	42	18	22	Whole section is 30% too long relative to its importance, mainly too much detail in first chapter. Key point that SST is not a single quantity unless more closely defined can be communicated with less of the physics background, which is unnecessarily detailed for the purpose of this chapter, I think. [Christopher Merchant, UK]	Taken into account. The subsection has been significantly shortened, partly moved to the Appendix.
2-817	2	17	43	17	43	add "surface" before "10-20" [Elizabeth Kent, England]	Taken into account. Combined with 2-818
2-818	2	17	43	17	43	Add "top" before "10-20" and "layers" after "1-2 mm" [Dian Seidel, USA]	Accepted
2-819	2	17	43	17	43	Replace "IR and MW radiometers sense water temperature of the 10–20 μm and 1–2 mm" with "IR and MW radiometers sense the temperature of the upper 10–20 μm and 1–2 mm water layers". [Robert Waterland, United States of America]	Taken into account. Combined with 2-818
2-820	2	17	45	17	45	meters → metres [Peter Burt, UK]	Rejected. U.S. spelling is used.
2-821	2	17	45	17	45	Remove one "the" [Mihai Dima, Romania]	Accepted. Reviewer means "that". See 2-822

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2-822	2	17	45	17	45	one "that" too much in the sentence [Birgit Hassler, USA]	Accepted
2-823	2	17	45			Delete “That” [Michel Boko, Benin]	Taken into account. Combined with 2-822
2-824	2	17	46	17	46	Negligble diurnal stratification is apparent during the day even at the mean oceanic wind speed of 7 m/s, so "only … when surface winds are strong" is perhaps a little misleading. Conversely, under a dead calm , stratification can persist during the night. Lastly, in the morning, it is day, but even under low winds, stratification may not have formed. Suggest "This assumption is usually valid at night, and under moderate or strong surface winds. Otherwise, thermal stratification may form as the sun heats the upper ocean and the SST may exhibit diurnal variability ..." [Christopher Merchant, UK]	Taken into account. The sentence is re-written.
2-825	2	17	48	17	48	temperature varies with the depth and time of day but is only indirectly dependent on them [Elizabeth Kent, England]	Taken into account. Sentence is reworded (and moved to the Appendix).
2-826	2	17	53	17	54	Not all IR radiometers are said to measure skin temperature since they are tuned to drifting buoy observations, although all are sensitive to the radiometric skin temperature of the ocean surface. [Christopher Merchant, UK]	Rejected. The sentence is correct as written, since it referes to radiometers as instruments, not to the radiometer-based SST data sets.
2-827	2	17	55	17	55	Should read "To estimate error variance or to verify uncertainty estimates" [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-828	2	17	55	18	2	Less true for radiometers? Suggest "To estimate error variance or to verify error estimates for SST observations by comparison of different kinds of SST data, data values have IDEALLY to be adjusted for time and depth differences by modelling the skin effect and diurnal variability or for MINIMUM GEOPHYSICAL ERRORS by constraining the comparison to the night-time data only, to minimize the diurnal variability effects. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Taken into account: sentence is re-worded.
2-829	2	17				not also discuss in a qualitative sense what the findings are fom both the AVHRR and ATSR records? Otherwise going through the [Klaas Folkert Boersma, Netherlands]	Rejected, being taken as a suggestion to describe the qualitative findings from AVHRR vs ATSR records: these types of data are described b/c of their role in the development of climate SST data sets (extending to the pre-satellite period), not b/c of climate variability inferences from the standalone satellite SST data sets.
2-830	2	17				lengthy exercise of introducing the AVHRR data set does not make much sense. Also, why is the set of SST anomalies not extended [Klaas Folkert Boersma, Netherlands]	Rejected, see 2-829. (Reviewer's comment got split into three: 2-829, 2-830, 2-831).
2-831	2	17				to 2010/2011? [Klaas Folkert Boersma, Netherlands]	Rejected, see 2-829. (Reviewer's comment got split into three: 2-829, 2-830, 2-831).
2-832	2	17				Section 2.2.2.1.2: In this and subsequent sections it is worth distinguishing carefully between 'direct' measurements of SST made in situ, from 'indirect' retrievals of SST made by satellites. For example, there are no "Passive MW data sets of SST", but there are "SST data sets derived from Passive MW measurements". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-833	2	17				Section 2.2.2.1.2: A few words concerning what is missing from SST analyses might be a good way to finish the section on SST. For example, there has been no systematic study of reconstruction uncertainties in SST leaving uncertainties prior to the early 20th century. Also confidence regarding SST biases post WW2 will remain under question until there are other analyses that can explore the structural uncertainties. The paragraph on page 19 line 24-34 suggests that current estimates will have little impact on longer term trends relative to other uncertainties, but that does not preclude the possibility that it will have a greater bearing on regional changes at a range of timescales, or that long term trends will be unaffected in future estimates. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. While this subsection is not about SST analyses, but about errors in different types of SST measurement and thus cannot accommodate reviewer's suggestions, the discussion of these issues is added to the Appendix.
2-834	2	18	1	18	1	suggest deleting "for time and depth differences" (for same reason as previous comment) [Elizabeth Kent, England]	Rejected: deletion will make the sentence more ambiguous.

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2-835	2	18	4	18	22	The acronym ATSR is defined and used, but the undefined acronym AATSR is also used extensively. This should be corrected to be more consistent. [Jeffrey Taylor, United States of America]	Taken into account. See response to 2-836
2-836	2	18	7	18	7	ATSR and AATSR are used in this section as if they were the same. Suggest that ATSR be used but an explanation that this includes AATSR be added in section 2.2.2.1.3, or the caption to Fig 2.3. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted. Made changes to this effect.
2-837	2	18	7	18	7	Be consistent: AATSR or ATSR? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-836
2-838	2	18	7	18	7	errors are less for "individual ATSR measurements". This may change when aggregated regionally or globally. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Pixel size is specified in the more detailed description given in the Appendix.
2-839	2	18	7	18	7	You need to introduce the acronym AATSR. This is the Advanced Along Track Scanning Radiometer. [Robert Waterland, United States of America]	Taken into account. See response to 2-836
2-840	2	18	12	18	13	it's the constituent measurements that are homogenised not the datasets (in the context of this sentence) [Elizabeth Kent, England]	Accepted. Edits made.
2-841	2	18	12	18	22	Use ARC with Version no. rather than reprocessed. This paragraph needs also discussion of operational results e.g. a Corlett paper on operational data validation. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Accepted, regarding the need to specify the version number. Rejectd, regarding the need to discuss operatinal data sets.
2-842	2	18	13	18	14	This is after Embury et al applied modelling to adjust the satellite data for the thermal skin and diurnal stratification effects. It would be a bad result if the satellite skin and drifter depth agreed to this level. [Christopher Merchant, UK]	Accepted - clarified
2-843	2	18	15	18	15	Note that Kennedy et al. used an earlier reprocessing of the AATSR data, so the numbers aren't directly comparable with those of Embury et al. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted - clarified.
2-844	2	18	15	18	15	Add "temperature measurements from" before "ships, change "warmer" to "higher" and make comparable changes in the next line. [Dian Seidel, USA]	Accepted
2-845	2	18	15	18	16	Kennedt et al were using an earlier AATSR data set known to have larger biases and not adjusted for skin and stratification effects, and so it is really only the difference in bias between ships and drifters that is of physical relevance here. suggest "Using an earlier AATSR data, Kennedy et al. (2011a) found that ships were warmer relative to matched satellite SSTs than drifting buoys, suggesting ships were biased relative to drifting buoys by 0.18 K. They hypothesized ..." [Christopher Merchant, UK]	Accepted (with edits).
2-846	2	18	18	18	18	The 1.8 factor is not obviously connected to the two biases mentioned earlier. If you keep this material, clarify the connection. [Dian Seidel, USA]	Taken into account. Clarified. Combined with 2-845
2-847	2	18	20	18	20	Change 2011b to 2011c [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-848	2	18	22	18	22	Section ends without a punchline. Given the net result is inconclusive, perhaps less detail is justified for this final paragraph of this section. [Christopher Merchant, UK]	Rejected. This paragraph is relevant to the revised figure 2.4.3
2-849	2	18	27	18	27	insert comma after 'necessary' [Peter Burt, UK]	Accepted
2-850	2	18	27	18	28	I would rewrite the sentence as "Globally complete objective analyses of historical SST are created by applying spatial and temporal analysis to such data sets." [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-851	2	18	30	18	30	"boreal autumn"? State the month instead. [George Kiladis, USA]	Accepted
2-852	2	18	33	18	33	suggest replacing "a nominal spatial resolution of" with "are presented on a spatial grid with resolution of" [Elizabeth Kent, England]	Accepted
2-853	2	18	34	18	34	Suggest to replace 'are possible' by 'exist'. [Klaas Folkert Boersma, Netherlands]	Editorial

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2-854	2	18	36	18	36	timeseries → time series [Peter Burt, UK]	Accepted
2-855	2	18	43	19	1	similar issue to Box 2.3 Table 1 [Peter Burt, UK]	Taken into account: table formats are unified throughout the section.
2-856	2	18		18		Table 2.3: it would be helpful to include in the first column one reference associated with these data sets, even if it is in the text. [Alice Grimm, Brazil]	Taken into account: table formats are unified throughout the section and tables of this type are moved to the Appendix.
2-857	2	19	3			Figure 2.4. Needs to be clearer in the figure caption whether the "bucket" temperature correction is used prior to 1940 or not. [Bruce Wielicki, USA]	Accepted - reference to Table 2.3 is added to the caption.
2-858	2	19	3			Figure 2.4 Colors can be difficult to distinguish: thicker color bars in the legend may help with the orange/pink/red. This is true of many of the figures (e.g. Figure 2.7, 2.12, etc): the figure itself is ok: its the legend that needs a thicker color line to see better. [Bruce Wielicki, USA]	Taken into account. The figure has been substantially re-formatted
2-859	2	19	4	19	4	"the mean of the three" [George Kiladis, USA]	Taken into account. Combined with 2-858.
2-860	2	19	4	19	4	Same comment as done for global temperature (#14). The text refers to global SST but the analysis and the figures refer to anomalies, and is not clear with respect to which mean. Caption of Figure 2.4 is not clear, probably because COBE data is shown separately, and this is not mentioned, as well as "top" panel, which can refer to any of 2 upper panels. I suggest using a), b), c) etc. [Celeste Saulo, Argentina]	Taken into account. Combined with 2-858.
2-861	2	19	4	19	7	About Figure 2.4: i) The temperature anomaly must be in the upper part of the figure, since it is the main one; ii) its dimension in the vertical axis must be at least twice its actual size; iii) in the top figure (that I suggest to be included in the bottom), the vertical axis needs to be indicated in a different way, for example, Temperature anomaly offset (°C); iv) for non-specialists in this field, “Anomaly” is a difficult and incomplete expression. It must be indicated as Global mean SST anomaly (or similar expression). [Rubén D Piacentini, Argentina]	Taken into account. Combined with 2-858.
2-862	2	19	4		7	same as page 131 lines 4 to 7 [Jean Poitou, France]	Taken into account. Combined with 2-858.
2-863	2	19	5	19	5	It is difficult to distinguish the ERRST, HadSST3 and HadISST curves due to the similarity in color. Consider different colors, or perhaps labeling the curves in Fig. 2.4. [George Kiladis, USA]	Taken into account. Combined with 2-858.
2-864	2	19	5	19	5	ERSST I presume. Second-lowest panel is I presume differences of ERSST, COBE and HadISST from their mean. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-858.
2-865	2	19	10	19	16	Table 2.4: the last column shows 1979-2010 trends for HadSST3 and COBE with trend differences much larger than the 90% confidence ranges specified: some comment/clarification on this would help. [Bruce Wielicki, USA]	Taken into account. The version of HadSST3 used in FOD ended by 2006, while COBE was available through 2010. Then data sets availability and thus the numbers in this table are different for SOD. A clarifying statement is added.
2-866	2	19	13	19	14	similar issue to Box 2.3 Table 1 [Peter Burt, UK]	Accepted
2-867	2	19	13	19	14	why are these numbers in italics? [Peter Burt, UK]	Rejected: as is explained in the caption, results for incomplete time periods are italicized
2-868	2	19	16	19	16	nineteenth → 19th [Peter Burt, UK]	Accepted
2-869	2	19	16	19	16	"with little or no data available prior to 1800". Which is it? Little data (which should be "few data"!) or no data? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted. Parentheses removed for expediency
2-870	2	19	19	19	19	How does Figure 2.2 show the bias described in the text? [Karen Rosenlof, United States of America]	Rejected. Global mean anomalies computed from co-located data sets of different kinds of SST measurements are shown in the bottom panel; these are offset from each other due to systematic biases between these types of measurements.

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2-871	2	19	22		22	"anomalies seen before 1940 in the top panel…" [George Kiladis, USA]	Accepted
2-872	2	19	24	19	34	The uncertainties in the trend associated with bias uncertainty as estimated in HadSST3 depend on the period considered. At the moment, only the longest periods are discussed. Uncertainty associated with biases is proportionately more important in the most recent period shown in Table 2.4. In Kennedy et al. 2011 bias uncertainty was found to be larger than structural uncertainty, represented by a range of estimates from other centres, over the periods 1960-1999, 1970-1999 and 1980-1999. It was comparable over the period 1940-1999. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph is re-written, partly moved to the Appendix.
2-873	2	19	25	19	25	Change 2011b to 2011c. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-874	2	19	25	19	26	Differences between HadSST2 and HadSST3 also reflect changes in the underlying data base of observations "ICOADS": the differences are not due only to different bias adjustments. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Clarification is added (in the Appendix).
2-875	2	19	28	19	29	The structural uncertainties given by the ensemble of opportunity used include this effect, since the other data sets in the ensemble of opportunity do not include these bias adjustments. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The structural uncertainty clause has been removed.
2-876	2	19	29	19	29	"Trend uncertainty" is ambiguous. Presumably it refers to the standard errors quoted in Table 2.4, which tell us something about the parameters of a particular statistical model. It is therefore not a like for like comparison. Errors in the data mean that the measured trend is not equal to the 'true' trend that would be obtained if the data were perfect and complete and a straight line was drawn through them. In this ideal world, there would still be a large 'trend uncertainty'. It would, at a minimum, be useful to the discussion to indicate the comparable range of trends associated with bias uncertainty to show their magnitude. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-875.
2-877	2	19	29	19	29	"similar to between dataset structural uncertainties" doesn't make sense. I'm guessing the reference is to the difference between the numbers in Table 2.4 giving an estimate of the structural uncertainties. But what is meant here isn't clear to me at all. [Elizabeth Kent, England]	Taken into account. Combined with 2-875.
2-878	2	19	33	19	34	Here, talking about HadSST3, it is said that "error estimates in global and hemispheric monthly means are more than double the estimates from HadSST2". However, looking at table 2.4, is does not seem that errors of HadSST3 are more than double of those of HadSST2. I am sure to have misunderstood the text, nevertheless this could be an indication that something may be not clear in the explaination. [Claudio Cassardo, Italy]	Taken into account. The sentence has been removed from this paragraph.
2-879	2	19	34	19	34	Change 2011a to 2011b. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-880	2	19	34	19	34	Citation should be for Kennedy et al 2011b here [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-881	2	19	36	20	3	Presumably the interpolation techniques have reduced the 1979-2010 trends (Table 2.4) because kriging makes anomalies tend to zero far from observations. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. The interpretation is possible but not definitive. Therefore it is not included into the revised text.
2-882	2	19	36		39	These good comments apply to land data too. [Kevin Trenberth, USA]	Noted. Moved to the Appendix, because these are also explained within a more general context in Box 2.1.
2-883	2	19	37	19	37	I don't understand the use of the word "imputed" here. [Dian Seidel, USA]	Editorial. Replaced by "infilled".
2-884	2	20	1	20	3	"HadISST remains unchanged from AR4" - not quite there has been a few more years added to the dataset (Table 2.4) [Gareth S Jones, UK]	Editorial. Have made clear its methodologically unchanged
2-885	2	20	3	20	3	Consider stating here why it is that only in situ data is used in ERSST3b (this is stated later on page 20, line 43, with reference to the use of ERSST3b in MLOST). [Colin Morice, UK]	Accepted, and later reference removed
2-886	2	20	5	20	22	The Figures that accompany this discussion are somewhat deceptive in that they put the oceanic data on a	Taken into account. The figure 2.5 has been

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						very different vertical scale than the land data. The land data in Figure 1b is plotted with a vertical scale from -3 to +2. But the ocean data are plotted in Figure 2.4d on a vertical scale from -0.8 to +0.6. To the casual reader they look the same, but the similarity is deceptive. Since these are the two key sources for the global average, and people appeal to the similarity between them as evidence of the validity of the magnitude of the surface trend, they should be presented on the same vertical scale, which would make clear how small the warming trends over the oceans are compared to the land record. [Ross McKitrick, Canada]	excluded.
2-887	2	20	8	20	11	This assumes that the AMO and PDO indices are purely influenced by internal climate variability. It is entirely possible this is not the case (B.B.B. Booth et al., Aerosols Implicated as a Prime Driver of 20th century variability within the North Atlantic. Nature,submitted). This sentence seems to be in conflict with the aims of this chapter stated in Page 2-7 L1 to L7 [Gareth S Jones, UK]	Taken into account. The offending text has been excluded together with figure 2.5
2-888	2	20	8	20	13	The attempt to attribute multidecadal features in the global temperature trends to various low-frequency oscillations (AMO, PDO)--a fraught procedure because of fundamental issues in how these indices are defined in the first place---appears completely at odds with the earlier instistance (page 7) that the chapter will not engage in any "attempt to further interpret the observed changes in terms of multidecadal oscillatory variations" [Michael Mann, USA]	Taken into account. Combined with 2-887
2-889	2	20	8	20	15	This discussion of trends in the context of variability is a bit problematic. The chapter seems to be of mixed mind regarding explaining trends in terms of variations and discusing trends IN indices of variability. I'm not sure what to suggest, but, at minimum, please consider connecting this discussion to the discussion in Box 2.4, and possibly moving Box 2.4 closer to the beginning of the chapter to lay a basic foundation for the issue. [Dian Seidel, USA]	Taken into account. Combined with 2-887
2-890	2	20	10		13	These comments lack context. Specifically the numbers for trends should be later. [Kevin Trenberth, USA]	Taken into account. Combined with 2-887
2-891	2	20	11	20	11	Box 2.4 Table 1 should add the warm pool over the tropical western Pacific Ocean. Corresponding to it, the relationships between El Nino and warm pool should be mentioned in some part of this chapter. [Zong-Ci Zhao, China]	Rejected: NINO4 is essentially the warm pool index
2-892	2	20	11	20	15	The pattern in the Pacific is symmetrical around hte equator and can be described better by the IPO (Power et al, 1999, doi:10.1007/s003820050284) or the decadal frequency tail of ENSO than by the PDO. [Geert Jan van Oldenborgh, Netherlands]	Rejected. We consider IPO and PDO to be different indices of essentially the same thing. We are not aware of qualittive distinction between these two phenomena demonstrated in the peer-reviewed literature.
2-893	2	20	13	20	15	These sentences are examples of the importance of the problem of how significance in trends is defined in this chapter. It is clearly used here to signifiy when a change is outside internal variability. But P2-7 L1-L7says this chapter would not be making any statements about changes are outside natural variability. [Gareth S Jones, UK]	Taken into account. Combined with 2-887
2-894	2	20	14	20	15	The PDO pattern has mean zero and hence swings in PDO do not affect the global emnan temperature (eg van Oldenborgh et al, 2012, Clim.Dyn., accepted and many other publications(). [Geert Jan van Oldenborgh, Netherlands]	Noted. This sentence referred to local SST trends, not the trend in the global mean. And it's excluded anyway. (See 2-887).
2-895	2	20	24			This section is very general and no specific regional level descriptions of trends. [Shouraseni Roy, USA]	Noted. The title is Global Combined Surface Temperature. There is not space to go into a large amount of regional detail. But maps are provided.
2-896	2	20	26	20	32	It is the surface temperature anomaly made up of multiple averages of non represebtative samples that has increased. It is wrong to asume that the same may be true of the mean surface temperature,which cannot be measured [VINCENT GRAY, NEW ZEALAND]	Rejected. Through the various boxes and the previous two sections it is stated explicitly what is going into this analysis.
2-897	2	20	26	20	32	The conclusion of this paragraph is not very clearly stated. If the conclusion is that, due to uncertainties in the data, it is not possible to say that the global temperature is increasing, especially since 1950, but that the warming since 1950 is due to change of phase of interdecadal oscillations, it should be said clearly in this way. What does the expression “the world is warming on multi-decadal (greater than 30 years) timescales” mean? If it is warming in the sense of global anthropogenic climate change, then it is simply warming and there is no	Noted. This text has been deleted here.

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						timescale associated to it. If there is a timescale involved (30 years), than there is also cooling after the warming, and this should be said clearly. In summary, if this is the conclusion, I suggest for the last sentence of this paragraph something of the type: “It is concluded with very high confidence that the world has shown particularly marked warming since the mid-twentieth Century and that part of it was caused by changes of phase of multi-decadal oscillations (greater than 30 years timescales).” [Alice Grimm, Brazil]	
2-898	2	20	26			could you please be more specific which three data sets you are referring to? [Shouraseni Roy, USA]	Taken into account. Rather than spell them out we have dropped the specific reference to three datasets in this text.
2-899	2	20	27	20	27	"were concluded" sounds like they were finished in order to be consistent [Elizabeth Kent, England]	Accepted. Sentence has been substantially shortened and modified for clarity.
2-900	2	20	28	20	30	This statement is unsupported and should be removed unless there is strong evidence to support it. The warm 1940s is well established in some regions (eg N Atlantic, Iceland, Greenland) despite the desire of some climate scientists to “remove the 1940s blip”. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Noted. This text has been deleted here.
2-901	2	20	30	20	32	"It is concluded with very high confidence that the world is warming on multi-decadal (greater than 30 years) timescales and that this warming has been particularly marked since the mid-twentieth Century" Again, this statement is not referenced in any way and lacks any basis. The conclusion should follow, not precede the evaluation of the data. This will enhance its credibility. [Philip Lloyd, South Africa]	Noted. We have moved this to the end as noted previously.
2-902	2	20	30			capitalization of 'Century' (or twentieth) [Larry Thomason, United States of America]	Editorial
2-903	2	20	31	20	32	Again, it's fairer to compare 1910-1940 with the recent warming [Marcel Crok, The Netherlands]	Noted. This text has been deleted here.
2-904	2	20	34	20	34	utilised → used [Peter Burt, UK]	Editorial
2-905	2	20	34	20	34	Is "innovations" really the correct term to use here? Perhaps instead "changes" or "improvements". [Karen Rosenlof, United States of America]	Noted. Text has been substantially revised.
2-906	2	20	37	20	38	Again the claim 'less of a 1940s maxima' is not supported by Morice et al. - a more accurate description would be that HADCRUT4 shows a less abrupt decline from the 1940 max than did HADCRUT3. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-909
2-907	2	20	37	20	40	Is the assessment that HadCRUT4 says that 1998 is now the 3rd warmest year robust? HadCRUT4 has realisations, do each give 1998 as third warmest, or is the median (or mean...) of the realisations being used to make the assessment? Either way it may be necessary to make this clear here. [Gareth S Jones, UK]	Taken into account. See response to 2-909
2-908	2	20	38	20	38	insert comma after 'temperatures' [Peter Burt, UK]	Taken into account. See response to 2-909
2-909	2	20	38	20	40	The first statement concerning the differences between HadcRUT3 and HadCRUT4 does not appear to take into account the estimated uncertainties. The size and character of the 1940s maximum varies from one realisation of the data set to another. The ranking of recent years is likewise sensitive to the uncertainties so I would suggest removing these two statements or qualifiying them appropriately. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-910	2	20	39	20	39	Although the warmest years in HadCRUT4 are now in greater agreement with MLOST and NASA GISS, the differences in HadCRUT4 annual anomalies for 1998, 2005 and 2010 are extremely small. The warming of recent years in general in the updated dataset is arguably a more significant result than the shifting of annual rankings. [Colin Morice, UK]	Taken into account. See response to 2-909
2-911	2	20	42	20	42	what is the most rcent decade? Give dates. [Peter Burt, UK]	Noted. Text has been removed to meet length limits
2-912	2	20	42	20	42	"most recent decade warming" is better "warming during the most recent decade". [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-911

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2-913	2	20	46	20	46	not the previous section, Section 2.2.1.2 [Peter Burt, UK]	Noted. Text has been moved to appendix and cross-reference amended accordingly.
2-914	2	20	50	20	51	The reference to GISS coding being put into another language is out of place here. So what? What purpose did that serve or WHY was it a big deal? Short of including additional specificity, it should be removed. [Michael Brewer, United States of America]	Noted. Text has been modified to make clear that this replication adds confidence. Discussion now resides in the appendix.
2-915	2	20	50		51	This is a modestly interesting point though one would assume(!) that issues like language, compiler, CPU type, etc. would not have a significant impact on their analysis since it should be quite stable and I can't imagine that it would have the sensitivities that some more delicate mathematical processes might exhibit. The fact that it is in a different language is really only a curiostiy. [Larry Thomason, United States of America]	Taken into account. See response to 2-914
2-916	2	20		20		Table 2.5: it would be helpful to include in the first column one reference associated with these data sets, even if it is in the text. [Alice Grimm, Brazil]	Noted. Table has been removed to appendix. Citations added.
2-917	2	21	3	21	3	nineteenth → 19th [Peter Burt, UK]	Editorial
2-918	2	21	3	21	4	The dates are too specific to me - or rather give the impression of being exact. Wouldn't it be better to make the statement a bit more fuzzy to reflect the actuallity? e.g. 1910s to the 1940s then 1960s onwards? [Gareth S Jones, UK]	Accepted. Added suitable qualifiers.
2-919	2	21	3	21	9	Mainly anecdotal evidence tends to indicate warming since the late 19th century, but the absence of a method to measure average surface temperfature means that assertions about the supossedly hottest years are not based on representative sampling [VINCENT GRAY, NEW ZEALAND]	Taken into account. See response to 2-909 and 2-922
2-920	2	21	3		9	This aspect could be better documented such as by showing maps of anomalies for the past 4 decades. (Jim Hurrell has a nice set of these) [Kevin Trenberth, USA]	Noted. Space considerations preclude such an expansive representation within the section.
2-921	2	21	4	21	4	– → : (hyphen looks like a minus sign!) [Peter Burt, UK]	Editorial
2-922	2	21	6	21	9	These statements refer to the rankings of and differences between individual years. Such statements are complicated by the presence of observational uncertainty - they should be couched in probabilistic terms. The statement concerning individual decades does not suffer from this problem. I would suggest either removing the final two sentences of the paragraph, or changing them to reflect their uncertain nature. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. This passage ahs been substantively modified to address these concerns.
2-923	2	21	7	21	7	ten → 10 [Peter Burt, UK]	Rejected. This reads very strangely as a number rather than word.
2-924	2	21	8	21	8	insert comma after 1997 [Peter Burt, UK]	Editorial
2-925	2	21	12	21	12	Figure 2.6 seems to be the first Figure (but please verify) to have a text related to the Copyright: "Crown Copyright 2011. Source: Met Office". Hence, this must be normalized in all the figures (to include the copyright of IPCC 2013, in a similar way as for IPCC 2007, or other copyright). [Rubén D Piacentini, Argentina]	Rejected. We will include copyright only if claimed by the figure providers.
2-926	2	21	12	21	16	(Fig. 2.6 caption) See above. In this case "combined land-ocean temperatutre" should be added. [Christian-D. Schoenwiese, Germany]	Accepted
2-927	2	21	12	21	16	I am very pleased to see this plot included, as I think it is a key figure - some clarification is needed in the caption and text as to what, e.g. "2000s" means - I believe it is 2001-2010, but it could equally well be 2000-2009. If it is the latter, then the text is confusing as it talks about 2010. I wonder (although I am not sure i agree with myself!) about including "the 2010's so far" ... either on the figure, or just in the caption, as it is an obvious question as to ask - "what is this decade looking like?" - in this context it really matters whether 2010 is in the 2000s or 2010s. [Keith Shine, UK]	Noted. We make these issues clearer in the redraft.
2-928	2	21	18	21	33	This paragraph deals well with the difference between the long-term warming trend and the interdecadal oscillations. It reinforces my suggestion above for Page 2-20, lines 26-32. [Alice Grimm, Brazil]	Noted.
2-929	2	21	18	21	33	I think this discussion is better placed in Chapter 10 with a brief pointer here as to why the question cannot be	Noted. We cannot avoid discussion of this issue. And

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						answered using the time series of global temperature alone. On their own (leaving aside all question of models, physics etc) the observational records of global temperature are consistent with a wide range of futures. While it is true that the long-term warming in the 20th century has been interrupted by periods of cooling it does not follow that any slow down in the rate of warming is simply an interruption on a long-term upward trend. The question of whether global warming has stopped is a question about the future and therefore relies on far more than the global temperature time series alone hence the need to defer this discussion to a later chapter. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	we already explicitly point the reader to Chapter 10 where a more in-depth discussion has occurred. We have modified this paragraph to be more tone neutral. We have also run it past the Chapter 10 CLAs to ensure that there are no inconsistencies.
2-930	2	21	18	21	33	The dismissive nature of the narrative description of the decadal length flattening of global T since about 1998 is inappropriate, particularly in view of highly publicized questions raised about this topic by some former IPCC authors. Consider, for example, this sentence: "Much interest has focussed on differences in the period since 1998 and the popularly posed question 'has global warming stopped?' based upon HadCRUT3 trends." While several references are provided in defense of the notion that the post-1998 flattening is not significant, no references are given in support of the idea that the flattened trend is significant. Objectivity and the IPCC's requirements and guidelines given above in Comments 1-6 suggest that, no matter the outcome, a decadal scale trend should not be dismissed but be given serious consideration. [Forrest Mims, USA]	Taken into account. See response to 2-929. We have calculated and reported the short term trend here for the three datasets.
2-931	2	21	18		33	Needed discussion, this must be here. [Larry Thomason, United States of America]	Taken into account. See response to 2-929
2-932	2	21	20	21	24	If you must refer to "global warming has stopped" please give a reference. Better, omit this. Simply acknowledge that there does appear to be some slow-down of warming this decade. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-929
2-933	2	21	22	21	22	It's good that you alert the reader once more that time series in nature/climate are auto-correlated. However this should be done in cases where there is a warming trend as well. A high auto-correlation greatly enhances the chance that 9 out of the ten warmest years are all in the same decade. I refer once more to Cohn and Lins 2005 and also the work of Demetris Koutsoyiannis (see as a starter this talk http://itia.ntua.gr/en/docinfo/991/). Koutsoyiannis is a specialist on Long Term Persistence or what he calls Hurst Kolmogorov behaviour. It's a deficiency that none of his work apparently is known to the scientists involved in WG1. [Marcel Crok, The Netherlands]	Taken into account. See response to 2-929
2-934	2	21	23	21	24	Would be appropriate to cite Knight et al as well here [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted. Knight et al is in Peterson et al. and endnote makes it impossible to cite sub-components.
2-935	2	21	24	21	24	After "…Santer et al., 2011)" and before "None …", add "decadal scale extreme cooling and amplied warming may be attrbuted to shifts of atmospehric circualtion pattern that redistribute heat transport in atmosphere and ocean (Zhang et [Xiangdong Zhang, United States of America]	Rejected. The discussion here is on global mean behavior.
2-936	2	21	24	21	24	al., 2008; Overland et al., 2011)". [Xiangdong Zhang, United States of America]	Suspected continuation of 2-935 - see that response
2-937	2	21	26	21	26	This also corroborated by upper ocean observations discussed in Section 3.2 [George Kiladis, USA]	Noted. We have not cross-referenced at this time although may do so in the final drafting round.
2-938	2	21	26	21	28	The sentence implies that trends since 1998 are explicitly treated in Morice et al. [submitted]. The paper describes the construction of the HadCRUT4 dataset but does not refer to warming trends since 1998. The positioning of the citation in the sentence should reflect this. [Colin Morice, UK]	Accepted
2-939	2	21	26	21	33	The end of this paragraph discusses issues of regions missing from analyses. This could be expanded upon. It does seem odd that you've not commented upon the Berkeley claim to have developed a series for land back to 1800, which coverage maps clearly show to basically European data before about 1840. Europe can be shown in the 20th century to have been quite different from the NH average. The NH in the early 1940s was quite warm, but exceptionally cold in Europe. [Philip JONES, UK]	Noted. We have a discussion of coverage aspects to the appendix.
2-940	2	21	28	21	31	The text claims that areal sampling was lower pre c.1945. You should show a chart of the number of stations used in GHCN. It will show that since the 1970s the sample size has collapsed to levels below that in the 1930s, taking it back to a sampling rate comparable to the earliest decades of the 20th century.That is hard to square with the claim in the text. [Ross McKitrick, Canada]	Rejected. The issue is geographical balance rather than pure station count. We only require a few, well spaced, sites to capture the global mean so station count is at best a crude and at worst a misleading

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							indicator in this regard.
2-941	2	21	30	21	30	Typo: many of the well sampled areas has an extra "the". [Marcus Sarofim, USA]	Editorial
2-942	2	21	30			Delete “the” [Michel Boko, Benin]	Editorial
2-943	2	21	31	21	31	delete open bracket? [Elizabeth Kent, England]	Editorial
2-944	2	21	36	21	36	as in comment #14 and #17 [Celeste Saulo, Argentina]	Comment makes no sense in the collated spreadsheet provided to us by TSU.
2-945	2	21	36	21	37	In the caption of Figure 2.7, I think it should be more clear that the upper panel represents the deviation from the 3-set mean. It is [Klaas Folkert Boersma, Netherlands]	Noted. The figure has been replaced and comment no longer pertains.
2-946	2	21				unclear what the lower panel (black line) represents exactly. [Klaas Folkert Boersma, Netherlands]	continuation of 2-946
2-947	2	22	2	22	2	Can you add a percentage of area after "almost the whole globe" and identify the few cooling regions? [Dian Seidel, USA]	Noted. This is already discussed in the text so it is unclear why this is being requested.
2-948	2	22	2	22	2	better to use (Figure 2.8, left panel) [Zhaomin Wang, UK]	Accepted. This necessitatied the addition of a corresponding cite later in the paragraph to the right hand panel for clarity.
2-949	2	22	2	22	4	This is an important correction to AR4 where it was claimed that all the continents has warmed. In New-Zealand there is a lot of discussion going on about the data adjustments. The raw data show little warming as well. With little trends in Australia, New-Zealand, Parts of Africa and Patagonia, this suggests that the Southern Hemisphere has warmed far less than the Northern Hemisphere. Maybe this could be noted in the summary of the chapter. [Marcel Crok, The Netherlands]	Noted. No changes made or requested to this particular section of text.
2-950	2	22	2	22	8	Text and Fig. 2.8 (page 135): The caption of Fig. 2.8 informs that trends have been calculated only for those gridboxes with greater than 70% complete records. I am surprised to find that for entire South America there are at least 70% temperature data for the period 1901-2010 (Fig. 2.8 left) for a 5 degree resolution. Is this true or was another method used which involved interpolation to distances greater than 5 degree? I tried to find a spatial distribution of the stations used to provide observations in different periods of the interval 1901-2010, in the NCDC site, but could not find it quickly. Therefore, I suggest including some information on the land observations used for this figure. Does it not seem strange that for the period 1901-2010 the entire South America displayed warming trend greater than 2 standard errors from zero, while in the period 1979-2010, when data are much more reliable and dense, and when the global temperature trend was much higher (see Fig. 2.7), only half of the continent showed that trend? The same could be said about the entire globe, but I am using South America as example because I know the scarcity of data, especially before 1960. Not including more information on the distribution of the observed data that support this figure would generate many doubts (as mine). [Alice Grimm, Brazil]	Noted. The revised figure now includes all three products and discussion of infilling. The trend significance issue is one of signal to noise and is referenced appropriately in Box 2.2 and references in this section. We also include in the appendix a suitable graphic denoting the change in coverage over time as requested by several additional reviewers.
2-951	2	22	2	22	8	This paragraph looks completely out of place, and it emphasizes the regions that don't show warming? [Philip JONES, UK]	Noted. We believe this pargraph and Figure should be here. Because the text about calling out certain regions was somewhat contentious we have deleted these specific call outs from the text. We have also tried to strengthen the intended core message of the paragraph in light of these concerns.
2-952	2	22	2	22	8	Are these trends robust to the choice of data set? Only the NCDC estimate is used here. To what extent does these trends depend on the choice of data set? Figure 2.5 suggests that differences could be interesting. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. We now include all three data products.
2-953	2	22	2	22	8	little or not warming over many land regions – but this negligible warming is not apparent in Fig 2.8 eg little difference in figure apparent between land and ocean despite different emphasis being given to each in the text, ie calling out little or no warming over many land areas but not over many ocean areas. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-951

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2-954	2	22	2			"Since 1901 almost the whole globe has experienced warming" This is actually misleading, because Figure 2.8 only shows two time periods. Figure 2.7 is much more sensible, in that it shows that, over the period of physical measurements from which estimates of global mean temperature can be derived, there have been periods when the earth has cooled and periods when it has warmed, so illustrating a key point, that the signal incorporates natural cycles as well as any contribution from human activities. A further argument against the use of Fig 2.8 and the quoted misleading conclusion is that atmospheric CO2 only really started increasing at a significant rate post 1950 - which was when fossil fuel use also started to increase rapidly. So most of the warming that took place from 1901 to 1950 must have been largely natural. [Philip Lloyd, South Africa]	Noted. Comment is largely to issues outside the chapter and more relevant to chapter 10.
2-955	2	22	4	22	5	It is more accurate to say that "Of the ocean regions for which there is sufficient observational coverage to assess the trend, only the North Atlantic south of Greenland has not warmed." [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-951
2-956	2	22	5	22	5	It is here necessary to specifically point at the weaknesses in the data density. I therefore suggest adding before "Warming…": "Except for the Pacific and Atlantic oceans, …." [Martin Hovland, Norway]	Rejected. Issues of data density and uncertainty have been covered in depth elsewhere and these regions are in fact well sampled, at least comparatively so.
2-957	2	22	5	22	5	"North Atlantic south of Greenland" could be taken to mean the region bounded by 60N & 0N rather than the smaller area shown to be cooling in the figure [Elizabeth Kent, England]	Taken into account. See response to 2-951
2-958	2	22	6	22	6	For the same reason, exchange the word "globe" with "land surface", and delete "at the surface" [Martin Hovland, Norway]	Rejected. We are talking about land and ocean here.
2-959	2	22	6	22	6	suggest to insert (Figure 2.8, right panel) after "the satellite era". [Zhaomin Wang, UK]	Accepted
2-960	2	22	7	22	8	"The global mean warming rate has been much greater in this recent period than for the record as a whole (Table 2.6)." There is a most egregious omission from Table 2.6, namely the period from 1911 to 1950. Is this something that should not be reported? That over this period the warming was of a similar magnitude to that over the period 1951-2010? Is this something to be hidden? The quoted statement should be corrected and the missing data incorporated in Table 2.6 [Philip Lloyd, South Africa]	Noted. We have included 1901-1950 where appropriate in the trend tables in the redraft.
2-961	2	22	11	22	14	(Fig. 2.8 caption) Again see above. In this case "surface air temperature" should be added. [Christian-D. Schoenwiese, Germany]	Accepted
2-962	2	22	17	22	21	The quoted confidence limits are absurd as they assume that the multiply averaged maximum and minimum measurements are constants without any variability [VINCENT GRAY, NEW ZEALAND]	Rejected. Comment has no supporting literature referenced and is stated as conjecture.
2-963	2	22	17			table 2.6: 2011 data is now available; it should be included. [Stephen Gaalema, USA]	Noted. 2011 was incomplete at time of drafting, we will always include up to the end of the most recent calendar year in each draft.
2-964	2	22	21	22	22	similar issue to Box 2.3 Table 1 [Peter Burt, UK]	Insufficient context for comment to be actionable
2-965	2	22	24	22	24	Is "Upper Air" completely appropriate when lower- and mid-tropospheric trends are discussed? [Dale Hurst, United States of America]	Noted. We are hard pressed to think of a more appropriate title and this section was similarly titled in AR4. No changes made.
2-966	2	22	24	29	10	There is no discussion of the question of temperature trends in the upper troposphere, yet the trend in this region is probably more critical than any other in validating (or otherwise) the general circulation models on which so much of the predictions of future climates depend. The debate on this issue is not to be avoided. If the data do not support the models (and that appears to be the situation), then it must be spelled out, and the sooner that is done, the better. [Philip Lloyd, South Africa]	Rejected. There is discussion of all layers and levels in the current text. Further, it is not the remit of this chapter to assess climate models. This is done in Chapters 9 and 10 where there is extensive discussion on precisely these issues amounting to several pages across these chapters.
2-967	2	22	24			Section 2.2.4 is mis-named ('Upper air'). It includes Lower Troposphere. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-965
2-968	2	22	26	30	9	Though the discussion of the various datasets and their	Noted. Careful consideration needs to be given to the

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						methodologies is very comprehensive I think that more discussion on the lapse rate issue is needed. Temperature lapse rate is an important feedback and so how much the surface is warming relative to the free atmosphere is quite important. So less on absolute warming and more on differences I think would improve this section. [Simon Tett, United Kingdom]	boundaries between this chapter and subsequent chapters where model expectations and comparisons are discussed so there are not wholesale changes made in response. The lapse rate is also only really a meaningful constraint in the tropics. This is also arguably the area where our knowledge of the observations has evolved most substantially between the assessments and so some discussion of this is added in the closing segment.
2-969	2	22	28	22	28	Thorne, 2011b should be Thorne et al., 2011a? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. Text has now been deleted.
2-970	2	22	29	22	30	The assessment that “trends of upper air temperature in the tropics is uncertain, although even this region is concluded to be warming” is supported by indirect observations of englacial temperature profiles [Gilbert et al., 2010] and regional climate modelling [Vuille et al., 2008]. Both studies agree each other and Gilbert et al. [2010] show two warming phases for atmopsheric temperature from 1900 to 1960 (+0.5 ± 0.3 K starting approximately in 1920-1930) and from 1985 to 1999 (+0.6 ± 0.2 K), corresponding to a mean atmospheric temperature rise of 1.1 ± 0.2 K over the 20th century, in Bolivia (16°S), at 6340 m asl. Ref : Vuille M., B. Francou, P. Wagnon, I. Juen, G. Kaser, B. Mark, R. Bradley, Climate change and tropical Andean glaciers - past, present and future, Earth Science Review, 89, 79-96, 2008. Gilbert, A. P.Wagnon, C. Vincent, P. Ginot and M. Funk, 20th century temperature reconstitution in a high altitude tropical site from Illimani (6340 m a.s.l., Bolivia 16°39’S) englacial temperature, J. Geophys. Res., 115, D10109, doi:10.1029/2009JD012961, 2010 [Patrick Wagnon, France]	Noted. This material should be discussed in the relevant palaeo and modelling chapters but is out of scope for this chapter.
2-971	2	22	31	22	34	How can something be concluded with very high confidence if there is a substantial degree of uncertainty in the observations? [Uwe Stoeber, Germany]	Noted. The text has been deleted for other reasons relating to a change in stylistics so this comment no longer pertains.
2-972	2	22	31	22	38	New advances … trends: This reads more as an executive summary than reporting the evidence on which to base - an executive summary. Give reference and explanation to support the claims. [Elisa Manzini, Germany]	Taken into account. See response to 2-971
2-973	2	22	33	22	33	I suggest adding the following before "it is…": "Despite a quasi-global grid density of radiosonde data," [Martin Hovland, Norway]	Taken into account. See response to 2-971
2-974	2	22	33	22	38	"It is concluded with very high confidence that the troposphere has been warming and the stratosphere cooling since the mid-twentiethCentury." Again, there are conclusions with no substantiation. Conclusions should not lead sections. This is supposed to be an assessment, not a polemic. If you lead with the conclusions, there is the inescapable perception that the issue has been prejudged. [Philip Lloyd, South Africa]	Taken into account. See response to 2-971
2-975	2	22	36	22	37	"Estimates…"- should specify what region you are referring to. Not necessarily true everywhere. [Melissa Free, USA]	Taken into account. See response to 2-971
2-976	2	22	40	22	49	A sentence should be considered to be added to the last parag on this page to mention the new IASI and AIRS infrared temperature measurement series which although is only valid for clear sky sampling over ocean potentially can give good vertical profile information. [Roger Saunders, United Kingdom]	Noted. Space constraints preclude actioning this recommendation which would potentially open the flood gates to other such insertions at other points in the chapter and report.
2-977	2	22	40	22	50	There should be mention of one of the major issues in MSU/SSU temperatures, that is problems in combining data from multiple satellites and dealing with corrections for drifts with each individual satellite. [Karen Rosenlof, United States of America]	Noted. These aspects are discussed specifically later on and in some detail in the appendix. So no changes made here.
2-978	2	22	41	22	41	on → at [Peter Burt, UK]	Editorial
2-979	2	22	41	22	41	I suggest adding the following before "Satellites…": "These exactly measured data are invaluable for calibration of remotely sensed (satellite) data." [Martin Hovland, Norway]	Rejected. The radiosondes are far from exactly measured data as is discussed later on in this section.

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2-980	2	22	41	22	41	What is a "distinct level"? [Elizabeth Kent, England]	Accepted. Changed distinct to pressure to reduce apparent ambiguity.
2-981	2	22	44	22	44	What is "bulk atmospheric temperature"? [Elizabeth Kent, England]	Accepted. We now clarify in the text.
2-982	2	22	48	22	49	In contrast to the MSU, the SSU is just named but not discussed at all. What is the reason for this different treatment? [Uwe Stoeber, Germany]	Noted. The discussion of MSU is to elucidate on the issue that the "tropospheric" channel is not truly tropospheric and attempt to outline two approaches that have been taken - germane to the figure being referenced. No such discussion is warranted for SSU for which the same issue does not exist.
2-983	2	22	49	22	49	Add "et al." after "Seidel". Also p 2-27 line 3. [Dian Seidel, USA]	Editorial. There are many cases where the citation software and citations need fixing.
2-984	2	22				Table 2.6 Misleading comparison of short and long trends again. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Noted. Choice of periods is to match AR4.
2-985	2	23	1	23	1	Figure 2.9 is very nice and clear, as well as Fig. 2.6 [Celeste Saulo, Argentina]	Noted. Thanks.
2-986	2	23	1	29	7	These sections contain much too much technical detail about the methods of construction of the newer datasets, and it is presented in a way that will almost certainly be incomprehensible to most readers. [Melissa Free, USA]	Noted. We have attempted to rationalize here for SOD by moving much of the technical detail to a chapter appendix.
2-987	2	23	6	23	7	– → : (hyphen looks like a minus sign!) [Peter Burt, UK]	Editorial
2-988	2	23	6	23	8	Replace "For AR4 only two published radiosonde temperature estimates that had assessed homogeneity issues existed – RATPAC (Free et al., 2005) and HadAT (Thorne et al., 2005b). Three additional estimates now exist using novel and distinct approaches (Table 2.7) in addition to a systematic effort to understand uncertainty in the" with "At the time of AR4 there were only two published radiosonde temperature estimates that included treatment of homogeneity issues – RATPAC (Free et al., 2005) and HadAT (Thorne et al., 2005b). Three additional estimates have appeared since AR4; these estimates are based on novel and distinct approaches (Table 2.7). In addition, a systematic effort has been made to understand uncertainty in the". [Robert Waterland, United States of America]	Noted. Changes to separate technical details into an annex means this is no longer applicable.
2-989	2	23	6	23	36	The paper Christy, J.R., B. Herman, R. Pielke, Sr., P. Klotzbach, R.T. McNider, J.J. Hnilo, R.W. Spencer, T. Chase and D. Douglass, 2010: What do observational datasets say about modeled tropospheric temperature trends since 1979? Remote Sensing, 2(9), 2148-2169 should also be mentioned here, although they deal with the tropics and compare with models. This paper gives a detailed comparison of all the datasets and their problems [Marcel Crok, The Netherlands]	Rejected. This paper adds no extra information to those already cited to this discussion. We have considered this paper and as per guidance not cited papers that are reundant. The paper is appliocable to and discussed in other chapters where it contains information that is not similarly redundant.
2-990	2	23	6	23	36	Should also note that one issue with the assorted radiosonde corrections is that they are only on standard levels….they don't easily help with correcting things like tropopause or cold point temperature. [Karen Rosenlof, United States of America]	Noted. This has already been stated in the section that directly precedes it where such discussion around the figure defining the various measures and their nature is made.
2-991	2	23	6		44	Hard to read. [Kevin Trenberth, USA]	Noted. Attempts have been made to make this more readable as well as concise in response to reviewer comments and TSU guidance. Much of this material has been moved to the appendix.
2-992	2	23	10	23	10	Insert 'the' after 'at' [Peter Burt, UK]	Editorial
2-993	2	23	10	23	13	Note any issues of circularity that arise in relation to using ERA to identify breakpoints and adjustments, in relation to the data that have been assimilated into ERA-40 (radiosondes, homogenised or not?) and the external forcings that have been applied to ERA in the form of SSTs and GHGs, which give a warming signal which might then bias the homogenisation towards producing a warming trend. I'm not an expert here, so this	Noted. This is indeed addressed in the papers at some length. As the guidance has been to reduce this section we cannot add such details in depth. We have to trust that the interested reader will look to the cited

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						may all have been addressed in the cited papers. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	papers for additional information above and beyond what we can give here.
2-994	2	23	11	23	11	The term "breakpoints" is introduced here without a definition. [Jeffrey Taylor, United States of America]	Taken into account. This text has been deleted.
2-995	2	23	11			“breakpoints” and “break”; What are they? It seems more than discontinuity. Therefore I don't understand part of table 2.7. [Francois DANIS, France]	Accepted. We have deleted the in-text occurrence and modified Table 2.7 column header and moved all this technical detail to the appendix.
2-996	2	23	16	23	16	issues likely remained'. What issues are meant here? [Klaas Folkert Boersma, Netherlands]	Taken into account. Text has been clarified here.
2-997	2	23	17	23	18	Change to: “Recourse to metadata and the analog cases used in the HadAT work increased confidence in the product (Titchner et al., 2009).” because Sherwood et al did not use metadata. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. Sentence has been deleted as per guidance on reducing this section.
2-998	2	23	20	23	20	What is a "fundamental uncertainty"? [Elizabeth Kent, England]	Taken into account. Sentence has been removed following other feedback.
2-999	2	23	26	23	26	The largest impact on the trend or on the uncertainties? [Klaas Folkert Boersma, Netherlands]	Noted. Sentence has been deleted as per guidance on reducing this section.
2-1000	2	23	26	23	26	Replace "was" with "resulted from". [Robert Waterland, United States of America]	Noted. Sentence has been deleted as per guidance on reducing this section.
2-1001	2	23	29	23	29	Table 2.7 explains RICH-obs and RICH-tau, but in the text you are using RICH. Since this data set will be used for SOD, it should be stated clearly which RICH data set will be used and keep the name consistently throughout the report [Celeste Saulo, Argentina]	Taken into account. The RICH-obs and RICH-tau innovations arrived within a week of FOD submission. They will now be more properly integrated into the new revised version. Table has now been migrated to the appendix.
2-1002	2	23	32	23	32	exchange "that" with a "than"? [Birgit Hassler, USA]	Editorial
2-1003	2	23	32	23	32	I suggest adding the following before "globally…": "Despite having a very low grid density, " [Martin Hovland, Norway]	Rejected. This would require then substantial additional details and citations to explain aspects such as correlation scales. It has also been discussed in previous assessments which we have been directed to build upon rather than replicate.
2-1004	2	23	32	23	32	This should be more specific as in "more tropospheric warming and less stratospheric cooling than" if that is what is meant. [George Kiladis, USA]	Accepted
2-1005	2	23	32			"less cooling thatn existing products" (spelling) [Philip Lloyd, South Africa]	Editorial
2-1006	2	23	33	23	34	"Substantial uncertainty" seems rather vague for a report of this type. [Melissa Free, USA]	Accepted. Removed the qualitative qualifier.
2-1007	2	23	35	23	35	"better sampled" is better than "well sampled". I'm sure we could do with more data even here, especially in the early period. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1008	2	23	36	23	36	For consistency, it would be good to also present a comparison of the trends that sondes report in the lowest 10m with the trends [Klaas Folkert Boersma, Netherlands]	Comment is curtailed and not understandable. Might be combined with 2-1017 which would be a case of the comment not being in scope of this particular section.
2-1009	2	23	39	23	49	It would be useful if this Table incldued the papers for each dataset, instead of having to find them in the text. [Philip JONES, UK]	Noted.Such references have been added. The table has been moved to the appendix.
2-1010	2	23	40	23	40	I think you mean parametric uncertainty. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure has been removed in redrafting.
2-1011	2	23	47	24	1	Table 2.7: Does not help the average reader much--the descriptions are pretty inscrutable. The third sentence	Noted. The table is now in the appendix which should

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						in the caption is unnecessary. [Melissa Free, USA]	assuage the reviewer's concerns.
2-1012	2	23	47	24	1	Table 2.7: can you give references in the table for each dataset product? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1009
2-1013	2	23	49	23	49	Please give references in table 2.7. [Karen Rosenlof, United States of America]	Taken into account. See response to 2-1009
2-1014	2	23	49	24	1	If this type of detail is kept in later drafts (which I don't think necessary) the description of RATPAC under "breakpoint test' should read as follows: "Multiple indicators…until 1996, first difference method using metadata after 1995", and that under "Adjustments method" [sic] should say "Manually based adjustments using individual time series prior to 1996; first difference method after 1995". There is no t-test involved in the first difference method. [Melissa Free, USA]	Noted. This table has been moved to the appendix. Changes have been made to this end.
2-1015	2	23	49	24	1	Table 2-7: "Individual sounding" is more likely to be understood by the average reader than "individual launch". [Melissa Free, USA]	Taken into account. See response to 2-1014
2-1016	2	23		23		Table 2.7: it would be helpful to include in the first column one reference associated with these data sets, even if it is in the text. [Alice Grimm, Brazil]	Taken into account. See response to 2-1009
2-1017	2	23				observed from ground instrumentation.But I don't know if such a consistency check has been done. [Klaas Folkert Boersma, Netherlands]	See response to 2-1008
2-1018	2	24	1	24	1	In Table 2.7, fourth column and first row, replace "Derived hierarchically looking 1. for breaks..." with "Derived hierarchically looking for 1. breaks..." [Celeste Saulo, Argentina]	Taken into account. See response to 2-1014
2-1019	2	24	1	24	1	Is "SNHT" defined somewhere? [Dian Seidel, USA]	Taken into account. See response to 2-1014
2-1020	2	24	3	25	2	Section 2.2.4.2 is too long and has too much detail, some of which is not new since AR4. [Dian Seidel, USA]	Noted. Much of the material is now in the appendix.
2-1021	2	24	5	24	24	I am surprised no mention is made of the new techniques being implemented to use the satellite radiances themselves to monitor changes in temperature (or water vapour) and simulate them from a model. That gets round the problem of channels changing with each new sensor. [Roger Saunders, United Kingdom]	Noted. Once these are mature and have been applied to climate studies they will be summarized but to our knowledge they have not been to date so we cannot include them at this juncture.
2-1022	2	24	6	24	6	I understand the usefulness of deepening the dataset VG2 as not updated. However, even only for sake of comparison, it could be useful to summarize its characteristics in the table 2.8. [Claudio Cassardo, Italy]	Noted. We have been guided to reduce this section rather than expand so we do not do this.
2-1023	2	24	7	24	7	Note that STAR is the name of a sub-component of NOAA (Center for Satellite Applications and Research), just like NCDC, not just a random label. [Dian Seidel, USA]	Noted, equally RSS and UAH are 'departmental' labels so no changes made.
2-1024	2	24	8	24	11	The UAH method is said to have remained essentially unaltered, however two modifications have been performed. Instead of say in this way, I suggest to rephrase the sentence in: "In the UAH dataset it was removed an apparent seasonal cycle artefact in the latter part of their record related to the introduction of AMSU in version 5.3 and changed the climatological baseline to 1981–2010 to produce version 5.4. Both changes had negligible impact on trend estimates.". [Claudio Cassardo, Italy]	Taken into account. This wording has been changed to remove redundancy along the lines suggested.
2-1025	2	24	14	23	14	Mears (2011) should be Mears et al. (2011). This seems to occur with many papers in this section. Christy (2010, 2011) should be Christy et al. (2010, 2011). [Philip JONES, UK]	Editorial. This is the vageries of endnote and presumably will be fixed in final copy editing. Manual changes get redacted on document save.
2-1026	2	24	14	24	14	Mears, 2011 should be Mears et al., 2011? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1025
2-1027	2	24	14	24	14	Change "isothermal" to "invariant". Temperatures, not trends, can be desribed as isothermal. [Dian Seidel, USA]	Accepted
2-1028	2	24	21	23	21	Monte-Carlo → Monte Carlo [Peter Burt, UK]	Editorial
2-1029	2	24	22			Are there rivers named Columbia and Paraguay? (It may be the text is talkin about Magdalena river and La Plata river) [José Daniel Pabón-Caicedo, Colombia]	This comment appears totally mis-appropriated. Perhaps it was meant for Chapter 12?

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2-1030	2	24	23	24	23	This 0.1 K/decade uncertainty is large, but the sentence doesn't put the number in context so the large uncertainty might be lost on the reader. [Dian Seidel, USA]	Noted. This should be clear from the later trend figures and table.
2-1031	2	24	26	24	26	define STAR [Peter Burt, UK]	Rejected - this is a departmental acronym with no meaning that elucidates the origin or value to the reader.
2-1032	2	24	26	24	39	The polar SNO's used by STAR only sample a narrow range of scene temperatures. Recent analysis by John et al (2012) have shown that the bias between satellites varies strongly with scene temperatures and so polar SNOs only give a zeroth order correction. Ref: John, V. O., G. Holl, S. A. Buehler, B. Candy, R. W. Saunders, and D. E. Parker (2012), Understanding intersatellite biases of microwave humidity sounders using global simultaneous nadir overpasses, J. Geophys. Res., 117, DXXXXX, doi:10.1029/2011JD016349 [Roger Saunders, United Kingdom]	Noted. Given that this analysis is for a different variable and sensing channel set it is unclear to what extent we should incorporate this comment here. We did add some discussion to the appendix to this end.
2-1033	2	24	29	24	29	...this is orbital..' → .'.. This is dependent on orbital..' [Peter Burt, UK]	Rejected. Suggested change lengthens the text and is little if any clearer than current wording.
2-1034	2	24	31	24	31	Simultaneous Nadir Overpass → simualtaneous nadir overpass [Peter Burt, UK]	Rejected. The capitalization is deliberate as it spells out the acronym that follows in the brackets. This discussion has been moved to the appendix.
2-1035	2	24				2.2.4.2 The STAR effort has reprocessed the basic MSU data in a way that should be used by all other groups. This is the GSICS effort internationally. The STAR processing is incomplete in that they do not fully deal with inhomogeneities in the record fully (diurnal cycle drift etc), The other structural choices of other groups should be applied to the reprocessed dataset. This is one place where a more complete assessment is needed. In addition, the ERA-interim reprocessing was amazingly able to reproduce the hot point temperature calibration independently from their assimilation system and strongly suggests that ERA-interim should be included in the tropsopheric temperature trend analysis. See Dee, D., and S. Uppala, 2009: Variational bias correction of satellite radiance data in the ERA-Interim reanalysis. Quart. J. Roy. Meteor. Soc., 138, 1830-1841. But I would NOT include any other reanalysis. [Kevin Trenberth, USA]	Rejected. Much of this comment moves beyond what the author team feels to be chapter bounds in terms of advocacy. Long discussions amongst the author team and the balance of comments elsewhere led to the dropping of the reanalyses here.
2-1036	2	25	1	25	1	insert 'do' after 'RSS' [Peter Burt, UK]	Editorial
2-1037	2	25	1	25	2	and paragraph on previous page. Is there an assessmenton which MSU record is better? [Karen Rosenlof, United States of America]	Noted. There is too much ambiguity to make a definitive assessment in this regard and such an assessment to be reached here would be down to solely qualitative author judgement - a route we are reticent to follow.
2-1038	2	25	2	25	2	suggest replacing "remaining" with "other" [Elizabeth Kent, England]	Editorial
2-1039	2	25	5	25	5	timeseries → time series [Peter Burt, UK]	Editorial
2-1040	2	25	7	25	8	similar issue to Box 2.3 Table 1 [Peter Burt, UK]	Comment is insufficiently detailed to be actionable.
2-1041	2	25	10	25	12	Change this section heading to "Intercomparisons of Various Long-Term Products". Replace "between" with "of" in line 12. [Robert Waterland, United States of America]	Noted. Title has been redrafted and the fact that it is between radiosonde and MSU has been noted.
2-1042	2	25	10	26	18	This section attempts to explain the scope of the problems involved in the cross assessment of various radiosonde records, but it seems that what is provided is too cursory to judge the results. On the other hand a fuller treatment would probably contain too much detail, especially given the final statement that (apparently) says that the results are inconclusive. Consider a shorter summary. The more streamlined approach taken in Section 3.4.1.2.2 of AR4 seems more appropriate, where uncertainties are noted within the description of the trends themselves. [George Kiladis, USA]	Noted. The section has been substantially redrafted and this comment ahs been considered in so doing.
2-1043	2	25	12	26	18	Stratospheric comparisons have also been done, e.g., Randel , W.J., K.P. Shine, J. Austin, J.Barnett, C. Claud, N.P. Gillett., P. Keckhut, U. Langematz, R. Lin, C. Long, C. Mears, A. Miller, J. Nash, D.J. Seidel,	Noted. While this is true there is little controversy over stratospheric temperatures at least outside the

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						D.W.J. Thompson, F. Wu and S. Yoden, 2009: An update of observed stratospheric temperature trends. J.Geophys. Res., 114, D02107, doi:10.1029/2008JD010421 [Melissa Free, USA]	immediate expert community so it is not included given length restrictions and potentail for causing reader confusion.
2-1044	2	25	12	26	18	Again, a lot of detail without a coherent bottom line. [Melissa Free, USA]	Noted. We have attempted to shorten and pull out the common thread more succinctly.
2-1045	2	25	13	25	13	is → are [Peter Burt, UK]	Editorial
2-1046	2	25	16	25	17	Sentence structure is awkward. [Karen Rosenlof, United States of America]	Editorial
2-1047	2	25	22	25	23	Replace "Several studies compared UAH and RSS products to raw / homogenized radiosonde station level data locally or regionally." to "Several studies compared UAH and RSS products to local and regional raw /homogenized radiosonde data." [Robert Waterland, United States of America]	Accepted
2-1048	2	25	22	25	36	Too much criticism of RSS here – almost all from Christy, associated with the rival product UAH. No need to make so much of this. UAH and RSS as in pretty good agreement [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1049
2-1049	2	25	22		25	Should comment that VIZ sondes are not a sound basis for this. Also there is a new paper that shows that UAH has incorrect trends that must be included: Po-Chedley and Fu J. Atmos. Ocean. Tech: in press A bias in the mid-tropospheric channel warm target factor on the NOAA-9 microwave sounding unit [Kevin Trenberth, USA]	Accepted. This and a similar paper by Mears and colleagues will be incorporated in the next draft and the paragraph substantially reworked.
2-1050	2	25	25	25	25	LT? This section is very heavy with acronyms which all need to be defined. [Elizabeth Kent, England]	Noted. The acronyms are defined in the weightings figure that precedes this section. No changes made.
2-1051	2	25	25	25	25	"LT"? I presume this an acronym for "Lower Tropospheric temperatures". [Robert Waterland, United States of America]	Taken into account. See response to 2-1050
2-1052	2	25	27	25	28	Replace "The transition from NOAA-11 to NOAA-12 (early 1990s) was identified as the primary period when the different comparisons consistently pointed towards an issue in RSS." with "The time of transition from NOAA-11 to NOAA-12 (early 1990s) was identified as the primary period when comparison of satellite and radiosonde data indicated an issue in RSS." [Robert Waterland, United States of America]	Noted. The sentence has been revised and shortened in redrafting.
2-1053	2	25	28	25	29	Replace "Christy et al. (2007) noted that this coincided with Pinatubo and" with "Christy et al. (2007) noted that this period coincided with the Mount Pinatubo eruption and,". [Robert Waterland, United States of America]	Taken into account. See response to 2-1052
2-1054	2	25	34	25	34	..spurious. But these..' → '..spurious, but these..' [Peter Burt, UK]	Editorial
2-1055	2	25		25		Table 2.8: it would be helpful to include in the first column one reference associated with these data sets, even if it is in the text. [Alice Grimm, Brazil]	Noted. Table has been moved to appendix where efforts will be made to include a reference to this end.
2-1056	2	25				2.2.4.3 Where is discussion of reanalyses, specifically ERA- interim (only one good enough)? [Kevin Trenberth, USA]	Noted. To date there is no paper to our knowledge that includes more than a cursory intercomparison of reanalyses to the upper air prodiucts so it is out of scope for this particular section. If there is no literature to assess it makes it hard to include.
2-1057	2	26	4	26	18	It may be worth noting that McKitrick McIntyre and Herman (2010) (see ref. in cell 8) tested for differences in trends over 1979-2009 in the tropics between the average MSU products and the average radiosonde products and concluded the trend differences between these products are not statistically significant. [Ross McKitrick, Canada]	Accepted. We will cite this paper appropriately in this context. The model intercomparison aspects are out of chapter remit and should be covered by Chapter 10.
2-1058	2	26	4	26	18	What is the bottom line here? [Karen Rosenlof, United States of America]	Taken into account. See response to 2-1044
2-1059	2	26	11	26	14	Christy 2010 should be Christy et al. 2010 in Journal of Remote Sensing? Listed citation is incomplete. The 2011 reference Christy Spencer and Norris is also incomplete. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial. There are many cases where the citation software and citations need fixing.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1060	2	26	16	26	18	"Whilst these products" sentence is a rather tortured construction that seems to duck the task of assessment. It may not be possible to conclude something definitively but this doesn't mean it isn't possible to assess whether a statement that some products are implausible (which ?) is a statement in which we can have high or low confidence. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-1044
2-1061	2	26	17	26	18	"conclude so definitively" is confusing [Elizabeth Kent, England]	Noted. This text has been removed as part of the section redrafting.
2-1062	2	26	20	26	20	A transitional sentence is needed to start this section. [Dian Seidel, USA]	Noted. Space constraints make such additional verbiage difficult to justify. Most of the technical detail in this section now resides in the appendix.
2-1063	2	26	20	26	20	Add something before "Datasets". Maybe "MSU"? [Dian Seidel, USA]	Title has been removed as the section has now been combined with others.
2-1064	2	26	20	26	31	GPS-RO should mentioned at the beginning of section 2.2.4 when other measurement techniques are introduced. [Jeffrey Taylor, United States of America]	Noted. Change in opening section style makes this redundant so not applied.
2-1065	2	26	22	26	31	Add an explanation which time period the GPS time series spans [Birgit Hassler, USA]	Taken into account. Section redraft should address this comment.
2-1066	2	26	22	26	31	Suggest to replace the current text by the following modified text that more clearly explains the GPS RO method: Global Positioning System (GPS) radio occultation (RO) now represents a mature remote sensing technique (Anthes et al., 2008; Anthes, 2011) and provides highly stable atmospheric observations. It is based on GPS radio signals which are bent and retarded by the atmospheric refractivity field, related mainly to pressure and temperature, during their propagation to a GPS receiver on a Low Earth Orbit (LEO) satellite. An occultation event occurs whenever a GPS satellite sets (or rises from) behind the horizon and its signals are occulted by the Earth's limb. The fundamental measurement is the signal phase which is based on precise timing with atomic clocks. Potential clock errors of GPS or LEO satellites are removed by differencing methods using an additional GPS satellite as reference and by relating the measurement to even more stable oscillators on the ground. Thus, GPS RO is anchored to the international time standard and currently the only self-calibrated raw satellite measurement with SI traceability, in principle (Leroy et al., 2006; Baringer et al., 2010). [The additional references cited here are: Anthes, R. A. (2011), Exploring Earth's atmosphere with radio occultation: contributions to weather, climate and space weather, Atmos. Meas. Tech., 4, 1077–1103, doi:10.5194/amt-4-1077-2011. Leroy, S. S., J. A. Dykema, and J. G. Anderson (2006), Climate benchmarking using GNSS occultation, in Atmosphere and Climate: Studies by Occultation Methods, edited by U. Foelsche, G. Kirchengast, and A. Steiner, pp. 287–301, Springer-Verlag Berlin Heidelberg, doi:10.1007/3-540-34121-8_24.] [Gottfried Kirchengast, Austria]	Noted. Much of the text has been moved to the appendix. Efforts have been made to consolidate this suggestion with the pre-existing text and partition between the two segments appropriately.
2-1067	2	26	26	25	26	low-Earth orbiting → low Earth-orbiting [Peter Burt, UK]	Noted. Text has been modified in response to other reviews.
2-1068	2	26	30	26	32	This sentence seems to contradict what precedes it, which has long been a bit of a problem in discussions of GPS RO data. Are they the reference-quality datasets that SI-traceabilty would suggest, or does processing the time delay data to obtain temperature and humidity profiles add unquantified uncertainty? [Dian Seidel, USA]	Noted. Accounted for in redrafting this section.
2-1069	2	26	33	26	48	Suggest to replace the current text by the following modified text that more clearly explains the GPS RO method: GPS RO measurements have unique attributes that make them well suited for climate studies: (i) long-term stability and reproducibility, (ii) high precision and no need for inter-satellite calibration (Hajj et al., 2004; Schreiner et al., 2007; Foelsche et al., 2009; 2011), (iii) insensitivity to clouds and precipitation, (iv) low structural uncertainty (Ho et al., 2009d), and (v) good error characterization (Kuo et al., 2004; Scherllin-Pirscher et al., 2011a;b). GPS RO observations are used to retrieve atmospheric temperature profiles with high accuracy and vertical resolution (~ 0.5 km to ~ 1.5 km) in the upper troposphere and lower stratosphere (UT/LS) (Kursinski et al., 1997). With their quality unaffected by the surrounding environment (e.g., geo-location, day and night, etc.), these data have been used to identify systematic temperature biases for different radiosonde sensors (Kuo et al., 2005; He et al., 2009; Baringer et al., 2010; Sun et al., 2010). Recently, Ho et	Taken into account. See response to 2-1066

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						al. (2007; 2009b; 2009c) used RO temperature profiles from 2001 to 2010 to simulate the AMSU LS temperature and used these data to calibrate AMSU data. [Insert here the clarified text replacing current sentences in chapter 2, page 26, line 44 to 48; see our related comment on chapter 2, page 26, line 44 to 48] The horizontal resolution of ~300 km allows for provision of gridded atmospheric climatologies (Foelsche et al., 2008) appropriate for weather characterization at synoptic scales and for climate applications. First demonstration studies (Steiner et al., 2009; Schmidt et al., 2008; 2010) and an optimal fingerprinting study (Lackner et al., 2011) confirmed the utility of RO for climate monitoring and change detection as reviewed by Steiner et al. (2011). The performance of the RO record underpins its capability to become a climate benchmark record in a future global climate observing system with the potential to overcome problems of conventional observations. [The additional references cited here are: Foelsche, U., M. Borsche, A. K. Steiner, A. Gobiet, B. Pirscher, G. Kirchengast, J. Wickert, and T. Schmidt (2008), Observing upper troposphere–lower stratosphere climate with radio occultation data from the CHAMP satellite, Clim. Dyn., 31, 49–65, doi:10.1007/s00382-007-0337-7. Foelsche, U., B. Scherllin-Pirscher, F. Ladstädter, A. K. Steiner, and G. Kirchengast (2011), Refractivity and temperature climate records from multiple radio occultation satellites consistent within 0.05%, Atmos. Meas. Tech., 4, 2007–2018, doi:10.5194/amt-4-2007-2011. Kursinski, E. R., G. A. Hajj, K. R. Hardy, J. T. Schofield, and R. Linfield (1997), Observing the Earth's atmosphere with radio occultation measurements using the Global Positioning System, J. Geophys. Res., 102, 23,429–23,465. Lackner, B. C., A. K. Steiner, G. C. Hegerl, and G. Kirchengast (2011), Atmospheric climate change detection by radio occultation data using a fingerprinting method, J. Clim., 24, 5275–5291, doi:10.1175/2011JCLI3966.1. Scherllin-Pirscher, B., A. K. Steiner, G. Kirchengast, Y.-H. Kuo, and U. Foelsche (2011a), Empirical analysis and modeling of errors of atmospheric profiles from GPS radio occultation, Atmos. Meas. Tech., 4, 1875–1890, doi:10.5194/amt-4-1875-2011. Scherllin-Pirscher, B., G. Kirchengast, A. K. Steiner, Y.-H. Kuo, and U. Foelsche (2011b), Quantifying uncertainty in climatological fields from GPS radio occultation: an empirical-analytical error model, Atmos. Meas. Tech., 4, 2019–2034, doi:10.5194/amt-4-2019-2011. Schmidt, T., J. Wickert, G. Beyerle, and S. Heise (2008), Global tropopause height trends estimated from GPS radio occultation data, Geophys. Res. Lett., 35, L11806, doi:10.1029/2008GL034012. Schmidt, T., J. Wickert, and A. Haser (2010), Variability of the upper troposphere and lower stratosphere observed with GPS radio occultation bending angles and temperatures, Adv. Space Res., 46, 150–161, doi:10.1016/j.asr.2010.01.021. Schreiner, W., C. Rocken, S. Sokolovskiy, S. Syndergaard, and D. Hunt (2007), Estimates of the precision of GPS radio occultations from the COSMIC/FORMOSAT-3 mission, Geophys. Res. Lett., 34, doi:10.1029/2006GL027557. Steiner, A. K., G. Kirchengast, B. C. Lackner, B. Pirscher, M. Borsche, and U. Foelsche (2009), Atmospheric temperature change detection with GPS radio occultation 1995 to 2008, Geophys. Res. Lett., 36, L18702, doi:10.1029/2009GL039777. Steiner, A. K., B. C. Lackner, F. Ladstädter, B. Scherllin-Pirscher, U. Foelsche, and G. Kirchengast (2011), GPS radio occultation for climate monitoring and change detection, Radio Sci., 46, RS0D24, doi:10.1029/2010RS004614.] [Gottfried Kirchengast, Austria]	
2-1070	2	26	33		48	GPs has a very large line-of-sight averaging kernel (probably at least hundreds of km) plus a spatial spread due to the movement of the spavecraft relative to one another during an event. Has this affect been considered in the comparisons with other satellite data where the spatial averaging is less pronounced? [Larry Thomason, United States of America]	Taken into account. See response to 2-1066
2-1071	2	26	37	26	38	This is more of an introductory sentence that belongs near the beginning of the previous paragraph. [George Kiladis, USA]	Noted. The entire section has been rewritten and this comment no longer pertains to the modified text.
2-1072	2	26	42			the text: "(from ~60m near the surface ... at 40km)" is confusing. I assume that this refers to the vertical resolution of the GPS-RO data, but this is not clear and needs to be made so. GPS is often only trusted for temperature trends from 5 to 20km altitudes, so the reference to near surface vertical resolution is confusing. [Bruce Wielicki, USA]	Taken into account. See response to 2-1066

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2-1073	2	26	43	26		What is meant by "simulate" here. Is it just applying vertical weighting functions? [Karen Rosenlof, United States of America]	Taken into account. See response to 2-1066
2-1074	2	26	44	26	48	This current explanation is scientifically unclear and factually not fully correct, please give it an updated formulation. The calibration studies of Ho et al. look at the MSU vs RO differences in a different approach than the Ladstaedter et al. (2011) study; the former work in the context of data calibration and cross-calibrated data, the latter is a "plain" comparison looking at differences in anomaly records. The inference what is seen implied is also not correct, the statement "...implying that there are still uncertainties..." is not an implication that derives from the evidence of the current studies. The current status might be taken from the conclusion section of Ladstaedter et al. (2011), basically that further study is needed to understand the remaining differences. [Gottfried Kirchengast, Austria]	Taken into account. See response to 2-1066
2-1075	2	26	44	26	48	These sentences leave the reader hanging and do not provide an expert assessment. They also beg the question of whether GPS RO should match MSU in the first place, given uncertainties in the latter. This is not an effective ending of this brief section and should be reconsidered. [Dian Seidel, USA]	Taken into account. See response to 2-1066
2-1076	2	26	44			The figure 2.11 is introduced here but it's not clear for me that RO AMSO is the GPS RO. If it is, that precision would be welcomed. [Francois DANIS, France]	Noted. We have removed the figure in edits.
2-1077	2	26	46		48	or the other way around? [Kevin Trenberth, USA]	Taken into account. See response to 2-1066
2-1078	2	26	51	26	51	STAR values have 0 anomaly (light blue dash), so this means that the anomalies have been calculated wrt this data set?. Caption of Figure 2.11 should be clarified to indicate this. [Celeste Saulo, Argentina]	Noted. Figure 2.11 has been removed
2-1079	2	26	51	26	51	(Fig. 2.11 caption) Explain LS. [Christian-D. Schoenwiese, Germany]	Noted. Figure 2.11 has been removed
2-1080	2	27	4	25	4	insert comma after 'theory' [Peter Burt, UK]	Editorial
2-1081	2	27	4	27	5	There is no reason why AMSU-A upper air channels cannot provide a good time series of upper air temperatures, it is the merging with MSU which may prove difficult (see ii above) but I agree it probably has not been done yet. [Roger Saunders, United Kingdom]	Noted. No changes requested or made.
2-1082	2	27	4	27	6	This sentence is difficult to understand: it basically says that AMSU cannot currently extend the SSU record, but takes some time to puzzle out. some improved wording would help [Bruce Wielicki, USA]	Noted. We have moved this text to the appendix to meet length limits and redrafted to take into account this comment.
2-1083	2	27	5	25	5	AMSU only → AMSU-only [Peter Burt, UK]	Editorial
2-1084	2	27	6	25	6	merge what? [Peter Burt, UK]	Noted. It should be obvious that we are talking about merging SSU with AMSU from the text. No changes made.
2-1085	2	27	7	25	7	insert comma after 'leak' [Peter Burt, UK]	Editorial
2-1086	2	27	14	27	14	Replace "solely" with "only". [Robert Waterland, United States of America]	Editorial
2-1087	2	27	14	27	21	Reference to Nash (1988) paper (given below) would be useful here.Nash, J. (1988), Extension of explicit radiance observations by the Stratospheric Sounding Unit into the lower stratosphere and lower mesosphere. Quarterly Journal of the Royal Meteorological Society, 114: 1153–1171. doi: 10.1002/qj.49711448213 [Roger Saunders, United Kingdom]	Taken into account. See response to 2-1088
2-1088	2	27	14	27	21	Sorry to have a bit of a rant about this paragraph - I do not think it is justified to place the Liu and Weng work on an equal footing to the Wang et al. work. The former was a useful interim step, but it is a rather brief paper, lacking detail, and there are some clear issues in their analysis - for example, in the neglect of the diurnal and semi-diurnal tides - this is justified by an analogy with other MSU channels which is not justified by the prior literature. Some of the potential issues are discussed in the Seidel et al. WIRES paper. By contast, the Wang et al work is thoroughly documented, publically-available, data set which shows carefully the impact of the assumptions and corrections it makes, and is simply a much more thorough analysis. I believe a form of	Accepted. We now make clearer the distinction between the two newer analyses in the redraft. A better reference to the Nash series is also now made. Much of this material has been migrated to the appendix.

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						words can be adopted that makes clear that one analysis is much deeper than the other and should be accorded greater weight - if the "low confidence" in the exec summary is based on an assessment that all the available studies have equal weight, then I feel this is unsustainable. A minor comments, many readers will associate the original SSU analyses with Nash, and to avoid confusion, I suggest that Nash and Forrester is a better citation than Brownscombe et al. Nash J, Forrester GF. Long-term monitoring of stratospheric temperature trends using radiance measurements obtained by the TIROS-N series of NOAA spacecraft. Adv Space Res 1986, 6:37–44. [Keith Shine, UK]	
2-1089	2	27	16	25	16	(Wang et al., Submitted) → Wang et al. (submitted) [Peter Burt, UK]	Editorial. There are many cases where the citation software and citations need fixing.
2-1090	2	27	16			First time you use the numbers of channels. I believe it would be better to carry on with LT, MT, LS channels as channel's numbers will probably be clear only to specialists. [Francois DANIS, France]	Rejected. These are distinct from those channels as evidenced in what was Figure 2.9 in the FOD and which has been retained.
2-1091	2	27	19	25	19	timeseries → time series [Peter Burt, UK]	Editorial
2-1092	2	27	19	25	19	behavior → behaviour [Peter Burt, UK]	Editorial
2-1093	2	27	20	25	20	insert comma afetr cooling [Peter Burt, UK]	Editorial
2-1094	2	27	20	27	21	Replace "Although all SSU datasets agree that the stratosphere is cooling beyond that very substantial uncertainty currently remains." with "Despite these difficulties, all SSU datasets show stratospheric cooling, but the magnitude of the cooling and its vertical structure are highly uncertain at present." [Robert Waterland, United States of America]	Taken into account, this text has been redrafted accordingly
2-1095	2	27	21	25	21	beyond that...remains' → 'very substantial... remains beyond that.' [Peter Burt, UK]	Taken into account, See response to 2-1094
2-1096	2	27	25	27	37	This subsection needs to be rewritten. It is not clear what "upper troposphere maximum" means, maximum in what? The meaning and significance of an "anchor point" is elusive without further explanation. [George Kiladis, USA]	Accepted. We have made edits to reflect these concerns. The section has also been subsumed into another section for brevity.
2-1097	2	27	25	27	37	Is there an assessment her on whether this method is useful? [Karen Rosenlof, United States of America]	Accepted. We now place a bottom line conclusion on this section which has been subsumed into a longer section.
2-1098	2	27	26	25	27	time varying → time-varying [Peter Burt, UK]	Editorial
2-1099	2	27	28	27	28	Replace "and Sherwood (2007) first investigated applicability of using these winds to infer" with 'and Sherwood (2007) initially used radiosonde wind data to infer". [Robert Waterland, United States of America]	Accepted
2-1100	2	27	29	25	29	Insert 'this' after extended [Peter Burt, UK]	Editorial
2-1101	2	27	29	27	30	Sentence is not clear. ".. distinct tropical upper tropospheric maximum…" of what? [Birgit Hassler, USA]	Taken into account. See response to 2-1096
2-1102	2	27	29	27	30	Replace "They then extended to a global analysis (Allen and Sherwood, 2008) which implied a distinct tropical upper tropospheric maximum" with "They then extended their treatment to a global analysis (Allen and Sherwood, 2008) which implied a distinct tropical upper tropospheric maximum in temperature gradient". [Robert Waterland, United States of America]	Taken into account. See response to 2-1096
2-1103	2	27	30	27	30	"upper tropospheric maximum" in what? Temperature gradient, temperature, temperature trend? [Dian Seidel, USA]	Taken into account. See response to 2-1096
2-1104	2	27	32	25	32	utilised → used [Peter Burt, UK]	Editorial
2-1105	2	27	34	27	34	Change "…a finding later confirmed by (Christy, 2010), who…" to "…a finding later confirmed by Christy (2010), who…" [Alice Grimm, Brazil]	Editorial. There are many cases where the citation software and citations need fixing.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1106	2	27	34			"Christy" should not be in parens, only (2010) [Michael Brewer, United States of America]	Editorial. There are many cases where the citation software and citations need fixing.
2-1107	2	27	36	27	37	In this sentence it is said that changes in anchoring tended to reduce the appearance of a tropical upper tropospheric maximum. Could a citation or citations be added to the sentence? And what does that mean for the suitability of the whole data set? [Birgit Hassler, USA]	Accepted. Citations added. See also response to 2-1097
2-1108	2	27	37	27	37	Add a reference to support the final sentence of this section [Dian Seidel, USA]	Taken into account. See response to 2-1097
2-1109	2	27	39			Is there added value in the synthesis of all observations in a reanalysis like ERA-interim? [Geert Jan van Oldenborgh, Netherlands]	Noted. This is discussed in Box 2.3.
2-1110	2	27	41	27	42	The statement "… with each decade warmer than all preceding decades in the record…" cannot be seen clearly in Figure 2.12, bottom. Maybe adjust figure? [Birgit Hassler, USA]	Noted. This segment of text has been removed and the figure redrafted in response to other concerns.
2-1111	2	27	41	27	42	It's not clear that Fig. 2.12. supports this statement about progressively warmer decades, at least not in any explicit way. Is there a published result to support this assertion? [Dian Seidel, USA]	Taken into account. See response to 2-1110
2-1112	2	27	41	27	43	Stricctly speaking sentence not correct, ie for surface temps not every decade has been warmer than all preceding decadesin he record- eg 1900s not warmer than all preceding decades. Just reword to make clear referring to since mid 20th century for both. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1110
2-1113	2	27	49	27	49	The statement "… Each decade has been cooler than all preceding decades." cannot be seen clearly in Figure 2.12, top. Maybe adjust figure? [Birgit Hassler, USA]	Accepted. This segment of text has been removed.
2-1114	2	27	49	27	49	This comparable statement about decade by decade stratospheric cooling is also not obvious. With the El Chichon warming, the 80s appear warmer than the 70s, but this is just an eyeball estimate. Is the statement supported by literature? If not, delete it. [Dian Seidel, USA]	Taken into account. See response to 2-1113
2-1115	2	27	52	27	54	Similar statements have been made by all radiosonde temperature dataset producers. See Free et al. 2005 (already in the references) and Free, M., and D. J. Seidel, 2007: Comments on "Biases in stratospheric and tropospheric temperature trends derived from historical radiosonde data" by Randel and Wu (2006). J. Climate, 20, 3704-3709, DOI: 10.1175/JCLI4210.1 [Melissa Free, USA]	Accepted. Sentence has been modified to make this clear.
2-1116	2	27	54	27	54	The last sentence should be qualified to refer specifically to lower stratospheric temperature. [Melissa Free, USA]	Accepted
2-1117	2	27	54	27	54	Last sentence stands somehow in contrast to the statement earlier that lower stratospheric temperature decadal means have been decreasing continuously since the mid 20th century. Maybe rephrase? [Birgit Hassler, USA]	Taken into account. See response to 2-1113
2-1118	2	27				2.2.4.7 nothing on reanalyses: there must be. [Kevin Trenberth, USA]	Noted. We discussed the inclusion of reanalyses for this diagnostic exhaustively based on this and other comments and decided against their inclusion.
2-1119	2	28	1	28	5	Fig 2.12 has a feature that bothers me. The AMSUs show a large excursion relative to the RAOB results between 1998 and 2008. I've looked at the AMSU satellites individually and can't find a problem there. The independent satellites all agree to very tight tolarances (we have NOAA-15 and AQUA for most of this - two exceptionally stable instruments.) I haven't had time to look for a reason in the RAOB datasets that would cause such a thing. I think the difference between UAH and RSS is the overcorrection of RSS for the diurnal drift, but you can see that is rather small compared to UAH and RSS collaborative differences vs. the RAOBs. Someone on the author team should look into this before things are written in stone. There is an explanation for the differences, and right now I can't find it in the AMSUs. Regarding the stratosphere: I think the microwave is much closer to the truth than the radiosodes and if that could be accomodated in the difference plot somehow (with microwave in the mean?), it would be more informative. To be persnickity, I've always used the convention that UAH be in blue (our school colors), RSS be red (R for red), HadAT green (obvious), and the others as seen fit. [John Christy, USA]	Noted. Some of the new papers discussed earlier start to make some sense of this (particularly the Mears et al. intercomparison paper) but we do not feel it is right, at least here, to try to unpick such issues. This would be more appropriate within the peer reviewed literature. The plot revision should hopefully improve the color differentiation but the colors are to be consistent with remaining similar plots..

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1120	2	28	7	28	7	Should include RATPAC. [Melissa Free, USA]	Accepted. RATPAC dataset creators have furnished us with the data required - the lack of its availability for the required regions and diagnostics had precluded inclusion in the FOD.
2-1121	2	28	7	28	11	(Table 2.9 caption) Explain OLS, LT, MT, and LS. Moreover, an information concerning the altitude range of these layers would be welcome. [Christian-D. Schoenwiese, Germany]	Taken into account. Cross-reference to Figure 2.9 has been added to address the reviewer's concerns. The trends issue is exhaustively discussed in Box 2.2.
2-1122	2	28	7	28	12	I would like to see a mention of the difference between measurement error and temporal sampling error. The first is due to all the problems with instruments and geography - and shows up even when there is no temperature variability. The second deals with the character of the time series, and is not really an error, but an uncertainty because it is present even when an instrument is perfect (i.e. without error). This tries to estimate the range of trends that would be produced from a random drawing of 53 or 33 year time series from a pot with lots of time series with the same statistical attributes as the observed. [John Christy, USA]	Noted. These issues are discussed in Box 2.1 and elsewhere in the temperatures section as a whole and there is not space to repeat them here.
2-1123	2	28	7	28	12	Why does this table not include adjusted data from RATPAC (Free, M., D.J. Seidel, J.K. Angell, J. Lanzante, I. Durre, T.C. Peterson, 2005: Radiosonde Atmospheric Temperature Products for Assessing Climate (RATPAC): A new data set of large-area anomaly time series, J. Geophys. Res., 110, D22101, doi:10.1029/2005JD006169)? Similarly, why does Fig. 2.12 omit this dataset? [Dian Seidel, USA]	Taken into account. See response to 2-1120
2-1124	2	28	7	28	12	Is the number of significant figures reported in the table really justified? [Dian Seidel, USA]	Noted. We discussed in some detail restricting the tables to 2 decimal places as this is an important point and we should not be giving a false sense of confidence. In the end the CLAs decided to go ahead with the current presentation style.
2-1125	2	28	10	28	10	If many groups include the parametric uncertainties in the trend estimations, were they removed for the table since in line 11 it is said that they are not considered? [Birgit Hassler, USA]	Noted. Three groups now have such estimates, but they are derived in very different ways which makes their inclusion problematic. They are compared in the revised version of Haimberger et al and that is now be suitably referenced in the SOD.
2-1126	2	28	14	28	21	It should be mentioned in the text that this analysis is based on ERA-Interim data, not any of the datasets described in Table 2.9 [Birgit Hassler, USA]	Noted. We have decided to replace the figure wth a multi-panel plot including RSS and UAH trends.
2-1127	2	28	14	28	21	Why is the analysis described here based on ERA-Interim data and not based on any of the datasets described in Table 2.9? [Birgit Hassler, USA]	Taken into account. See response to 2-1126
2-1128	2	28	14	28	21	This comment on regional trends needs confidence bounds such as those in Table 2.9 above: perhaps just that all trends colored in Fig 2.13 are significant at the 5 to 95% confidence level, while greyed out areas are not? [Bruce Wielicki, USA]	Taken into account. See response to 2-1126
2-1129	2	28	15	28	16	It's unclear why the surface temperature is presented here in the upper-air section. No terribly significant point seems to be made about it. [Melissa Free, USA]	Taken into account. See response to 2-1126
2-1130	2	28	16	28	16	Change "channels" to "layers" because it's the troposphere, not the channel, that we are considering. [Dian Seidel, USA]	Accepted
2-1131	2	28	18	28	18	Change "is cooling" to "cooled" as the data end in 2010, and who knows what will be the case when the AR5 is issued. [Dian Seidel, USA]	Accepted
2-1132	2	28	18	28	21	please give reference [Elisa Manzini, Germany]	Comment contains insufficient detail to be understood or actioned. Text being mentioned is describing the maps so no reference should be required.
2-1133	2	28	21			"similar longitudes" Huh? [Kevin Trenberth, USA]	Accepted. This text has been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1134	2	28	23	28	23	Insufficient basis has been given for using ERA-Interim for upper-air temperatures, especially in a figure that shows values all the way to the poles. [Melissa Free, USA]	Taken into account. See response to 2-1126
2-1135	2	28	23	28	23	I find Fig. 2.13, showing upper-air temperature trends from the ERA-Interim, to be unacceptable, both because the AR5 should not be relying on reanalyses for trends and because doing so would send a terrible message to the community, which is already misusing reanalyses this way much too often, [Dian Seidel, USA]	Taken into account. See response to 2-1126
2-1136	2	28	23	28	25	Fig 2:13 is rather profound. It clearly shows the trend-ratio of surface vs. troposphere is not very remarkable, especially in the tropics. This is a key point to note - the scaling ratio is very nearly 1 or even less. The modeler evaluators will then have to deal with this observation. [John Christy, USA]	Noted. No action was requested in the comment or taken in response to it. Note that the figure has been replaced following other feedback and no longer contains surface estimates in this draft. This is in response to requests to use satellite prducts. We have had discussions with Chapters 9 and 10 over the appropriate use of observations in their sections which address this issue.
2-1137	2	28	24	28	24	(Fig. 2.13 caption) Compare comment to chapter 2, page 12, line 25 ff. In this case "temperature" should be added. [Christian-D. Schoenwiese, Germany]	Accepted
2-1138	2	28	27		42	This section fails to adequately address the incomplete global coverage and changes over time. The statement line 42 is inadequate. Biases in sondes are also not adequately addressed. This relates to the changes in instrumentation over time as thermisters have become smaller and less subject to radiation effects. [Kevin Trenberth, USA]	Rejected. The biases have been adequately sampled in the context of the five estimates of the radiosonde trends resulting from different analysis pathways and outlined in some detail in the radiosonde section and table in the appendix. The spatial sampling issue would be of great interest to cover, but there simply is not the space afforded us to address this issue graphically or in any further detail than is already the case.
2-1139	2	28	29	28	29	When it is said "the rate and details of the vertical structure are distinctly dataset dependent", I think this is especially true for the stratospheric data, while those in the troposphere show an fairly good agreement between the different dataset. [Claudio Cassardo, Italy]	Accepted. Sentence has been nuanced to stress that this is more so in the stratosphere.
2-1140	2	28	31	28	32	Does the distinct amplification also extend vertically to within the UT? [Klaas Folkert Boersma, Netherlands]	Taken into account. See response to 2-1141.
2-1141	2	28	31	28	32	This statement doesn't seem consistent with the figure which appears to show 4 rather similar datasets for much of the troposphere in the tropics (only showing differences higher up when they switch to cooling from warming). [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. This statement has been removed for expediency. A discussion now occurs later on in its stead.
2-1142	2	28	32	28	32	The relationship between the "upper-troposphere" and the near surface in the tropics is not shown in Fig2.14. Near surface is not shown and how is upper-troposphere defined? This seems a rather vague statement. It would be useful to back it up with an appropriate figure. [Gareth S Jones, UK]	Taken into account. See response to 2-1141.
2-1143	2	28	34	28	34	quadrature? [Marcus Sarofim, USA]	Taken into account. See response to 2-1144
2-1144	2	28	35	28	36	Can you say this using less jargon, or remove it altogether? [Dian Seidel, USA]	Accepted. Sentence has been removed.
2-1145	2	28	38	28	38	Add "radiosonde" before "data products". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1146	2	28	38	28	38	Similar as above. [Christian-D. Schoenwiese, Germany]	Comment has insufficient context to be actionable. Am assuming reviewer is asking for explicit mention of temperature trends in the caption. In which case it is addressed.
2-1147	2	28	44	28	44	This lead sentence is pretty weak. A better transition from discussion of radiosondes to satellite data should be crafted. [Dian Seidel, USA]	Accepted. This has been redrafted accordingly.
2-1148	2	28	44	28	46	What does this sentence mean? Differences between radiosondes and MSU or between different radiosonde	Taken into account. Combined with 2-1147

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						data sets? [Karen Rosenlof, United States of America]	
2-1149	2	28				Table 2.9: I think that this information would be more striking if it were presented as a figure. [Jeffrey Taylor, United States of America]	Noted. These data are already available in what were Figures 2.14 and 2.15 and have been retained so no changes made.
2-1150	2	29	1	29	7	You should mention here that for the 1958-2010 data series in the tropics, inclusion of a mean shift (or break term) to capture the 1977-78 Pacific Climate Shift causes the RICH and HadAT trends in the LT and MT to fall to zero and become statistically insignificant. See McKitrick and Vogelsang (2010) ref. in cell 43. It is rather misleading to talk about the linear trends in the entire 1958-2010 radiosonde series when the rise is all accounted for in one step. [Ross McKitrick, Canada]	Noted. Issues of linear trend calculation have been covered already in Box 2.2 and the timeseries are also plotted.
2-1151	2	29	1	29	7	McKitrick, Ross R. and Timothy Vogelsang (2010) "Multivariate trend comparisons between autocorrelated climate series with general trend regressors" in prep. See University of Guelph Economics Discussion Paper 2011-09, http://www.uoguelph.ca/economics/sites/uoguelph.ca.economics/files/2011-09.pdf. [Ross McKitrick, Canada]	Taken into account. See response to 2-1150
2-1152	2	29	1	29	7	It's not clear what this means. What's a damping of a trend? What's an amplification of a trend? Do you really mean reduction and increase? [Karen Rosenlof, United States of America]	Taken into account. This discussion has been deleted and replaced by a more streamlined discussion at the end.
2-1153	2	29	2	29	3	Please clarify what the damping and amplification is with respect to. If it is near-surface temperatures then these should also be shown in Fig2.15. [Gareth S Jones, UK]	Taken into account. See response to 2-1152
2-1154	2	29	2	29	3	This is an important sentence but needs unpacking. First it is ambiguously stated - damping of trends aloft means what exactly ? Damping of surface trends ? And - discounting Raobcoare - ths variatinon from slight damping to substantial amplification in the tropics does not apper clearly in the figure certainly not for radiosondes and for satellite not very easy to see in the figure (and MT is contaminated by stratospheric cooling so there is some unknown discounting that has to take place of the spread due to influence of stratospheric cooling) [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1152
2-1155	2	29	2	29	4	Second sentence on the page is extremely unclear. Maybe you mean to say "a range of possible vertical patterns" instead of "possible behavior"? [Melissa Free, USA]	Taken into account. See response to 2-1152
2-1156	2	29	3	29	3	"substanial amplification"? I don't see it at all in the global or tropics. At best it is 1.0 amplification. This has been published. [John Christy, USA]	Taken into account. See response to 2-1152
2-1157	2	29	5	29	5	delete 1st 'are' [Peter Burt, UK]	Editorial
2-1158	2	29	8			It is here that I would have expected some conclusions. And among the conclusions I would have expected reference to Figure 2.10 and explicit mention of that fact that the more rapid warming of the upper troposphere predicted by all General Circulation models (see Figure 10.7 in AR4) was NOT evident. It may be inconvenient to some, but in science, inconvenience must bow to data, and if the data do not confirm the models, then - in this case in particular - the global audience must be warned. Anything less is irresponsible. [Philip Lloyd, South Africa]	Rejected. It is not the role of this chapter to compare the observations to the models. That is the role of Chapters 9 and 10 where such analyses are discussed in some depth.
2-1159	2	29	10	29	10	Add “and including MSU products”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1160	2	29	14	29	16	Such absurdly accurate confidence limits have to assume that the multiple averages of miscellaneous neasurements upon which they are based can be assumed to be constants, when all of them have considerable variability, often non linear. [VINCENT GRAY, NEW ZEALAND]	Rejected. Comment is not readily understandable and the statement being questioned is backed up by a whole sub-section on surface temperatures and hence the literature. The commenter gives no substantive grounds for their criticism.
2-1161	2	29	14			"Near-surface". In that summary, I cannot find a paragraph talking about near-surface temperatures... To make a difference with land surface T° (following paragraph) I would emphasise that it is *world* surface T°? Or is it “just above ocean surface” temperature? [Francois DANIS, France]	Accepted. Wording has been added to this effect here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1162	2	29	15	29	15	nineteenth → 19th [Peter Burt, UK]	Editorial
2-1163	2	29	15	29	16	Can you state the datasets that the amounts of warming refer to in this paragraph? Also giving trends in deg C per decade makes the text read odd. A trend is given since 1901, then you say much of this (and give a larger number) occurred since 1979. [Philip JONES, UK]	The numbers have been removed in redrafting
2-1164	2	29	15	29	16	"Much of this warming has occurred since 1979 (0.167 ± 0.034°C per decade)." It would be better expressed as "Some of this warming occurred between 1910 and 1940 (0.117 ± 0.029°C per decade) but the warming between 1979 and 2010 (0.167 ± 0.034°C per decade) was significantly higher." HADCRUT3 data [Philip Lloyd, South Africa]	Taken into account, see response to 2-1163
2-1165	2	29	16	29	18	Instead of “high frequency variations” perhaps it is better to use simply “variations” or “natural variations”, since the high frequency variations could be related to even synoptic variability, and this obviously is not the intention in this sentence. [Alice Grimm, Brazil]	Accepted.
2-1166	2	29	18	29	18	I suggest adding the following after "…effects": "and should be omitted." [Martin Hovland, Norway]	Noted. The wording here has been changed
2-1167	2	29	18	29	20	It is important that "uncertainty studies" are mentioned, it is a shame however that they don't appear to have been used in the assessment of near surface trends. [Gareth S Jones, UK]	Noted. We do not entirely understand this comment as such uncertainties have been used in various ways in the sections upon which this conclusion is built. Without further explicit guidance no action can be taken here.
2-1168	2	29	20	29	22	The last sentence is mainly a repeat of the first 2 sentences of the same paragraph. [Francois DANIS, France]	Noted. Efforts have been made to improve brevity of the text generally. Much of this text has been removed.
2-1169	2	29	21	29	21	superscript 'th' [Peter Burt, UK]	Editorial
2-1170	2	29	24	29	25	It should be noted that there has been no significant warming since 2001 [VINCENT GRAY, NEW ZEALAND]	Rejected. This is discussed in the main text for trends since 1998 - it does not raise to summary status in the authors' view.
2-1171	2	29	25	29	25	superscript 'th' [Peter Burt, UK]	Editorial
2-1172	2	29	25			temperatures have "risen" not "warmed" The air has warmed. Elsewhere also. [Kevin Trenberth, USA]	Accepted
2-1173	2	29	27	29	27	The main reason is the reduced sampling density - meaning that there are areas with no samples. The other two are not that important. [Philip JONES, UK]	Accepted, the review comment better matches the main section text.
2-1174	2	29	32	29	36	Again, I don't think this conclusion is warranted [Marcel Crok, The Netherlands]	Rejected. Reviewer provides no reason beyond their personal opinion.
2-1175	2	29	32	29	36	Return to the mere opiniions of the self styled experts [VINCENT GRAY, NEW ZEALAND]	Rejected. Comment is ad honheim with no science to act upon.
2-1176	2	29	32	29	36	This is a good paragraph. Make sure it is kept! [Philip JONES, UK]	Noted. Given the substantive section rewrite some changes have been required.
2-1177	2	29	32	29	36	As stated above, the "less than 10%" claim is made up from thin air, and the whole paragraph relies on the invalid arguments used to set aside evidence of global-scale correlations between the spatial pattern of warming and the spatial pattern of socioeconomic activity. I won't belabour the point, but fixing the text in the chapter will require removing and replacing the various summaries here and elswhere. [Ross McKitrick, Canada]	Rejected. The conclusion is supported by the literature reviewed in the main section. The main section and this summary paragraph have both been redrafted to make this clearer.
2-1178	2	29	34	29	36	Is the 1/4 of the observed warming in "recent decades" (how many decades over what period?) over all of China or over sub regions of China. This should be clarified. [Gareth S Jones, UK]	Noted. We have removed specific reference to China here in the redraft to avoid such issues.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1179	2	29	34			"underlie"? [Kevin Trenberth, USA]	Accepted. Changed to account for
2-1180	2	29	38	29	42	The reduction in DTR is well established, also with homogenized data. Christy used homogenized data for his studies in Africa and California [Marcel Crok, The Netherlands]	Rejected. East africa and California are not globally representative and as discussed in the chapter there are multiple ways to homogenize the data. Until DTR homogeneity is looked at in more detail and globally changes cannot be made along the lines suggested.
2-1181	2	29	38	29	42	There should be a reference to this interesting speculation. I have long been concerned at the inaccuracy and possible bias of assuming that the average of a maximum and minimum temperature should be regarded as a plausible average daily temperature. I carried out a small test on 24 of New Zealand Weather stations in which I compared the Max/Min average with the hourly average, I published the results in my paper: Gray, V R, Climate Change 2007: he Physical Science Basis:: Summary for Policymakers. Energy and Environment, 18, pages 433-440. which you might like to include in your bibliography.. This is an extract from that paper: "What you must not do is calculate the average from the maximum and minimum values. The error involved in this calculation may be sampled from figures supplied by: http://www.niwascience.co.nz/edu/resources/climate/minairtemp/data_minairtemp_excel.xls/view_file This website supplies two Excel spreadsheets giving hourly temperature measurements from 24 New Zealand weather stations, one in summer and one in winter. . For the summer figures, the mean difference between the max/min reading and the average of 24 hourly readings is +0.5ºC, with a range of +2.6ºC to -0.4ºC. For the winter figures, the mean difference between the max/min reading and the average of 24 hourly readings is +0.9ºC, with range of +1.9ºC to -0.9ºC." [VINCENT GRAY, NEW ZEALAND]	Noted. References have been made in the sub-section which this is summarizing. The summary section should not include references. The maximum and minimum are precisely what is required to calculate the diurnal temperature range. So remainder of comment makes no sense in the context of the text being commented upon by the reviewer.
2-1182	2	29	39	29	39	Diurnal Temperature Range → diurnal temperature range [Peter Burt, UK]	Rejected. First use of term when pulled through to SPM requires capitalization so that the acronym used thereafter is obvious to the reader
2-1183	2	29	41	29	42	This isn't quite worded like the main text and the last sentence lends itself to becoming the take-home message, which seems to give more credance to min increasing more than max temperatures than the main text indicates on pg 2-15. [Beverly Law, USA]	Accepted. This final sentence has been deleted in the current redraft.
2-1184	2	29	44	29	45	It should be noted that there has been no significant warming since 2001 [VINCENT GRAY, NEW ZEALAND]	Rejected. The statement currently there is characterizing changes on the longest timescales.
2-1185	2	29	44	29	50	This sumary doesn’t seem to quite capture the state of play with SST analyses, that only one to date (HadSST3) has incoporprated bias corrections for different measurement types and which shows systemaic inconsistency between the new estimate and previous uncorrected estimates. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted, text reflecting how the HadSST3 estimate nuances our understanding is now explicitly added here.
2-1186	2	29	45			"step change" is really a big increase [Kevin Trenberth, USA]	Taken into account. See response to 2-1187
2-1187	2	29	45			"step change" is again ambiguous as in comment number 2. : "major improvement"? [Bruce Wielicki, USA]	Accepted
2-1188	2	29	50	29	50	superscript 'th' [Peter Burt, UK]	Editorial
2-1189	2	29	50	29	50	"since the 1950s" [George Kiladis, USA]	Accepted
2-1190	2	29	52	29	52	I suggest inserting the following between the words: "from" and "weather": "a very low density grid of" [Martin Hovland, Norway]	Rejected. See responses to similar comments from the same reviewer in the section which this is summarizing.
2-1191	2	29	52	29	53	It should be noted that there has been no significant warming since 2001 [VINCENT GRAY, NEW ZEALAND]	Rejected. See response to 2-1184

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1192	2	29	52	29	55	If there is medium or low confidence in the results for the tropics and SH extratropics, how can the "global" results be stated with "very high confidence"? [Ross McKitrick, Canada]	Accepted. We have significantly modified this section to make it clearer.
2-1193	2	29	55	29	55	I am struggling to reconcile this low confidence statement with Fig 2.14 middle lower panel (Tropics) which until the datasets start showing cooling above about 200hPa seem to show very similar trends indeed from 4 different datasets. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have added explicit wording to make clear that the spread primarily relates to the shorter sub-period since 1979. This is when the largest instrumentation changes occurred - specifically sheilding of the instruments from direct solar radiation and therefore the biases are most important.
2-1194	2	29	55			"but elsewhere confidence is low, particularly in the tropical upper troposphere." I would question this conclusion, particularly in the light of the data shown in Fig 2.10 and 2.12 (and see also the next comment) [Philip Lloyd, South Africa]	Taken into account. See response to 2-1193
2-1195	2	29				Fig. 2.15: it would be useful to define the Tropical and Extra-Tropical regions here or in the text. Some people use "Tropics" to mean between 30 degrees N and S, while others define the region to be between the Tropic of Cancer and the Tropic of Capricorn. [David Pearson, United Kingdom]	Accepted. Figure caption to Figure 2.14 has been modified to make the definition of the tropics used explicit.
2-1196	2	29				some of written expression may need more refined [Bing Qiao, China]	Noted. Without explicit guidance we cannot make any revisions.
2-1197	2	30	2	31	26	FAQ2.1: The language of this FAQ is well suited to a non-specialist reader, and the "initial answer" paragraph is clearly expressed and helpful. [David Wratt, New Zealand]	Noted. We have further tightened the text in response to science editor guidance.
2-1198	2	30	4	30	5	This overstates the situation in the stratosphere. Add "lower" before "stratosphere" as we don't know anything about the middle and upper stratosphere in the mid-20th century. And what are the "four independent observing technologies"? I count one - radiosondes. For a shorter period, there are three: sondes, MSU, and SSU. And for an even shorter period, GPS RO makes a total of four. Others are not global. [Dian Seidel, USA]	Accepted. Sentence has been modified to take these criticisms into account.
2-1199	2	30	4	30	9	Here is the summary - where is the evidence discussed? [Elisa Manzini, Germany]	Noted. The text it is summarizing is earlier in the section.
2-1200	2	30	4		8	The wording on confidence can surely be improved? The specific value will always have errors bars on it. [Kevin Trenberth, USA]	Rejected. The confidence language is used appropriately here. The stratospheric estimates have substantial differences that can only yield low confidence in the details of the stratospheric cooling.
2-1201	2	30	12	31	26	FAQ 2.1 sits a little awkwardly here as much of the evidence comes in later chapters. However this may still be the best place. If so, cross referencing to each of the facts mainly discussed in other chapters is needed. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Editorial. The explicit guidance is to remove dependencies on sections from the FAQ.
2-1202	2	30	14	30	14	The title should be "How do we know the world has warmed?" past tense. The observational record is all past tense. What is happening right now and the question "how do we know the world is warming (present tense)?" is handled in later chapters. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1203	2	30	14	30	47	While the temperature of the entire world has warmed, many regions have not warmed and some have cooled. That this fact should be stated is supported by the "spatial variability of precipitation" that is addressed under "2.3.1.2 Spatial Variability of Observed Trends" at 2 32 (lines 46 and following), yet the well known spatial variability of temperature goes unmentioned. Large regions around the world have not warmed or have even cooled. For example, my observation site since 1988 is centered in Central Texas. From Jan 1895 to Nov 2011 (116 years), Central Texas (NOAA State Code 41, Division 7) cooled by slightly more than 0.1 degree F (based on the means for all months and the IPCC preferred linear trend). Susan Solomon et al., and others have shown that temperatures measured by Thomas Jefferson and by the Lewis and Clark Expedition closely track measurements made for those respective locations today. The air and sea temperatures measured at Hilo, Hawaii, during winter 1840-41, by the US Exploring Expedition were very close to modern values. Many other examples can be cited. See Comments 39 and 40 above. [Forrest Mims, USA]	Rejected. The FAQ makes abundantly clear that it is dealing with global mean quantities. Regional details are discussed in this chapter and in the two other chapters on which this FAQ builds.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1204	2	30	16	30	22	Extremely misleading and requires major revision. Average temperatures either of "The world" or of the surface of the earth cannot be measured,.so any such"warming" is speculation and is certinly not "equivocal" There is anecdotal evidence that current temperatures that exist on the earth are similar to several other periods in human history. There is anecdotal evidence that the 18th Century was cooler than the 20th century. There is both anecdotal evidence and limited observationl evidence that there may have been a smlall increase in the course of the 19th Century and there is much better evidence from the improved observations that, so far, there has been no significant rise in the 21st Century. There are also a number of indications, ranging from a pause or increase in ice and glaciers to reversal of ocean oscillations and changes in the sun, that there may be a future reduction. The draft statement cannot be supported from the evidence that is presented in this Report. [VINCENT GRAY, NEW ZEALAND]	Rejected. The reviewers comments are largely at variance with the actual, measurement and analysis based, evidence presented in the FAQ. The aspects surrounding potential future changes are out of scope.
2-1205	2	30	16	31	18	"That the world has warmed is unequivocal." Nothing in science is truly 'unequivocal'. The use of the word in this context has come to represent the worst aspects of the IPCC's assessment of science, and should therefore be avoided at all costs. Could I suggest "All evidence confirms that the world has warmed" as a more judicious? and close with "all points unequivocally to one thing: the world has warmed" [Philip Lloyd, South Africa]	Rejected. This language is deliberate linkage to AR4.
2-1206	2	30	16	31	25	FAQ2.1 is good! [Philip JONES, UK]	Noted.
2-1207	2	30	16			That the world has warmed at the end of the twentieth century does not mean it will necessarily continue to warm. If there is a 60 years-period oscillatory component and that the pause observed in the 13 recent years is indeed the top of the cycle, a cooling might even occur as has been observed from 1950 to 1975. It has been attributed to aerosols like SO2 but this is questionable because SO2 is also a greenhouse gas owing to the difference of electronegativity of sulfur and oxygen in the molecule. More generally to attribute cooling events to aerosols which would disappear by some magic from the atmosphere during warming events every sixty years is questionable. [François GERVAIS, France]	Noted. But this is entirely outside the scope of the FAQ. The FAQ is dealing with solely the observational evidence, not the causes or the potential future climate system trajectory. Change to title in response to 2-1202 should help here.
2-1208	2	30	24	2	24	It is not possible to measure a globally averaged temperature. The sequence you favour is a temperature anomaly based on unrepresentative, poorly characterised and controlled constantly changing sites which are bedevilled by urban, land use and economuc change, and by variabilities in ocean oscillations, the sun and cosmic rays [VINCENT GRAY, NEW ZEALAND]	Rejected. Unsupported assertion at odds with the chapter text and assessment. Comment is requesting we overlap with Chapter 10 and start discussing causes which is not the intended purpose of this FAQ or chapter.
2-1209	2	30	25		26	The slope of the warming trend observed from 1910 to 1940 when greenhouse gas concentrations were much lower than nowadays, is similar to the one observed from 1975 to 1998. This reinforces the evidence for the natural oscillatory component of period 60 years reported by Scafetta and other authors. [François GERVAIS, France]	Noted. Out of FAQ scope which is dealing with the evidence for changes and not trying to ascribe likely causes. Change in title should help here.
2-1210	2	30	25			"warmed between 1900 and 1940" actually we don't know that the sub surface ocean warmed or Arctic sea ice changed etc. A lot of "warming" was over land in drought (e.g. US dustbowl) and is not so clearly a warming of the planet. [Kevin Trenberth, USA]	Accepted. But edits have made this point moot.
2-1211	2	30	26	30	27	The wording "The IPCC AR4 concluded that this “warming of the climate system is unequivocal” is curious. Is the author implying that this conclusion was specific to AR4 and is no longer supported in AR5? A casual reading might seem to suggest that. If that is *not* the intent of the author, then this should be rephrased so it is less ambiguous, e.g. "The IPCC concluded that this “warming of the climate system is unequivocal (AR4)" or "The IPCC concluded that this “warming of the climate system is unequivocal in AR4". [Michael Mann, USA]	Accepted. This sentence has been deleted.
2-1212	2	30	29	30	38	You seem to be admitting the limited validity of the mean surface temperature anomaly, but you do not admit that it is the best you have and thatn the others are even worse, descending to the level of mere anecdotes [VINCENT GRAY, NEW ZEALAND]	Rejected. The datasets considered here are peer reviewed. The science process works and it is clear that remaining estimates have substantial value.
2-1213	2	30	29	31	24	I'm glad discussion of this topic (the strong interlinkages between the different climate system elements, and the consistency between independent data sets and indicators) occurs in this report; it is important to emphasis this interlinkage in gaining a good understanding of the climate system and changes in the climate system. [Richard Heim, U.S.A.]	Noted. Thanks.

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2-1214	2	30	30			“line of evidence”... it's a FAQ so a use of easy to understand words would help. Indicator is good enough to me. [Francois DANIS, France]	Accepted. This wording has been made clearer in the redraft.
2-1215	2	30	32	30	32	I suggest inserting the following between the words: "world" and "comes": "including the oceans" [Martin Hovland, Norway]	Rejected. Comment breaks the flow of the text and oceans is obvious in the context of the FAQ as a whole.
2-1216	2	30	34	30	47	If you are going to appeal to the similarity of changes across all the different data sets then you need to plot them (at least the temperature data sets) with the same vertical scale. The top 3 panels in the left bank and the top 2 panels in the right bank are on different scales, with the dilations serving to exaggerate the apparent similarity of trends. [Ross McKitrick, Canada]	We created two versions of this diagram in response to this comment and the view of the author team members who expressed an opinion was that the version with dynamic y-axis ranges was preferable.
2-1217	2	30	34	30	47	"...and this is borne out by a number of independent analyses." But you didn't cite any. Instead the few studies cited found otherwise. [Ross McKitrick, Canada]	Editorial. FAQs should not include citations. The text also relates to the timeseries plot and not to the literature.
2-1218	2	30	35	30	35	FAQ 2.1 Figure 1: I would use either 'specific humidity' or 'water vapour' instead of just 'humidity' because relative humidity and specific humidity do not behave the same so this can be confusing. [Kate Willett, UK]	Accepted, water vapor has been used
2-1219	2	30	38			Because it is part of FAQ, I would add something like: measurements of all those climate elements have changed in the way expected for a warming world; some are shown figure 2. [Francois DANIS, France]	Accepted. Extra wording has been added to the Figure caption pointing to the second figure to this effect.
2-1220	2	30	40	30	43	This sentence is somewhat unclear. Is the intention to say that near-surface air temperature and sea surface temperature are independent indicators of climate change? The current phrasing could be misinterpreted as meaning that the underlying measurement data in each land record is independent (rather than being based on a similar set of measurement data), with the same being said of data independence in the SST datasets. [Colin Morice, UK]	Taken into account. Edits hopefully clarify this in the new draft.
2-1221	2	30	43	30	43	sea-surface temperature: previously sea surface temperature [Elizabeth Kent, England]	Editorial, has been modified for consistency
2-1222	2	30	43	30	44	The differential rate of warming between land and ocean is given a different explanation in Chapter 10 (pg 16 line 3-5). The statement needs possibly to be rewritten. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence has been removed for expediency.
2-1223	2	30	43	30	45	The fact that the gap remains in stabilisatiuon runs shows that hte major factor is the difference between wet and dry lapse rates. [Geert Jan van Oldenborgh, Netherlands]	Taken into account. Combined with 1222
2-1224	2	30	44	30	47	Because it's a FAQ, the answer should be accessible to most people. I wouldn't bother with such details (the last 2 sentences). [Francois DANIS, France]	Noted. Some simplification has been undertaken in response to this and other comments.
2-1225	2	30	45	30	45	replace "taken by ships" with "measured on board ships" [Elizabeth Kent, England]	Accepted
2-1226	2	30	49	30	49	It is about time you got round to defining what you mean by "well mixed" There is consderable variabilty in properties of both the atmosphere and the ocean and some of these persist for long periods. We have the doldrums and the trade winds. We have the gulf stream and the Sargasso sea. Are you trying to argue that this variability can be ignored? [VINCENT GRAY, NEW ZEALAND]	Taken into account. See response to 2-1227
2-1227	2	30	49	30	49	"Both the atmosphere and ocean are well mixed…" Really? I thought that they are both stratified in terms of temperature, especially the oceans. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted. Will use different terminology.
2-1228	2	30	52	30	52	replace hyphens with commas [Peter Burt, UK]	Editorial
2-1229	2	30	53	30	54	Chapter 3 has the amount of energy absorbed by the oceans at 90% in the last 40 years. It might be a good idea to make the statement in the FAQ consistent with the Chapter 3 statement. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Changed to 90%. We have worked with Chapter 3 Lead Authors to ensure that they are happy with the SOD version text in this regard.
2-1230	2	30	53	30	54	"More than 80% of the energy absorbed by the climate system since the 1960s has been stored in the oceans". Is there a reference for this? [Norman Loeb, United States of America]	Noted. The evidence is discussed in Chapter 3. FAQS need to avoid references according to the editorial

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							guidance provided. See also response to 2-1229.
2-1231	2	30				FAQ 2.1: Opening line to the chapeau would be more compelling if a time-frame was added to this statement. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Added since the nineteenth Century here.
2-1232	2	30				FAQ 2.1: Page 31, lines 8 - 13. Care is needed in linking changes in temperature to observed changes that in fact are not solely linked to temperature. The role of precipitation changes for example could easily be overlooked here. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. On a global scale temperature is likely to dominate. The current text does not imply that the sole determinant is temperatures. It is hard to see how to work in a precipitation nuance without making this much less readable / accessible.
2-1233	2	30				FAQ 2.1, Fig 1: Add quantitative information to the figure, i.e.., add numbers to the arrows. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. The quantitative information is in the Figure 2.2 and adding it here makes this figure very messy and much less readable and understandable. We tried this route in very early versions of the figure and decided it unworkable. The value is actually in having two distinct figures that complement each other and are understandable to different audience segments.
2-1234	2	30				FAQ 2.1, Fig 2: This will be a very compelling and important figure, but it must be robust. This is a particular challenge for an FAQ given that they are required to be stand-alone, so not reliant on citations to other material. The datasets used must therefore be listed, perhaps as a legend. Is there a possibility to show uncertainty in the various datasets, eg, by showing the common range in grey, with thin colored lines over top. What is the reference period associated with each dataset? [Thomas Stocker/ WGI TSU, Switzerland]	We will include information on the datasets used in each panel as a part of the Chapter appendix and an online URL where the necessary information can be laid out. This was agreed verbally with David Wratt who is leading the faqs. In each panel the datasets have been normalized to a common period. This will be made clear in the revised figure legend.
2-1235	2	31	1	31	1	I would suggest saying "A number of independently assessed records of rising sea-levels extend back more than a century" rather than 'several'. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. Text has been deleted to meet length guidelines.
2-1236	2	31	4	31	4	It would be helpful I think for the readership of the FAQs to have some explanation of why warmer air will on average contain more water [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted. Within the confines of the FAQ we cannot see a way to do this so no changes have been made. Most people know that it is more humid in summer than winter even if they do not know why.
2-1237	2	31	4	31	4	Take care when describing humidity – I would use 'moister' rather than 'wetter' because 'wetter' may imply increasing rainfall and so the statement 'A warmer world is also a wetter one..' is misleading as there are certainly regions where temperature is rising but rainfall is not, or at least the rainfall signal is not with much confidence. [Kate Willett, UK]	Accepted
2-1238	2	31	4	31	5	The connection between the two sentences could be more clear if we substitute the words "water" with "condensed water vapor" and "moisture" with "water vapor". [Claudio Cassardo, Italy]	Accepted. The wording has been changed along the lines suggested by the reviewer.
2-1239	2	31	4	31	6	Theoretically this is so; however as you show elsewhere in this chapter, there is no trend yet in global precipitation in the last century, although we had warming. [Marcel Crok, The Netherlands]	Rejected. The discussion here is not about precipitation.
2-1240	2	31	4	31	6	Needs a reference. [Karen Rosenlof, United States of America]	Editorial. We cannot include references in FAQs.
2-1241	2	31	5	31	24	You should mention the period every time. Many measurements are only recent and they may be transient. Or periodic. [VINCENT GRAY, NEW ZEALAND]	Rejected. The periods are intrinsically obvious from the figure and such mentioning every time would break any flow.
2-1242	2	31	8	31	8	replace hyphens with commas [Peter Burt, UK]	Editorial.
2-1243	2	31	8	31	13	Is there a peer-reviewed basis for the statement that snow cover is particularly sensitive to temperature in spring? If so cite it. Even if there is a basis for this, it is nevertheless important to state what the snow cover trends are in the other seasons and in the Southern Hemisphere. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. Explict justification has been added.

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2-1244	2	31	9	31	10	Statement "The mass … years" not clear. [Christian-D. Schoenwiese, Germany]	Accepted. We have changed to be explicit about ice.
2-1245	2	31	12	31	12	northern hemisphere → Northern Hemisphere [Peter Burt, UK]	Accepted
2-1246	2	31	13	31	13	There has been a significant rise in Antarctic sea ice, with speculation that this may be caused by freshwater from land ice melting (Bintanja et al, 2012, submitted). [Geert Jan van Oldenborgh, Netherlands]	Noted. Added caveat about comparatively little change here.
2-1247	2	31	13	31	16	Lack of reduction in Antarctic sea ice may not appear to the reader to point to a warming world, at least not without some explanatin of why the expectation would not be for reducing Antarctic sea ice under observed forcing changes. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted. We have cross-checked with Chapter 4 CLAs the wording ehre and they have noted that they are happy with the wording as being put forward in the revised version.
2-1248	2	31	17	31	17	"warmer air" [George Kiladis, USA]	Editorial
2-1249	2	31	17	31	18	Since the stratosphere is not warming, and is inside the "edge of the atmosphere", the statement is misleading. Actually, the warming extends only to somewhere in the upper troposphere. [Melissa Free, USA]	Accepted
2-1250	2	31	17			Actually to top of troposphere for warming, not the "edge of the atmosphere" [Kevin Trenberth, USA]	Taken into account. See response to 2-1250
2-1251	2	31	18	31	18	Be careful about using just 'humidity' – its better to be clear that you mean absolute humidity as opposed to relative humidity by using 'water vapour', 'moisture' or the measured quantity such as 'specific humidity'. [Kate Willett, UK]	Accepted
2-1252	2	31	20	31	24	The word "redundant" is not needed, or appropriate. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1253	2	31	20	31	24	FAQ 2.1 Figure 2: BAMS State of the Climate 2009 citation – this should be Kennedy et al. 2010 - Kennedy, J. J., P. W. Thorne, T. C. Peterson, R. A. Ruedy, P. A. Stott, D. E. Parker, S. A. Good, H. A. Titchner and K. M. Willett, 2010: [Global Climate] How do we know the world has warmed? [in .State of the Climate in 2009.]. Bull. Amer. Meteor. Soc., 91 (7), S26-S27. [Kate Willett, UK]	Accepted
2-1254	2	31	29	31	41	In Chapter 2, "hydrological" appears 31 times and "hydrologic" 6 times. Both "hydrological" and "hydrologic" are used together here. The preferred term is "hydrologic". [Forrest Mims, USA]	Dictionary has both as equivalent with no preference given, choice is author's, but for consistency change to hydrological.
2-1255	2	31	29			In section 2.3 Changes in Hydrological Cycle, groundwater is not mentioned. Groundwater, as an important part of terrestrial hydrological cycle and important natural resources, should be mentioned in this section. [Jianting Cao, China]	Groundwater is important, it is assessed in WGII.
2-1256	2	31	29			This assessment of changes in the hydrological cycle is problematic will out an explicit discussion of the sensitivity of trend analysis to length and timing of the observational record. As stated or implied throughout this section, the hydrological cycle is characterized by strong decadal to multi-decadal variability, especially the variables streamflow and runoff where there is storage, persistent and serial autocorrelation. As a result, some detected trends could be an artifact of this low-frequency variability. See St. Jacques, et al. 2010. Geophysical Research Letters, Vol. 37, L06407, doi:10.1029/2009GL042045, 2010. [David Sauchyn, Canada]	Thanks for the comment. Yes it is characterized by strong variability, but an explicit discussion of trend sensitivity is included elsewhere in the chapter and not repeated here due to length constraints.
2-1257	2	31	31	31	34	Climate change is mostly hydrologic change, and the changes in the hydrologic cycle are the primary changes of consequence. Water is the dominant greenhouse gas in the atmosphere, and the one that will dominate the warming as a feedback from the CO2 driven initiation. Water moves large amounts of heat around in the form of latent heat, which is rarely discussed in the context of "temperature trends". Clouds of condensed water vapor are one potential feedback processes relative to the global energy balance. Freshwater resource availabllity will be (and I would say already is) the most critical economic and ecological shift occuring. Please give the role of the hydrological process its due place at the table here ... the climatologists are just along for the ride. It's all about water. Just because we have more "certainty" about temperature does not mean that is what we should all be talking about first...besides regional temperature trends are strongly influenced by the local hydrologic trends! [Charles Luce, United States of America]	Changes in the hydro cycle are important, although I would disagree that its all about the hydro cycle. We assess what is available in the literature and more has been written about temperature than water.
2-1258	2	31	33	31	34	Replace "atmospheric water vapour impact both the energy balance, as water vapour is one of the most	agree, makes it read better.

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						abundant greenhouse gases, and the hydrologic cycle." with "atmospheric water vapour impact both the the hydrologic cycle and the energy balance, as water vapour is one of the most abundant greenhouse gases." [Robert Waterland, United States of America]	
2-1259	2	31	34	31	34	hydrologic → hydrological [Peter Burt, UK]	done.
2-1260	2	31	36	31	36	I am not clear where satellite estimates of precipitation from 1979 comes from. Reliable satellite estimates were only available when SSM/I was launched in 1987. The use of the geostationary satellites with Vis/IR channels is quite crude. [Roger Saunders, United Kingdom]	GPCP is a combined GOES, POES IR and MW data set that also merges gauge data. Starts in 1979.
2-1261	2	31	45			I was surprised in this section (2.3.1.1) on precipitation not to see any mention of the satellite passive microwave precipitation trends claimed in Wentz et al. 2007 of increasing ocean precipitation from 1987-2006. This was very confusing to the research community and while I don't believe it, at least some mention of why it is or is not included should be provided. Note that Wentz paper is discussed in chapter 2, page 63, line 5-6: but not here: inconsistent use. [Bruce Wielicki, USA]	Added one sentence on the Wentz findings at te end of 2.3.1.1.
2-1262	2	31	47	31	57	For the evaluation of precipitation trends, a time period of 26 years is too short. Owing to the bad statistics of precipitation data, a time period of 50 years in minimum is required to obtain at least somewhat reliable trends. I suggest to shorten this paragraph and to omit most of the lines 49 to 53, from "The changes in...." to "..."(2) trends". It's reading the tea leaves. [Sabine Wurzler, Germany]	Thank you for the comment but we disagree with your contention about 26 years being too short. Precipitation is noisy, but these were included since they are mainly satellite based.
2-1263	2	31	47	32	38	My main issue with this section is that it does not allow for hypothesis testing of whether observed changes in global precipitation per degree change in global temperature are consistent with projections from global climate models (which I think are in the order of a couple of percent per degree). To do this it would be necessary to work out the best estimate of the percentage change from each of the datasets, together with estimates of the confidence intervals. I would expect that, given the uncertainty in global precip measurements, the confidence intervals would be wide relative to the expected changes due to the change in temperatures thus far, but would be good to test this formally. [Seth Westra, Australia]	This is beyond what is in an assessment. See Wentz et al. (2007) for this kind of analysis.
2-1264	2	31	47	32	44	In discussion of data gaps, the lack of precipitation data, particularly long term precipitation data, and the difficulty estimating it based on remote sensing should be discussed. Because of orographic effects mountains have generally higher precipitation than surrounding areas - at least the upwind sides. In some regions, mountains are effectively the "water towers" yet we have little data about precipitation trends in these areas except for streamflow measurements. [Charles Luce, United States of America]	Much of this information is contained in the AR4 and has not been repeated here due to page length constraints.
2-1265	2	31	50	31	51	There is a missing trend turn-around date in this sentence. When did the increase in the early 20th Century end? [Dian Seidel, USA]	added 1950 date.
2-1266	2	31	51	31	51	Futher' should be 'Further' [Zhaomin Wang, UK]	done.
2-1267	2	31	51	31	51	Replace "Futher" with "Further". [Robert Waterland, United States of America]	done.
2-1268	2	31	51	31	51	Typo: Instead of "Futher" it should be "Further" [Sabine Wurzler, Germany]	done.
2-1269	2	31	53	31	54	This states that confidence in global-scale precipitation estimates is low because of incomplete data. Isn't it really the case that the confidence in the long-term *trends* of large-scale (global-scale) mean precipitation is low, because those trends are small relative to the local-scale, shorter term variability that is being observed? i.e. if there were large global-scale trends, we would probably see them in the datasets, even though the datasets do have their limitations. By saying that there is low confidence in the global estimates, it implies that there could have been some big trends but we couldn't observe them. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	This is true, there could be real long term trends, but due to poor spatial sampling early in the record the increase from 1900-1950 could be real or an artifact of the poor spatial sampling. The reconstructed precipitation data set by Smith shows no long term trend, again could be an artifact of the reconstruction technique, or real.
2-1270	2	31	53	31	54	"confidence in precipitation estimate is low" is confusing. Do you mean annual mean of global average precipitation since 1900, do you mean data not good enough for trend estimate? Was AR4 confident enough to estimate? [Xuebin Zhang, Canada]	Yes, this is global mean annual and the AR4 said pretty much the same thing.
2-1271	2	31	55	31	57	Need references for this statement [Bruce Wielicki, USA]	Based on Figure 2.16, added to text.

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2-1272	2	31	56	31	56	"show little change", note that global total precip is not expected to have had a great change for about 1C warming. [Xuebin Zhang, Canada]	Thanks, noted in text.
2-1273	2	31	56	31	57	The text 'the resulting time series shows little changes in land-based precipitation since 1900.' is repeated on page 2-32, lines 30-31. [Kate Willett, UK]	Thanks the whole paragraph on p. 32 lines 28-31 was redundant and removed.
2-1274	2	31				section 2.3.1. This is supposed to deal with changes in precipitation but it falls short in many ways. Precipitation is intermittent and this deals only with amount, not intensity or frequency. The framing of it also falls short. What should be expected with precipitation changes? Certainly not global mean uniform increases. The nature of the analysis and many fields presented make no sense. There is a comprehensive review of mine that deals with this topic and it is not referred to but I beleive should help this section. Please see Trenberth, K. E., 2011: Changes in precipitation with climate change. Climate Research, 47, 123-138, doi:10.3354/cr00953. (available from my web site) [Kevin Trenberth, USA]	Spatial variability of changes is contained in section 2.3.1.2. This is the only review that takes issue with the presentation. Consideration will be made in editing after reviewing the reference in the comment.
2-1275	2	31				focus on global land like this does not make much sense. Confidence in some regional changes is good. One certainly expects regional changes of different sign and any global mean changes should be a small residual. Absence of big chunks of Africa and S America make this quite unsatisfactory. The reconstruction techniques p 32 l 14 can not possibly get intensity and frequency right. Precip is not a continuous variable. Satellite data do not provide "true" (p 32 l 26) global coverage as their temporal sampling is lacking. Wasn't 2010 the wettest on record? Fig 2.16: isn't that worth a comment? Here again more of an assessment is needed. [Kevin Trenberth, USA]	Added words about 2010, and the word "true" has been changed to full.
2-1276	2	32	2	32	3	It reads strange to refer to a paper from 1992 using an updated dataset through 2010. Perhaps the reference just needs to be moved to after the GHCN data set (Vose ..) [Lena M. Tallaksen, Norway]	This is the original reference and there is no new one for the precipitation part of GHCN.
2-1277	2	32	3			"using the GHCN data set updated through 2010 (Vose et al., 1992)." Reference? Vose 1992 could not have updated anything to 2010! [Philip Lloyd, South Africa]	This is the original reference. The precipitation data set has been regularly updated but no new reference.
2-1278	2	32	5	32	5	delete '(' before Adler. [Zhaomin Wang, UK]	agree, makes it read better.
2-1279	2	32	6	32	6	The word 'true' is misleading here. 'complete' or 'full' would be better. [Kate Willett, UK]	change to full
2-1280	2	32	6	32	7	The sentence “One new global …” is awkward. [David Sauchyn, Canada]	changed to A
2-1281	2	32	6	32	17	the description of reconstructed data is out of proportion. If you do have so much the space, it would be better to describe performance/uncertainty of the reconstructed data. Reconstruction by itself does not increase information already available from the source data. [Xuebin Zhang, Canada]	agree, reduced discussion.
2-1282	2	32	8			It is a little awkward to reference submitted papers since it is unavailabel to me. What happens if this paper is not accepted in a timely fashion. This happens in a number of places throughout this chapter. [Larry Thomason, United States of America]	Papers are available on the IPCC web site. If a paper is not accepted by the cutoff date the text will be removed.
2-1283	2	32	25	32	25	Smith → Smith et al. [Peter Burt, UK]	removed paragraph.
2-1284	2	32	25	32	31	I don't think the data presented support such a strong statement. I would say that data sets suggest a century-scale increase in global precipitation: there was a substantial increase in the early to mid Century and a pronounced 20-year drying trend from the mid-1970s to the mid-1990s. [Robert Waterland, United States of America]	We have added a statement about a lack of confidence in the increase from 1901-1950 or so. There is a spatial sampling problem leading to less confidence.
2-1285	2	32	26	32	26	In my opinion the statement that for rainfall "satellite-based data sets provide true global coverage" is in some way questionable. A brief discussion about the limits and the uncertainties of the satellites rainfall estimation is highly desirable. A discussion about the problem of comparing rain gauges measurements and satellite estimation is desirable too, especially when looking for weak trends, assembling long time series. [Walter Dragoni, Italy]	this statement has been modified and including discussion of uncertainties is beyond the scope here due to space limitations.
2-1286	2	32	26	32	26	"satellite-based data sets provide true global coverage", however, satellite-based data are not reliable enough for trend analysis over the region north of 40N or south of 40S. [Xuebin Zhang, Canada]	Not sure this is correct. Polar orbiters are reliable at high latitudes, geostationary probably not so reliable.

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2-1287	2	32	27	32	29	Is the trend significant? It does not look so in the figure? [Uwe Stoeber, Germany]	no and no trend is presented, only qualitative.
2-1288	2	32	27			Where is CRU in Figure 2.16? [Uwe Stoeber, Germany]	CRU data are now included in Figure 2.28
2-1289	2	32	29	32	29	Smith → Smith et al. [Peter Burt, UK]	removed paragraph.
2-1290	2	32	30	32	31	It is unclear if the lack of trend is due to artifact of the reconstructed data or if it truly reflect what would have been in the global precipitation. [Xuebin Zhang, Canada]	This is true, its could be a combination however the same methodology for temperature does show trend.
2-1291	2	32	33	32	34	I suggest to omit the first sentence of this paragraph. The time period considered is much too short to say anything reliable. It is reading the tea leaves. [Sabine Wurzler, Germany]	Thanks but we disagree, see previous answer to your same suggestion.
2-1292	2	32	33	32	38	The Smith et al. curves don't show any trend even for the latitude bands. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Indeed that is true.
2-1293	2	32	33		38	Here the "tropics" is presented but this is NOT the tropics and the region 30N to 30S is not homogeneous: it includes the deep tropics where the monsoon trough is expected to increase in precip and the subtropics where it is expected to decrease. This discrimiation must be made. [Kevin Trenberth, USA]	There are numerous ways to define the tropics, the monsoon trough being only one. We choose to keep the definition at 30 N and S.
2-1294	2	32	40	32	43	What is the time period of that increase? It is confusing if in ll. 33-34 a drying trend is described and now increased precipitation is discussed. What happende when? [Uwe Stoeber, Germany]	added time periods into text.
2-1295	2	32	40	32	44	Climate zones are mentioned here. In the Mediterranean area, dry climate zone expanded by more than half from the period 1950-78 to 1979-2006. See 5. Jylhä, K., Tuomenvirta, H., Ruosteenoja, K., Niemi-Hugaerts, H., Keisu, K. and Karhu, J.A., 2010. Observed and projected future shifts of climatic zones in Europe, and their use to visualize climate change information. Weather, Climate, and Society, 2:2, 148-67. And references therein. http://journals.ametsoc.org/doi/abs/10.1175/2010WCAS1010.1 [Kirsti Jylhä, Finland]	Thanks for the reference, but adding is beyond the scope of this section.
2-1296	2	32	40	32	44	Should this paragraph be moved into Section 2.3.1.2 since it addresses spatial patterns? And does the final sentence, linking precipitation and humidity changes, refer to results from the studies cited earlier in the paragraph, or is this a result of the overall chapter assessment? [Dian Seidel, USA]	Since these are zonal time series we prefer to leave them where they are. Sentence about humidity is a link to studies cited earlier.
2-1297	2	32	40	33	19	Firstly, the last paragraph of S2.3.1.1 seems better placed in the spatial variability section? My main comment however is that I would like to see a global map in which the climate zones are demarcated, and trends in precipitation (like Figure 2.16) are estimated for each of these key climate zones. This would provide a nice visual assessment of whether the the dry regions of the sub-tropics are changing differently to the wet regions in the tropics and northern hemisphere mid-latitudes. Obviously there will be some subjectivity about demarcation, but it is important to show nonetheless. [Seth Westra, Australia]	See answer to 1296. Which climate zones? Not clear. The latitudinal bands or some other zones?
2-1298	2	32	41	32	53	These two paragraphs are confusing. [Michel Boko, Benin]	One discusses the time series for each band, the other the grid box trends, not sure why they are confusing.
2-1299	2	32	43	32	44	More discussion on the complexity of the humidity-precip relationship would be useful here as this sentence implies that there is a straightforward response to increasing tropospheric humidity. Is it appropriate here to talk about the differential increases in heavy precipitation (over oceans at least) that are close to clausius-clapeyron scaling (Allan and Soden 2010) verses much smaller (2-3% per K) changes in the mean? Some brief discussion of why the relationship between temperature and precip or humidity and precip is so complex would be useful for the reader. [Kate Willett, UK]	We will consider adding some text but must consider space constraints as to whether we add much.
2-1300	2	32	44	32	44	Insert "specific" before "humidity". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Done.
2-1301	2	32	46			why only GHCN? Why not the new GPCC material (which is much better than at time of AR4)? [Kevin Trenberth, USA]	trend maps are now included for all 4 major data sets.
2-1302	2	32	48	32	48	In this context it should be realized that different trend signs in different seasons may weaken or even compensate the annual data trends. For example, in Germany winter precipitation 1901-2000 has increased whereas summer precipitation has decreased; see reference given in comment to capter 2, page 8, lines 29-	Thanks for the comment, no action requested.

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						36. This behaviour holds also for other countries in Central Europe. [Christian-D. Schoenwiese, Germany]	
2-1303	2	32	49	32	50	How many is "quite a number"? [Dian Seidel, USA]	This text is changed in light of including maps for all 4 data sets.
2-1304	2	32	51	32	52	Why is the significant drying trend over the West Africa coast not mentuoned? [Geert Jan van Oldenborgh, Netherlands]	This text is changed in light of including maps for all 4 data sets.
2-1305	2	32	51	32	52	Why are the eastern and northwestern America trends mentioned when later it is conceded that these re anot significant? [Geert Jan van Oldenborgh, Netherlands]	This text is changed in light of including maps for all 4 data sets.
2-1306	2	32	55	33	8	This is an interesting paragraph. It seems not to rest of recent peer-reviewed literature but on analysis made particularly for the AR5, assessing precipitation trends for the same time periods as was done for temperature. That is a good idea. But the interpretation part of the paragraph seems a little shakier, and I'd suggest avoiding speculation that does not have a foundation in the literature. [Dian Seidel, USA]	This text is changed in light of including maps for all 4 data sets.
2-1307	2	32	55	33	8	What is the implication of station density change, especially sharp drop of spatial coverage since 2000 and since 2005 in particular, on the spatial variability of observed trends? [Xuebin Zhang, Canada]	This is a good and relevant question, but without an analysis it is hard to say whether the drop off of stations in recent years has a large impact. This does lead to greater uncertainty in recent years. Including trend maps for GPCC helps answer this question.
2-1308	2	33	1	22	9	What are the reasons for the changes from the AR4 analysis? Is it just an extension of the time series, or are different datasets used? Can you state whether the issue is actually that a simple linear fit is a poor representation of the data? [Karen Rosenlof, United States of America]	Due to data since 2006 being included plus additional data sets.
2-1309	2	33	1	33	1	hemispheres → Hemispheres [Peter Burt, UK]	correct, done.
2-1310	2	33	1	33	8	These apparently reversing and opposing trends likely are a function of low-frequency variability in the time series (see the previous comment re entire section 2.3). [David Sauchyn, Canada]	Yes that is probably correct.
2-1311	2	33	1		19	For precip the use of linear trends is much more problematic than for temperature, owing to the large circulation changes and their effects which are more apt to produce a step function like change. Such is the case for the US, for instance. The analysis as presented is not very useful and fails to deal with the changes over time well that are not linear. [Kevin Trenberth, USA]	Yes, precipitation tends is more problematic than linear trends for temperature. However there is interest in what the linear trends are for large area precipitation time series.
2-1312	2	33	4	33	6	Are there any references for the sentence beginning "Explanations for fewer specific" or is this speculation? A reference would add validity. [Michael Brewer, United States of America]	No this is based on examining time series in Fig 2.16.
2-1313	2	33	4	33	6	The author states that "Explanations for fewer significant trends is probably due to areas that are getting wetter over the long-term, experiencing drought in recent years, and vice versa.", yet the note in the associated figure caption raises concerns about precisely what method was used to assess trends. How do we know that isn't the reason? Did the author apply the same method to the shorter series used in Trenberth et al (2007) to insure that it yields the same conclusions as AR4. That is required before any statement can be made about the cause of the different conclusions about precip trends (i.e. whether it is the time interval or the method that is responsible for the different conclusion). It is important to get this right. [Michael Mann, USA]	This is based on examining the time series in Fig. 2.16, no trend analysis was performed but looking at the time series, esp. since 2005 compared to earlier in the record.
2-1314	2	33	8	33	8	Also in southern South America there are opposite trends between the 1901–2010 period and 1979–2010 period. [Alice Grimm, Brazil]	Yes, we will acknowledge that.
2-1315	2	33	14			For potential regional assessments of precipitation trends, please see 1) Kilpeläinen, T., Tuomenvirta, H. & Jylhä, K. 2008: Climatological characteristics of summer precipitation in Helsinki during the period 1951–2000. Boreal Env. Res. 13: 67-80. borenv.net/BER/pdfs/ber13/ber13-067.pdf 2) Ylhäisi, J.S., Tietäväinen, H., Peltonen-Sainio, P., Venäläinen, A., Eklund, J., Räisänen, J. and Jylhä, K., 2010. Growing season precipitation in Finland under recent and projected climate. Nat. Hazards Earth Syst. Sci., doi:10.5194/nhess-10-1563-2010. [Kirsti Jylhä, Finland]	Regional is in Chapt. 14, Chapt. 2 is more global.
2-1316	2	33	16	33	18	In the Mediterranean, just in south Italy, fig. 1.17 gives a rainfall increase of 0-15% for decades. As far as I know, for central and southern Italy, the literature gives a decreasing trend. I suggest considering this issue	Regional is in Chapt. 14, Chapt. 2 is more global.

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						after having a look to some of the following papers: //////D. Ducci and Tranfaglia G. (2008): Effects of climate change on groundwater resources in Campania (southern Italy). Geological Society, London, Special Publications, 2008; 288: 25 - 38.;////// M. Polemio and D. Casarano (2008): Climate change, drought and groundwater availability in southern Italy. Geological Society, London, Special Publications, 2008; 288: 39 - 51.////Dragoni W. (1998): Some considerations on climatic changes, water resources and water needs in the Italian region south of the 43°N. In "Water, Environment and Society in Times of Climatic Change". Issar A., Brown N. editors. Kluwer, (Water Science and Technology Library), pp. 241 - 271. ///// Brunetti & Others (2006): Temperature and precipitation ... in Italy. International Journal of Climatology.. [Walter Dragoni, Italy]	
2-1317	2	33	21	33	56	I think this section has the wrong title. It deals almost entirely with changes in the frequency of snowfall events, not changes in snowfall amounts. The title doesn't capture that idea. [Robert Waterland, United States of America]	Not true, much of this section is about snowfall changes.
2-1318	2	33	24	33	24	insert 'that' after 'were' [Peter Burt, UK]	Accepted.
2-1319	2	33	26	33	26	insert 'the' after 2nd 'in' [Peter Burt, UK]	Accepted.
2-1320	2	33	28	33	28	What is the reason for "Further" here? Can it be deleted? It's confusing. [Dian Seidel, USA]	Deleted.
2-1321	2	33	29	33	29	delete one full stop after 'Lakes' [Peter Burt, UK]	Accepted.
2-1322	2	33	30	33	30	I suggest adding the following after "…observed": "especially over the oceans." [Martin Hovland, Norway]	Reject, no citation for this is provided and not clear if this is true.
2-1323	2	33	32	33	39	Christy 2012 (J Hydromet) shows no changes in 133 years and last 50 years of California snowfall - a critical factor in California's economy. [John Christy, USA]	Thanks, regional is in Chapter 14.
2-1324	2	33	35	33	35	insert comma after 'mainly' [Peter Burt, UK]	Accepted.
2-1325	2	33	37	33	39	Kunkel 2009 was cited for observing an overall trend of decreased snow in the western US, NE, and southern margins. By how much and since when? Are there more recent citations on this phenomenon? [Beverly Law, USA]	No there are not more recent citations for this. Kunkel et al (2009) is only 3 years old. Also the amount of change varies across the region.
2-1326	2	33	41	33	46	See new paper by Cohen et al (2012) on increased area of snowfall in Eurasia [Katharine Law, France]	What journal? Is this in print or review?
2-1327	2	33	44	33	46	Please cite also the work of Scherrer and Appenzeller (2006) which shows that "the third pattern of snow pack variability over Swiss Alps is height dependent with a strong maximum at lowland stations and a minimum at high stations. " This pattern explains 10% of the total variance of this variable and "its time component shows a distinct trend. It is well correlated with the 0°C isotherm which increased from ~600 m in the 1960s to ~900 m ASL in the late 1990s and could be related to climate change." . S.C. Scherrer and C. Appenzeller, 2006: Swiss Alpine snow pack variability: Major patterns and links to local climate and large-scale flow. Clim Res,32, 187-199. [Valentina Pavan, Italy]	Included a sentence and citation.
2-1328	2	33	46			Please add “Serquet et al. (2011) analyzed snowfall and rainfall days …association with increasing temperatures…” although both the new snow sums and day with snow pack showed a relative trend reversal in most recent years (since 2000), especially at low and medium altitudes (Scherrer et al., 2012). This illustrates how important decadal variability is in understanding regional trends in snow indicators. S. C. Scherrer, C. Wüthrich, M. Croci-Maspoli, R. Weingartner and C. Appenzeller; 2012: Snow variability in the Swiss Alps 1864-2009, Int.J.Clim. In review. [Christof Appenzeller, Switzerland]	Included earlier citation by Scherrer and Apenzeller since this one is not accepted yet.
2-1329	2	33	48	33	51	One more reason for the increased snow fall in Antarctica is that the snow crystal shapes are highy dependent on the temperatures and humidities during their formation and that you get at higher temperatures crystal shapes that can better conglomerate to snow flakes and sediment to the ground. See e.g. Pruppacher and Klett 1997, p.550 ff. and Pruppacher and Klett 1980, p. 32 Fig. 2-26. [Sabine Wurzler, Germany]	Thanks for the comment, no action requested.
2-1330	2	33	48	33	55	Antarctic snowfall change: Snowfall accumulation rate is shown in Fig. 1 of Shepherd and Wingham (2007).	Statement has been revised, see comment 1333

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						There is no increase over the past decade. This figure is redrawn from A. J. Monaghan et al., Science 313, 827 (2006), where the conclusion is “There has been no statistically significant change in snowfall since the 1950s”. However, there is a clear downward trend in the snowfall accumulation after the 1980s, as shown in this study. [Zhaomin Wang, UK]	response
2-1331	2	33	48	33	55	In van Ommen and Morgan (2010), precipitation data is given at one point (Law Dome) of the coastal region of East Antarctica, so no conclusion can be made for precipitation changes over Antarctica from this study. Even for this one-point data, there is a clear increase in precipitation after 1980, and after 1980, there is no clear evidence to support Antarctic warming (see below). [Zhaomin Wang, UK]	Statement has been revised,
2-1332	2	33	48	33	55	The results of Eric J. Steig et al. (Nature 2009, 457, 459-462) showed warming over Antarctica since 1957. This is questioned by an improved reconstruction method (O’Donnell, Ryan, Nicholas Lewis, Steve McIntyre, Jeff Condon, 2011: Improved Methods for PCA-Based Reconstructions: Case Study Using the Steig et al. (2009) Antarctic Temperature Reconstruction. J. Climate, 24, 2099–2115, doi: http://dx.doi.org/10.1175/2010JCLI3656.1). Fig. 3 of this paper compared the improved reconstructions with that of Steig et al. (Nature 2009), and clearly showed cooling over Atnarctica between 1979-2003. [Zhaomin Wang, UK]	Statement has been revised,
2-1333	2	33	49	33	51	The comment about van Ommen and Morgan is correct, that recent accumulation increase is detected in a large part of East Antarctica (also shown in Monaghan et al., Science, 2006). However, this should not be taken as synonymous with overall Antarctic, or even East Antarctic increases in accumulation. Nonetheless, the point is important, and van Ommen and Morgan (2010) note evidence that the increased meridional transport of moisture is likely to be anthropogenic (ozone related). The fact that this is some of the most northerly coastline in Antarctica makes the increase in accumulation also supportive of the view that is related to lower latitude warming. A few more words here would help the reader avoid the erroneous conclusion that van Ommen and Morgan's result applied to Antarctica as a whole. [Tasman van Ommen, Australia]	Added statement from Monaghan and Bromwich about all Antarctic snow accumulations showing increase to 1990 then decline to 2004.
2-1334	2	33	53	33	53	This summary statement about changes "in most regions analyzed" is misleading, because most regions appear NOT to have been analyzed. [Dian Seidel, USA]	Re-phrased to "in most analyzed regions".
2-1335	2	33	53	33	54	Increased winter temperatures are a necessary condition for a decreased number of snowfall events, but it is not sufficient. The temperatures must also approach of exceed zero degrees. [David Sauchyn, Canada]	Thank you it is actually more complicated.
2-1336	2	33	53	33	55	Number of snowfalls heavier than 2 mm per day, as well as snow amount, has increased over large part of Russia, along with rising of the air temperature. The paper on snow depth increase has been published in 2010 (Shmakin A.B., 2010. Climatic characteristics of snow cover over North Eurasia and their change during the last decades. "Ice and Snow", vol. 1, No. 1, pp. 43-57); a review of snow cover changes is published in 2011: Callaghan T.V., M. Johansson, R.D. Brown, P.Ya. Groisman, N. Labba, V. Radionov, R.S. Bradley, S. Blangy, O.N. Bulygina, T.R. Christensen, J.E. Colman, R.L.H. Essery, B.C. Forbes, M.C. Forchhammer, V.N. Golubev, R.E. Honrath, G.P. Juday, A.V. Meshcherskaya, G.K. Phoenix, J. Pomeroy, A. Rautio, D.A. Robinson, N.M. Schmidt, M.C. Serreze, V.P. Shevchenko, A.I. Shiklomanov, A.B. Shmakin, P. Sko¨ld, M. Sturm, M.-K. Woo, E.F. Wood. Multiple Effects of Changes in Arctic Snow Cover. AMBIO, Vol. 40 (Suppl. 1), pp. 32–45, DOI 10.1007/s13280-011-0213-x). The paper on the snowfall statistics in Russia is accepted and will be published in the first half of 2012 (Borzenkova A., Shmakin A. Change of snow cover depth and daily snowfall intensity resulting in expenses for snow removal from streets in Russian cities. "Ice and Snow", 2012, Vol.3, No.2). [Andrey Shmakin, Russia]	Thanks, noted in text.
2-1337	2	33	53	33	55	Number of snowfalls heavier than 2 mm per day, as well as snow amount, has increased over large part of Russia, along with rising of the air temperature. The paper on snow depth increase has been published in 2010 (Shmakin A.B., 2010. Climatic characteristics of snow cover over North Eurasia and their change during the last decades. "Ice and Snow", vol. 1, No. 1, pp. 43-57); a review of snow cover changes is published in 2011: Callaghan T.V., M. Johansson, R.D. Brown, P.Ya. Groisman, N. Labba, V. Radionov, R.S. Bradley, S. Blangy, O.N. Bulygina, T.R. Christensen, J.E. Colman, R.L.H. Essery, B.C. Forbes, M.C. Forchhammer, V.N. Golubev, R.E. Honrath, G.P. Juday, A.V. Meshcherskaya, G.K. Phoenix, J. Pomeroy, A. Rautio, D.A. Robinson, N.M. Schmidt, M.C. Serreze, V.P. Shevchenko, A.I. Shiklomanov, A.B. Shmakin, P. Sko¨ld, M.	Repeat of previous comment.

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						Sturm, M.-K. Woo, E.F. Wood. Multiple Effects of Changes in Arctic Snow Cover. AMBIO, Vol. 40 (Suppl. 1), pp. 32–45, DOI 10.1007/s13280-011-0213-x). The paper on the snowfall statistics in Russia is accepted and will be published in the first half of 2012 (Borzenkova A., Shmakin A. Change of snow cover depth and daily snowfall intensity resulting in expenses for snow removal from streets in Russian cities. "Ice and Snow", 2012, Vol.3, No.2). [Andrey Shmakin, Russia]	
2-1338	2	33	54	33	55	"Antarctica is the exception where increased snowfall is occurring with increased temperatures." This is a surprising statement, because nowhere in the earlier sections on temperature changes was there any suggestion that Antarctica was warming. For consistency, it seems advisable to omit this statement, or perhaps refer to higher humidity rather than higher temperatures. [Philip Lloyd, South Africa]	revised statement to indicate no real change in Antarctic snowfall.
2-1339	2	33	54	33	55	In summary, there is no evidence for "increased snowfall is occurring with increased temperature over Antarctica". [Zhaomin Wang, UK]	this has been changed.
2-1340	2	33	54	33	55	Is there any explanation of why Antarctica sees increasing snowfall when other regions do not? This seems to be left very open. It might be worth saying that there has been limited research in this area if this is so. [Kate Willett, UK]	Yes, research is limited and will be indicated as such.
2-1341	2	33	55	33	55	Regarding your comment about increasing temperatures in Antarctica, should we consider that Figure 2.13 is the evidence for these increased temperatures?. Though hard to tell, it seems that some portions are warming while others are cooling. [Celeste Saulo, Argentina]	revised statement to indicate no real change in Antarctic snowfall.
2-1342	2	33				2.3.1.3 Here the framing is lacking. One expects that with warming the snow season will be shorter, but also that in mid winter the snow amounts will be more. The material presented here fails to bring out these aspects whether true or not. For example in 2010 over the US the heavy snows that led in part to the flooding of the Missouri were surely expected even as the snow season gets shorter? The capacity for big snowstorms in the east US is surely a part of global warming as long as temperatures over land are low enough because of increased moisture supply, as will happen in winter half year. The conclusions line 53-55 need major revisions. [Kevin Trenberth, USA]	We are assessing the literature and if it is not in the literature we cannot frame it in this way. Why necessarily would we have expected the 2010 heavy snows? Natural variability still plays a large role.
2-1343	2	34	1	34	35	in 2.3.2 Streamflow and Runoff, it should be pointed out that runoff and discharge is a different concept, the main material to conclude is from Dai et al, who is discuss the discharges. It is better to provide the conclusion of the runoff changes in main continents, or different latitude belts. [Jianting Cao, China]	Taking into account. The liinkage between runoff and discharge is adressed.
2-1344	2	34	1	34	35	The following work is worth being considered and mentioned: http://orbi.ulg.ac.be/handle/2268/66197 [Benjamin Dewals, Belgium]	Nothed.
2-1345	2	34	1	34	35	As this section mainly refers to annual runoff from very large river basins, even if not impacted by human influences, reagonal and seasonal changes will not be detected. Referring to studies where smaller pristine river basins are included and seasonal or extreme river flow is studied would make it possible to discuss e.g. the impact of temperature increase on river flow (see references in comment No. 2). If it is not within the scope or capacity of WGI to describe the effect of changes in climate on hydrology - or only at the global and annual scale, this needs to be clearly stated both in chapter 1 and throughout the report. It is then anticipated that the topic will be reflected in the report of WGII. [Hege Hisdal, Norway]	Take into accout. Since more detail climatic change impact on runoff is discussed in WG II, here we foucs on some important lager sclae changes. In addition, the pointed limitations are included in the moedifed text.
2-1346	2	34	3	34	12	This repetition of AR4's findings is perhaps unnecessary. It gives the impression that AR4's findings are about to be confirmed, which is, of course, not true. An introduction along the lines of "Further studies since AR4 have failed to confirm many of the conclusions reached there. In particular, AR4 concluded that runoff and river discharge generally increased at high latitudes, with some exceptions. Based on newer evidence, this conclusion no longer holds," is probably more appropriate. [Philip Lloyd, South Africa]	accepted. That part for AR4's results is removed.
2-1347	2	34	3	34	12	This paragraph is problematic. It reports a change in results from AR4 to AR5; however, there are no references (see next comment). Therefore, how did the authors reach this conclusion? Was it based on one study or more than one? [David Sauchyn, Canada]	accepted. That part for AR4's results is removed.
2-1348	2	34	3	34	12	Given the complete absence of references in the paragraph, I suggest that you provide sources, including St. Jacques, et al. 2009. Geophysical Research Letters, 36, 1401, doi:10.1029/2008GL035822 [David Sauchyn, Canada]	Accepted. This reference is included.

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2-1349	2	34	3	34	35	This section on streamflow trends focuses primarily on the mean, and on one study. It should be broadened to include other regional studies - there are a great many in Europe, for example, and the Luce and Holden, 2009 paper cited above in the Pacific Northwest. The magnitude and percentage changes cited in these other studies are quite large! [Charles Luce, United States of America]	Taking in to account. The continntal studies are included in the new text.
2-1350	2	34	3	34	35	Section 2.3.2 (Streamflow and Runoff) mainly includes references to trend studies in (annual) runoff from continental-scale river basins, which as commented on, will be largely impacted by human influences. This includes land use changes, reservoirs and abstractions, which may modify the annual water balance components. In addition, large river basins commonly integrate across a range of hydroclimatological regime from mountainous headwaters to lowland regions, making it difficult to detect regional patterns in runoff trends as processes with opposing hydrological influences may act simultaneously. Large river basin may also include regions with contrasting trends in climate variables like precipitation. The focus on trend studies from large river basins (both in AR4 and AR5) may therefore be of limited value in terms of their suitability to detect regional changes in climate forcings (introduced for the purpose of estimate the continental freshwater fluxes to the oceans). [Lena M. Tallaksen, Norway]	Take into accout. Limitations are menthioned.
2-1351	2	34	3	34	35	Further, focus is on trends in annual runoff. It is suggested that studies on regional and seasonal runoff trends are added as they provide valuable information on the cause of the changes in annual values, and allow seasonal changes from e.g. snow dominating regions to be addressed. Trend analyses on the seasonal or monthly time scale can not be performed using heavily modified river basins due to their altered hydrological regimes. [Lena M. Tallaksen, Norway]	taking into account. Refereces on large scale changes are cited for various regions. Meaningfaul seasonal trends are mentioned.
2-1352	2	34	3	34	35	In the following, the results of two recent papers (Stahl et al., 2010; 2012) addressing spatial patterns in continental-scale runoff trends in Europe (annual, monthly, high and low flow) are referred to. Both studies investigate streamflow trends based on a benchmark dataset of near-natural streamflow records from more than 400 small catchments in 15 countries across Europe for the period 1962–2004. Similar datasets exists for other regions like the USA, e.g. Krakauer and Fung (2008) for climate change attribution studies and Burn et al. (2010) for studies of trends in extremes in Canada (references given below). The European records represent near-natural river flow regimes from catchments with different hydrological characteristics. Thus, they provide a basis for investigating the predominant climate and catchment processes that govern changes in regional hydrology. A further advantage of such networks is that the gauged catchments are typically small, by virtue of the need to minimize the impact of human disturbance. [Lena M. Tallaksen, Norway]	accepted
2-1353	2	34	3	34	35	Stahl et al. (2010) elucidated spatial patterns and regional variability of streamflow trends that give rise to some important conclusions regarding the sensitivity of regional hydrology across Europe to changing temperature and precipitation patterns. In particular, it was found that annual and seasonal trends need to be carefully distinguished. A regionally coherent picture of annual streamflow trends was found for Europe during 1962-2004, with negative trends in south-eastern Europe, and generally positive trends elsewhere. In many European regions, annual streamflow trends appear to reflect wetting trends of the winter months, including a consistent increase of winter low flows. Strong trends of decreasing streamflow were not only found in south-eastern Europe, but were also found to be widespread across Europe in spring and summer months. The study largely confirms findings from national and regional scale trend analyses, but clearly adds to these by confirming that these tendencies are part of coherent patterns of change covering a much larger region. [Lena M. Tallaksen, Norway]	Accepted. Results are cited.
2-1354	2	34	4	34	12	This paragraph is hard to digest and seems to contain some inconsistent statements. In particular, the sentence starting on line 7 about decreases seems to contract the final sentence about increases. [Dian Seidel, USA]	Take into accout. That peragraph removed.
2-1355	2	34	7	34	7	river-flow → river flow [Peter Burt, UK]	editorial.
2-1356	2	34	10	34	12	The conclusion on an increase in global river runoff may no longer holds. However, a recent study using gauged data indicates a continuing increase in the major Euraisn Arctic river discharges and a record high occurred in 2007 (Zhang [Xiangdong Zhang, United States of America]	Take into accout. Increase in the major Eurasian Arctic revier dischages is menthioned.
2-1357	2	34	10	34	12	et al. 2012: Enhanced poleward moisture transport amplified the nothern high-latitude wetting trend. Submitted). [Xiangdong Zhang, United States of America]	taking in account. As see response for comment 2-1390.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1358	2	34	11		12	I strongly disagree with this characterization of AR4. Please see p 264 of AR4. [Kevin Trenberth, USA]	accepted, that part for AR4 results is removed in the new version
2-1359	2	34	14	34	27	Extreme hydrological events in the Amazon basin need to be mentioned in this paragraph. In the Amazon basin an increase of discharge extremes is observed since the last decades (Espinoza et al., 2009). In additions, recent hydrological extreme evens hammered the Amazon region. For instances, floods in 1999 and in 2009 (Marengo et al., 2011a; Chen et al., 2009) and extreme droughts in 2005 (e.g. Zeng et al., 2008; Cox et al., 2008, Marengo et al., 2008) and in 2010 (Espinoza et al., 2011; Lewis et al., 2011; Marengo et al 2011b). [Jhan Carlo Espinoza, Peru]	Taking into account. Exterme hydrological events are mentioned in the text.
2-1360	2	34	14	34	27	Reference: [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1361	2	34	14	34	27	-Chen, J. L., Wilson, CR., Tapley, DB. 2010. The 2009 exceptional Amazon flood and interannual terrestrial water storage change observed by GRACE, Water Resources, 46, 1-10, doi:10.1029/2010WR009383. [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1362	2	34	14	34	27	-Cox PM, Harris PP, Huntingford C, Betts RA, Collins M, Jones CD, Jupp TE, Marengo JA, Nobre CA. 2008. Increasing risk of Amazonian drought due to decreasing aerosol pollution. Nature 453:U212–U217. [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1363	2	34	14	34	27	-Espinoza, JC., Guyot, J-L., Ronchail, J. Cochonneau, G., Filizola, N., Fraizy, P., de Oliveira, E., Ordoñez, J.J., Vauchel, P. 2009b. Contrasting regional discharge evolutions in the Amazon basin (1974-2004). Journal of Hydrology 375: 297–311. [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1364	2	34	14	34	27	-Espinoza, JC., J. Ronchail, J. L. Guyot, C. Junquas, P. Vauchel, W. Lavado, G. Drapeau, R. Pombosa. 2011a. Climate variability and extreme drought in the upper Solimões River (western Amazon Basin): Understanding the exceptional 2010 drought, Geophysical Research Letters, 38(13), 1-6, doi:10.1029/2011GL047862. [Jhan Carlo Espinoza, Peru]	Taking into account.Change in exterme hydrological events is mentioned if in the text.
2-1365	2	34	14	34	27	-Lewis SL., Brando PM., Phillips OL., van der Heijden GMF., Nepstad D. 2011. The 2010 Amazon drought. Sience. 311, 554. DOI. 10.1126/science.1200807. [Jhan Carlo Espinoza, Peru]	rejected. Annual event.
2-1366	2	34	14	34	27	-Marengo, J., Nobre, C., Tomasella, J., Oyama, M., de Oliveira, G., de Oliveira, R., Camargo, H., Alves, L. 2008. The drought in Amazonia in 2005. Journal of Climate, 21:495–516. [Jhan Carlo Espinoza, Peru]	rejected. Annual event.
2-1367	2	34	14	34	27	-Marengo, J. A., Tomasella, J., Soares, WR., Alves, LM., Nobre C. 2011a. Extreme climatic events in the Amazon basin, Theoretical and Applied Climatology, doi:10.1007/s00704-011-0465-1. [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1368	2	34	14	34	27	-Marengo, J. A., Tomasella, J., Alves, LM., Soares, WR., Rodriguez, DA. 2011b. The drought of 2010 in the context of historical droughts in the Amazon region. Geophysical Research Letters, 38(13), doi:10.1029/2011GL047436. [Jhan Carlo Espinoza, Peru]	Taking into account. Change in exterme hydrological events is mentioned if in the text.
2-1369	2	34	14	34	27	-Zeng, N., Yoon, J., Marengo, J., Subramaniam, A., Nobre, C., Mariotti, A., and Neelin, J. 2008. Causes and impact of the 2005 Amazon drought. Environmental Research Letters, 3: 99pp. [Jhan Carlo Espinoza, Peru]	rejected. Annual event.
2-1370	2	34	14	34	35	This paragraph is not clearly enough written, and therefore confuses the reader about the main message (that there are no significant trends in streamflow during the 20th century - as stated in Section 2.8, p.86, in one of the bullet points). Especially the last sentence is with its structure very difficult to read and understand. Please rephrase this sentence and maybe rearrange the paragraph so that the main message becomes clearer. [Birgit Hassler, USA]	Take into accout. Paragraph rearranged.
2-1371	2	34	14	34	35	The changes in river runoff for two largest rivers in China should be mentioned, since China is the largest country in this world in terms of population, and these changes have very great impacts on chinese economy. There are very interesting and important changes in river runoff for Yangtze River and Yellow River, with the former having increased runoff and the latter having decreased runoff since 1960. These changes reflect increased precipitation around Yangtze River, the southern part of China, and decreased precipitaion around Yellow River, the northern part of China. The precipitation pattren change forced Chinese goverment to lauch	Take into accout. Relavent information is cited.

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						a very expensive project aiming to transport water from the south to the north. A good reference for the above mentioned changes in river runoff for these two Chinese rivers is Piao et al. (Nature, 467, 43-51) [Zhaomin Wang, UK]	
2-1372	2	34	14			This paragraph sould mention the importance of large-scale river flow archives to assess historical trends (see, e.g., Hannah et al., 2010). [Jean-Philippe Vidal, France]	Take into accout. Comments 2-1372, 2-1373 and 2-1392 are combinedly handled.
2-1373	2	34	14			This paragraph should furthermore emphasize the need for a careful review of such archives in order to actually assess hydroclimatological trends and not trends associated with changes in catchments (land-use changes), rivers (changes in stage-discharge relationship) and measurement devices (manual gaugings, Doppler devices), all of them of anthropogenic origin. [Jean-Philippe Vidal, France]	Take into accout. Comments 2-1372, 2-1373 and 2-1392 are combinedly handled.
2-1374	2	34	14			European assessments of trends in river flows from natural catchments exist in the litterature (see, e.g., Sthal et al., 2010) and should be cited here. [Jean-Philippe Vidal, France]	Cited.
2-1375	2	34	14			References for the comments on this section: - Hannah, D. M., Demuth, S., van Lanen, H. A. J., Looser, U., Prudhomme, C., Rees, G., Stahl, K. and Tallaksen, L. M. (2011), Large-scale river flow archives: importance, current status and future needs. Hydrological Processes, 25: 1191–1200. doi: 10.1002/hyp.7794 - Stahl, K., Hisdal, H., Hannaford, J., Tallaksen, L. M., van Lanen, H. A. J., Sauquet, E., Demuth, S., Fendekova, M., and Jódar, J.: Streamflow trends in Europe: evidence from a dataset of near-natural catchments, Hydrol. Earth Syst. Sci., 14, 2367-2382, doi:10.5194/hess-14-2367-2010, 2010 [Jean-Philippe Vidal, France]	Taking into account. Relavent information is cited.
2-1376	2	34	16	34	16	This is a section of BAMS State of the Climate and should be Fekete B. M. and A. Macdonald, 2011: [Global Climate] River Discharge [in State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S46-S48. [Kate Willett, UK]	noted.
2-1377	2	34	18	34	18	Delete "it must be noted that". Presumably everything in the chapter is worth noting! [Dian Seidel, USA]	accepted
2-1378	2	34	19	34	19	after "construction", insert "land use-land cover changes, increased upstream withdrawals", [Sharad K Jain, India]	accepted.
2-1379	2	34	19	34	19	After "…with caution" and before "Dai et al. …", add "The human influence mainly impacts seasonality of river discharge and has trival effect on annual discharge when exmaining long-term varaibiltiy and changes". [Xiangdong Zhang, United States of America]	NO CHANGE MADE
2-1380	2	34	20	34	20	most → mostly [Peter Burt, UK]	accepted
2-1381	2	34	20	34	20	What is meant by "925 most downstream stations"? The ones closes to the mouths of rivers? Or most of the stations within a group of 925 total? [Dian Seidel, USA]	taking into account. Rephraced.
2-1382	2	34	21	34	24	Nothing is said about the Amazon basin. I would add in the middle of line 24: " An increase of discharge extremes is observed on the main stem of the Amazon basin since the seventies". Reference: Jhan Carlo Espinoza Villar, Jean Loup Guyot, Josyane Ronchail, Gérard Cochonneau, Naziano Filizola, Pascal Fraizy, David Labat, Eurides de Oliveira, Juan Julio Ordoñez, Philippe Vauchel, Contrasting regional discharge evolutions in the Amazon basin (1974–2004), 2009. Journal of Hydrology, 375: 297-311. [Josyane Ronchail, France]	Accepted.
2-1383	2	34	22	34	22	What is meant by "the top 200 rivers"? Best for river-rafting vacations? And I assume these reported trends are in runoff, but that's not clear from the sentence. [Dian Seidel, USA]	taking in to account. Sentence rephrased
2-1384	2	34	24	34	24	Are all the rivers (their runoff, I guess) correlated with ENSO, or are some anti-correlated, which one might supposed given the disparate ENSO precipitation effects around the world. [Dian Seidel, USA]	Taking into account. Sentence rephrased.
2-1385	2	34	24	34	24	But the use of number of rivers having increase/decrease trend is not a good way to indicate streamflow changes across the globe, as the catchment sizes can be very different. [Xuebin Zhang, Canada]	Noted.
2-1386	2	34	25	34	25	This is a good cross reference to ENSO. Mentioning the effects of large scale modes of variability for all	Taking into account. Sentence rephrased..

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						relevant variables would be nice. [Kate Willett, UK]	
2-1387	2	34	28	34	28	Sentence is not totally clear: Does it mean that because of less precipitation there is less runoff? [Birgit Hassler, USA]	Take into accout. This paragraph rearranged.
2-1388	2	34	28	34	29	This interpretation of upward trends in streamflow at high northern latitudes contradicts the conclusion reached in the previous paragraph but it does agree with St. Jacques, et al. 2009 (the reference in the preceding comment). [David Sauchyn, Canada]	taking into account. previous paragraph is deleted.
2-1389	2	34	29			add "apparently" not accompanied… because the precip records are not adequate. [Kevin Trenberth, USA]	accepted.
2-1390	2	34	32	34	32	After "…2008)" and before "The most …", add "However, large uncertainties existed in precipitation data. A recent study through diagnistic analysis of 3 dimensional reanalysis data found a significantly enhanced poleward atmospheric moisutre transport, which suggests an increase in precipiation in the major Euraisn Arctic river basins and serves as the leading driver for the increased river discharge (Zhang et al. 2012) [Xiangdong Zhang, United States of America]	taking into account. Uncertainty in precipitation in Eurasian Arctic is mentioned.
2-1391	2	34	32	34	35	The authors should also cite here Legates et al. (2005, AWR) who were the first to point at the main flaws of the Labat et al. (2004) study. Ref: Legates, D.R., et al. 2005, Advances in Water Resources, 28, 1310-1315. [Sonia Seneviratne, Switzerland]	accepted.
2-1392	2	34	35			Need to add more on this: explain that the issues of missing data and how spatial gaps are filled in are substantial and have been improved, but that the records are not viable globally prior to about 1948 (Dai et al 2009). [Kevin Trenberth, USA]	Accepted.
2-1393	2	34	37	34	48	in 2.3.3 Soil Moisture, conclusion of the soil moisture change shoud be made, or provide trends in main continents, even if with some uncertainty. [Jianting Cao, China]	There are not real trends in observed soil moisture, which are mainly regional (e.g. Ukraine). LSMs in relation to drought is discussed in the drought section of section 2.7
2-1394	2	34	37	34	48	Sorry because this comment should have been mentioned before: The discussion about Soil moisture reflects that there are no studies about this variable at regional and/or global scale, regardless the availability of GLDAS, GLDAS2 and satellite data. I consider that some recommendation in this regard should be stated. Do we need to recomend that we need more studies regarding this variable, do we think that GLDAS2 is not reliable to be used with this aim? Which data sets and/or efforts should be supported in this area? [Celeste Saulo, Argentina]	IPCC does not make recommendations. LSM analyses are discussed in relation to drought in section 2.7. We are adding some text on satellite derived soil moisture.
2-1395	2	34	37	34	48	2.3.3 Soil Moisture: BAMS State of the Climate 2010 included a soil moisture section showing satellite data from 1991-2010. There seemed to be more to say than is in the paragraph here. See de Jeu, R., Dorigo, W, W. Wagner and Y. Lui, 2011: [Global Climate] Soil Moisture [in State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S52-S53. [Kate Willett, UK]	This reference discusses the fact that there are satellite data sets, but doesn't really describe any longer term changes. LSM output is discussed in section 2.7 in relation to drought, so its not clear
2-1396	2	34	39	34	48	It will be helpful to mention here that increased application of irrigation water has led to significant change in moisture regime over large land areas in many places such as India. [Sharad K Jain, India]	Is there a reference? Cannot add text like this without a reference.
2-1397	2	34	39	34	48	Mention of the satellite climate data record for soil moisture since 1991 from scatterometer measurements should be made (see Wagner et. al. 2003)) Wagner, W., K. Scipal, C. Pathe, D. Gerten, W. Lucht, and B. Rudolf. (2003) Evaluation of the agreement between the first global remotely sensed soil moisture data with model and precipitation data. Journal of Geophysical Research-Atmospheres, 108(D19). [Roger Saunders, United Kingdom]	We are going back to the original references from the BAMS report to include material on the 1991-present period based on satellite derived soil moisture.
2-1398	2	34	39	34	48	should this be combined with Section 2.7 as there is really no assessment on soil moisture here. [Xuebin Zhang, Canada]	We are adding some material and we feel that soil moisture should remain separate.
2-1399	2	34	41	34	41	A rare, 45-year..' → A rare 45 year..' [Peter Burt, UK]	Accepted.
2-1400	2	34	42	34	42	Reference required [Peter Burt, UK]	included reference to Robock et al.

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2-1401	2	34	44	34	44	using 'etc' is poor style. Omit. [Peter Burt, UK]	Accepted.
2-1402	2	34	45	34	45	Avoid calling model simulations of soil moisture "data", which suggest actual observations. [Dian Seidel, USA]	Accepted, changed to simulations.
2-1403	2	34	48	34	48	At the end, add "A recent study identified a larger increasing trend of net atmosperhic moisutre transport than that of river diacharges in the major Eurasian Arctic river basins, suggesting a wetting trend of soil moisture (Zhang et al. 2012). [Xiangdong Zhang, United States of America]	Reject, reference not provided, cannot find this reference in a search.
2-1404	2	34	50	35	35	This comment relates both to section 2.3.4, as well as the second on evaporation in Chapter 12. In particular, I am having trouble reconciling the observations of pan evaporation with projections of future actual evaporation. It would be good, for example, if the discussion in Chapter 12 could include projections for potential evapotranspiration (and better still, that it can be broken down into changes in radiative forcing, moisture deficit and winds etc), that could be compared to observations. Also in chapter 2 it would be worth discussing some of the differences between pan evaporation and potential evapotranspiration. As it currently stands, it is difficult to reconcile observations with modelling projections of this important hydrological quantity. [Seth Westra, Australia]	Reject, we are assessing the observed record and its not clear that model projections for the future should correspond to observations. Pan evaporation is not PET since PET includes transpiration from vegetation.
2-1405	2	34	52	34	52	Change "have" to "had" in discussing AR4 [Dian Seidel, USA]	Thanks, changed to were.
2-1406	2	34	52	35	38	It could perhaps be mentioned also the soil moisture-precipitation-temperature feedback that was one of the key factors in enhancing the heat wave effect during summer 2003 in Europe (several studies on this point). That episode is local but a significant example of how the soil moisture-precipitation-temperature feedbacks could potentially alter the climate of a region. [Claudio Cassardo, Italy]	Reject, that is included in the temperature extremes section (2.7) in relation to the 2010 heatwave.
2-1407	2	34	54	34	54	delete comma after 'humidity' [Peter Burt, UK]	done.
2-1408	2	34	56	34	56	Reanalysis evaporation fields from ? What does it mean ??? [Michel Boko, Benin]	deleted "from" after fields.
2-1409	2	34	56	34	56	Something is missing at : "reanlaysis evaporation fields from" [Sharad K Jain, India]	deleted "from" after fields.
2-1410	2	34	56	34	56	Please rewrite: 'fields are not reliable' in stead of 'fields from are not reliable' [Valentina Pavan, Italy]	deleted "from" after fields.
2-1411	2	34				2.3.5 There are reports of increased ET in regions of irrigation such as India. Irrigation effects are not adequately dealt with. It should note that pan evaporation often has little or no relation to actual ET. This section should emphasize the poor, short and spotty records that make any statement about ET uncertain. [Kevin Trenberth, USA]	References for increased ET in regions of irrigation?
2-1412	2	35	1	35	4	The Fluxnet data-driven analysis of measured evapotranspiration, satellite data and machine learning showed an increasing trend in global ET from 1982-97, and the increase in ET ceased thereafter due to decreased soil surface moisture supply (TRMM surface moisture data), particularly in semi-arid regions. This information shows up in more of a short summary on lines 33-38 (Jung, M., M. Reichstein, P. Ciais, S.I. Seneviratne, J. Sheffield, M.L. Goulden, G. Bonan, A. Cescatti, J. Chen, R. de Jeu, A.J. Dolman, W. Eugster, D. Gerten, D. Gianelle, N. Gobron, J. Heinke, J. Kimball, B.E. Law, L. Montagnani, Q. Mu, B. Mueller, K. Oleson, D. Papale, A.D. Richardson, O. Roupsard, S.W. Running, E. Tomelleri, N. Viovy, U. Weber, C. Williams, E. Wood, S. Zaehle, K. Zhang. 2010. A recent decline in the global land evapotranspiration trend due to limited moisture supply. Nature 467: 951-954, DOI 10.1038/nature09396.). [Beverly Law, USA]	Thanks we are including a modified version of this statement and reference.
2-1413	2	35	3	35	4	Instead of saying "providing an unprecedented look at global evapotranspiration", how about giving concrete results of what the new data show. [Dian Seidel, USA]	see answer to 1412 above.
2-1414	2	35	4	37	28	Figure 2.18 reveals no long-term trends whatsoever. No significance is given to any of the data in Table 2.10 either. The conclusion, that "absolute moistening has been widespread across the globe since the 1970s, with very high confidence." seems very difficult to comprehend from the data presented. What is the basis for the 'very high confidence'? I do not believe that it is in any way substantiated by the evidence presented. [Philip Lloyd, South Africa]	Figure 2.18 map shows mostly positive trends since 1973, only a few areas with negative trends. We are recalculating the trends in the map and will indicate significance. The words "very high confidence" have been revised.
2-1415	2	35	6	35	6	Delete "some new material has been published on pan evapotranspiration". Of course, or else you would not	revised text.

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						be summarizing that new material in this paragraph. [Dian Seidel, USA]	
2-1416	2	35	8	35	8	insert comma after 'similarly' [Peter Burt, UK]	done.
2-1417	2	35	9	35	9	Many regions of the world have reported decreases in Epan with increases in air temperature and this has been termed as of Epan paradox. A statement of this effect can be included here. For details, please see: Jhajharia et al. Hydrol. Process. 26, 421–435, (2012). [Sharad K Jain, India]	Considered
2-1418	2	35	9			Canadian Prairies ??? [Michel Boko, Benin]	Not sure what this is asking, there is a reference given.
2-1419	2	35	11	35	11	examine → examined [Peter Burt, UK]	Accepted.
2-1420	2	35	13	35	13	conclude → concluded [Peter Burt, UK]	Accepted.
2-1421	2	35	15	35	16	This language is not clear to me. [Dian Seidel, USA]	clarified in text.
2-1422	2	35	18	35	19	The authors could also cite here the following review paper on this specific topic: Seneviratne, S.I. et al. 2010, Earth-Science Reviews, 99, 125-161. [Sonia Seneviratne, Switzerland]	Rejected, reference not needed.
2-1423	2	35	23	35	24	The study by Jung et al. (2010) is not about trends but about variability in recent years (in the time period 1998-2008 vs 1992-1987). It also states explicitly in its introductory paragraph: "Whether the changing behaviour of evapotranspiration is representative of natural climate variability or reflects a more permanent reorganization of the land water cycle is a key question for earth system science". Hence I would suggest that the authors replace the current text with: "In (semi-)arid regions, decadal variability in evapotranspiration is expected to follow respective variations in precipitation (Jung et al. 2010)." [Sonia Seneviratne, Switzerland]	We included a sentence citng this study stating there was an increase to 1997 then the increase stopped due to moisture deficit.
2-1424	2	35	24	35	24	When referring to trends in surface winds, it would make sense to add a reference to Section 2.6.2 after "Vautard et al. 2010" for internal consistency within the chapter. [Sonia Seneviratne, Switzerland]	Agree, added.
2-1425	2	35	24	35	25	How? [Peter Burt, UK]	Adding explanation of how wind and CO2 alter the energy balance is beyond the scope here.
2-1426	2	35	28	35	28	insert commas after 'regions' and 'from' [Peter Burt, UK]	Accepted.
2-1427	2	35	29	35	29	Since there is still some debate on the exact magnitude of this effect, the authors should probably add "can" before "lead to reduced stomatal opening and evapotranspiration". [Sonia Seneviratne, Switzerland]	Agree, added.
2-1428	2	35	29	35	30	A more recent publication documenting such effects based on field experiments is the one by Morgan et al. (2011, Nature). Ref: Morgan, J.A., et al. 2011, Nature, 476, 202-205. [Sonia Seneviratne, Switzerland]	Will include mention in text.
2-1429	2	35	33	35	34	Confusing that pan evaporation "declines in most regions" yet on global scale evapotranspiration "increased". Clarify. [Bruce Wielicki, USA]	This is because pan evap and ET as calculated by LSMs don't necessarily agree.
2-1430	2	35	33	35	38	A recent paper by Van Heerwaarden et al (2010) shows that regulation of the near-surface temperature and humidity by land-atmosphere feedbacks results in a strong connection between pan evaporation, actual evapotranspiration and vapor pressure deficit (VPD) depending on the climate forcings. When climate change occurs, the feedbacks direct the system towards a different combination of the three variables. If the trends in pan evaporation, VPD and wind speed are known, these can therefore infer the change in the forcings and estimate the trend in actual evapotranspiration. Reference: Van Heerwaarden, C. C., J. Vilà-Guerau de Arellano, and A. J. Teuling (2010), Land-atmosphere coupling explains the link between pan evaporation and actual evapotranspiration trends in a changing climate, Geophys. Res. Lett., 37, L21401, doi:10.1029/2010GL045374. [Albert A.M. Holtslag, Netherlands]	Thank you for the comment. We will consider including something about this in the text.
2-1431	2	35	35	35	34	The two time periods (1980s to 1990s and 1982 to 2002) are not the same. [Dian Seidel, USA]	This is revised to separate the two main studies (Jung and Wang) conclusions since they are similar but not identical.

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2-1432	2	35	36	35	37	This sentence is difficult to understand. What do you mean by "an increase in moisture limitation"? And what is the context for the phrase "further increase"? This is the first mention of Southern Hemisphere changes. [Dian Seidel, USA]	It means drying, or reduction in moisture availability. Will revise also add text about SH earlier.
2-1433	2	35	36	35	38	As mentioned above, the study by Jung et al. (2010) is not about trends but about variability in recent years (in the time period 1998-2008 vs 1992-1987). It also clearly states that whether the documented change corresponds to a permanent modification of the climate system is not established with that analysis. Hence, the following text (or similar) should be added after "has acted as a constraint to further increase of global evapotranspiration (Jung et al. 2010)": ", although it is still unclear at the present stage whether this reflects a long-term tendency." [Sonia Seneviratne, Switzerland]	This is revised to separate the two main studies (Jung and Wang) conclusions since they are similar but not identical.
2-1434	2	35	37	35	37	I suggest to insert 'land' between 'global' and 'evapotranspiration', becase Jung et al. (2010) only reconstructed evapotranspiration over land. [Zhaomin Wang, UK]	Accepted.
2-1435	2	35	40	39	30	IMPORTANT. The near surface, lower tropospheric, upper tropospheric and stratospheric roles played by atmospheric water vapour are so important that they should be discussed and expanded (see Comment 49 below) under the heading "Water Vapour" as was done in AR4 (see "3.4.2. Water Vapour" at http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch3s3-4-2.html). Stratospheric water vapour is covered later in this draft, but it should be moved here as was done in AR4. This consolidation will provide a far better organization of AR5's water vapour presentation. [Forrest Mims, USA]	Reject, stratospheric water vapor is not very important to the hydrological cycle, but is important in radiative forcing.
2-1436	2	35	40			Section 2.3.5: The whole section gives mixed messages combined with Figure 2.18b-e. The datasets described in Table 2.10 show an increase in global surface humidity, however, the reanalyses show rather a stagnation or decrease in Figure 2.18b-e. It might help if Figure 2.18 would be divided into two figures, or if the structure of the section would be rearranged. [Birgit Hassler, USA]	Reject, all of figure 2.18 is from observed data (HadCRUH) not reanalyses. This figure, esp. the map of trends is being redone using a different trend estimator. C and E from 2.18 are RH, and B and D are SH, which do show trends and are consistent with the map.
2-1437	2	35	42	35	42	Change "alongside" to something like "coincident with" or "along with". Alongside suggest nearness in space, not at the same time. [Dian Seidel, USA]	agree, done.
2-1438	2	35	49	36	2	Consider removing this material as unnecessary background. [Dian Seidel, USA]	agree it is not need, also do need to reduce length.
2-1439	2	35	52	35	52	water vapor is not "prolific". Better word would be "abundant". [JOHN OGREN, USA]	removed paragraph.
2-1440	2	35	52	35	52	Replace "prolific" with "abundant". [Robert Waterland, United States of America]	removed paragraph.
2-1441	2	35	53	35	53	insert comma after 'gases' [Peter Burt, UK]	removed paragraph.
2-1442	2	35	55	35	46	Might be worth noting here and mean precipitation does not correlate well with specific humidity/water vapour. [Kate Willett, UK]	removed paragraph.
2-1443	2	35	55			Change "hydrological" to "hydrologic" here and in a total of 31 places in Chapter 2. [Forrest Mims, USA]	removed paragraph.
2-1444	2	35	55			"Absolute humidity" generally refers to the mixing ratio in a given parcel of air, often 1 m^3. A better parameter here is "precipitable water," the total column water vapour. [Forrest Mims, USA]	removed paragraph.
2-1445	2	35				FAQ 2.1, Fig 1: I suggest changes to one of the labels in this figure - see my comments for Chapter 3 Page 143. [David Wratt, New Zealand]	Wrong place for comment? Not about section 2.3
2-1446	2	35				in the middle of line 24 add: Gao et al. (2007) reported that the spatial distribution of the trend for the actual evapotranspiration is similar to that of the potential evapotranspiration in the humid region in south China, while the trends are opposite in arid and semi-arid regions in north China; where the change in precipitation played a key role for the change of estimated actual evapotranspiration. Gao, G., D.L. Chen, C-Y Xu, E. Simelton, 2007. Trend of estimated actual evapotranspiration over China during 1960-2002. Journal of Geophysical Research – Atmospheres, VOL. 112, D11120, doi:10.1029/2006JD008010, 2007	Reject, doesn't actually provide change, only description of spatial consistency. This is too regional specific.

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						[Chong-Yu Xu, Norway]	
2-1447	2	35				In line 28 after "et al., 2010).", add the following: "Xu et al. (2006c) reported that for the Yangtze River basin in China, there is a significant decreasing trend in both the reference evapotranspiration and the pan evaporation, which is mainly caused by a significant decrease in the net total radiation and to a lesser extent by a significant decrease in the wind speed over the catchment". Xu, C-Y , L. Gong, T. Jiang, D. Chen, and V.P. Singh, 2006c. Analysis of spatial distribution and temporal trend of reference evapotranspiration in Changjiang (Yangtze River) catchment. Journal of Hydrology 327, 81-93 [Chong-Yu Xu, Norway]	Reject, too regional specific and we are assessing literature since the AR4.
2-1448	2	36	1			And implications for ecosystem health as well. [David Sauchyn, Canada]	This paragraph has been removed to reduce length.
2-1449	2	36	4	36	13	Can you discuss the sign difference between RH and specific humidity….at least mention the required temperature increase. Also, the time series plot shows specific humidity (which I assume is a global average) is essentially constant until 1997, there is a jump and then a decrease. Given those types of progressions in time, how meaningful is a calculated linear trend? [Karen Rosenlof, United States of America]	Yes, it is related in the next paragraph
2-1450	2	36	4			I miss a reference to the since AR4 newly compiled EDGAR 4.2 global CH4 emissions inventory for 1970-2008, comprising all anthropogenic sources and showing a 45% increase since 1970. Since 2000, CH4 emissions increased by 18%, driven bij large decreases in China an Brazil. Trend analysis and methodology used can be found in: Olivier, J.G.J. and G. Janssens-Maenhout (2011) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2011 Edition", pp. III.1-III.49. International Energy Agency (IEA), Paris. ISBN 978-92-64-102835. [Jos Olivier, Netherlands]	Comment must be misplaced, the comment appears to be about methane and this section is surface humidity.
2-1451	2	36	7	36	8	Standard errors would be useful to compare these rates. [Peter Guttorp, USA]	We are recalculating trends using OLS with DF reduction and will provide significance.
2-1452	2	36	10	36	10	southern hemispherer → Southern Hemisphere [Peter Burt, UK]	Accepted.
2-1453	2	36	11	36	11	– → : (hyphen looks like a link between words!) [Peter Burt, UK]	changed
2-1454	2	36	12	36	12	What are "the 23 regions"? [Dian Seidel, USA]	the regions are defined in the reference (Giorgi and Francisco, 2000) and were used in AR4 regional chapter.
2-1455	2	36	15	36	22	Figure 2.18: It would be better to show negative trends in blue and positive trends in red. [Norman Loeb, United States of America]	Standard for hydrological variables is green for increase, brown for decrease. Red and blue is for temperature.
2-1456	2	36	15			Figure 2.18. trend lines and confidence bounds would be useful for b) through e). [Bruce Wielicki, USA]	We are recalculating trends using OLS with DF reduction and will provide significance.
2-1457	2	36	16	36	17	Fig 2.18a, and its caption, would be better if all local areas where the trend is significant at the 5% level were shown. . Field significance can then be judged - or even calculated? Nevertheless, this is an informative map - any chance of an update, e.g. in the SOD, beyond 2003? [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	We are recalculating trends using OLS with DF reduction and will provide significance.
2-1458	2	36	25	36	25	The difference between the trends quoted for marine surface humidity from Willett et al (2008) and Berry and Kent (2009) are almost entirely due to the adjustment for height applied by the latter but not the former. The height adjustment is based on good knowledge of measurement height (Kent et al. 2007) and is well-founded as humidity is expected to decrease with height above the sea surface. Thus the reasons for the fairly large difference between the global trends from the 2 datasets is known and the uncertainty is smaller than the difference between them would indicate. [Elizabeth Kent, England]	This is discussed on p. 37, second paragraph.
2-1459	2	36	25	37	3	As was done with temperature trends, humidity trend confidence intervals should be included. [Dian Seidel,	We are recalculating trends using OLS with DF

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						USA]	reduction and will provide significance.
2-1460	2	36	25			Table 2.10. This table needs statistical significance for its trends in specific and relative humidity. [Bruce Wielicki, USA]	We are recalculating trends using OLS with DF reduction and will provide significance.
2-1461	2	36	50			I miss a reference to the since AR4 newly compiled EDGAR 4.2 global N2O emissions inventory for 1970-2008, comprising all anthropogenic sources and showing a 40% increase since 1970. This dataset has been compiled using the latest 2006 IPCC guidelines, including new indirect N2O emission factors and including also non-agricultural indirect N2O sources. Since 19900, global N2O emissions increased by 8%. Non-Annex I countries saw increases of over 30% since 1990, mainly in the agricultural sector in Asia and Latin America. Annex II Europe saw decreases of almost 30% mainly due to abatement in the chemical industry and less use of fertilisers. Trend analysis and methodology used can be found in: Olivier, J.G.J. and G. Janssens-Maenhout (2011) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2011 Edition", pp. III.1-III.49. International Energy Agency (IEA), Paris. ISBN 978-92-64-102835. [Jos Olivier, Netherlands]	Comment must be misplaced, the comment appears to be about NOx and this section is surface humidity.
2-1462	2	36				Table 2.10: If possible, please add uncertainties to the trend estimates. [Birgit Hassler, USA]	We are recalculating trends using OLS with DF reduction and will provide significance.
2-1463	2	37	3	37	3	"There is good agreement with ERA reanalyses…" Of what? The observations summarized in Table 2.10? [Dian Seidel, USA]	Text has been changed
2-1464	2	37	3	37	3	In what sense is ERA-Interim an "improvement" over ERA-40? Is this remark based on the agreement with the observations reported in Table 2.10? Actually, I find all the material in this paragraph a bit hand-wavy and without firm literature basis. And if the authors decide to downplay reanalyses in this chapter, the paragraph is not needed at all. [Dian Seidel, USA]	Text has been changed
2-1465	2	37	3		10	The flattening or even decrease of relative humidity since 2000 contradicts the water vapor feedback hypothesis. Conversely, it is consistent with the observed temperature plateau. [François GERVAIS, France]	It is consistent with the flattening of global temperatures since 2005.
2-1466	2	37	4	37	9	The text refers to a comparison between data and ERA interim that is supported by Figure 2.18 b and c, but this figure only shows HadCRUH anomalies. (I assume that the anomalies are with respect to the 1973-2003 HadCRUH mean, although not stated in the text). Could you clarify how this comparison with ERA interim has been done? [Celeste Saulo, Argentina]	Figure 2.18 is being redone with additional data sets.
2-1467	2	37	6			Need to introduce RH as different than specific or abolute humidity. [Kevin Trenberth, USA]	Disagree, assume reader knows the difference.
2-1468	2	37	8	37	8	compatible with the plateau in specific humidity and the increase of surface temperature [Claudio Cassardo, Italy]	Disagree, plateau is in SH, global temps also plateaued since 2000.
2-1469	2	37	9	37	10	This sentence would be clearer if it said "the greater warming of the land surface relative to the ocean surface" with a cross reference to where this is shown earlier in chapter 2. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Agree, changed.
2-1470	2	37	9	37	10	I believe the Joshi 2008 study is purely a climate model study with no claims made about any observed land/sea contrast. This is not an appropriate reference here. [Gareth S Jones, UK]	Yes, but it does discuss land/sea differences.
2-1471	2	37	14	37	14	add "or mechanically ventilated" after "handheld". [Elizabeth Kent, England]	Accepted.
2-1472	2	37	15			Please explain "data failing neighbour consistency checks". The AR5 will have a broad scientific audience; not confined to specialists in statistical climatology. [David Sauchyn, Canada]	Disagree, people reading the detailed chapters will understand. Most non-specialists don't read this.
2-1473	2	37	16	37	16	quantify 'good agreement' [Peter Burt, UK]	Cannot quantify, this is a qualified good agreement. Basically they show similar results.
2-1474	2	37	16	37	16	Replace "Both show good agreement with (Dai, 2006)…" with "Both show good agreement with Dai (2006)…" [Alice Grimm, Brazil]	corrected.
2-1475	2	37	16			Incomplete sentence? [Shouraseni Roy, USA]	Not incomplete, the parenthesis before Dai is in the

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							wrong place.
2-1476	2	37	19	37	22	This suggests that there is a problem with marine relative humidity data but not with marine specific humidity data. That is hard to understand. I'm guessing the basic measurement is dew point or wet-bulb temperature, so both RH and q would be affected. [Dian Seidel, USA]	Yes, correct
2-1477	2	37	21	37	22	Berry (2009) conclude that this is a feature of moving from a very positive NAO to a very negative NAO. This is probably worth mentioning as this reason for this bias/shift has not been unequivocally identified. (Berry, D. I., 2009: Surface forcing of the North Atlantic: accuracy and variability. University of Southampton, School of Ocean and Earth Science, Doctoral Thesis, 176pp.) [Kate Willett, UK]	Will include mention in text.
2-1478	2	37	22	37		Please state what the "non-climatic data issues" actually is (or if it is entirely unknown). [Karen Rosenlof, United States of America]	this was a change in reporting practice, has been included in text.
2-1479	2	37	24	37	25	Fig 2.18a doesn't appear to fully support a high confidence statement on moistening since the values at the start of the record are higher than at the end or at least such a statement might require a bit more explanation as to why fig 2.18 is consistent (short term variability vs longer term changes, also confounded by data issues as also discussed for other parts of fig 2.18) [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Agree, the high confidence statement has been revised.
2-1480	2	37	30	37	36	Ideally the entire atmospheric water vapour section will be reorganized along the lines of AR4 (see Comment 46 above). In any case, and as explained in these comments, the second sentence of this introduction will better fit the mixed results from the various major water vapour projects if revised to read along this line: "Reanalyzed radiosonde measurements from 1978 of in situ and integrated column water vapour (precipitable water or IPW) and measurements of IPW by GPS stations from 1990 to 2000 suggest slight increases in total column abundance of tropospheric water vapour over land. Satellite measurements of tropospheric water vapour over oceans are influenced by instrumental differences and clouds. Some satellite programs show slight increases in water vapour over oceans while others do not. Thus, there is not yet a discernable trend for global satellite IPW measurements." [Forrest Mims, USA]	Editorial. Rejected. The outlay of the water vapor section remains unchanged since stratospheric water vapor is covered under Atmospheric Composition. The published studies of satellite water vapor trends cited here and in AR4 contradict the reviewers assertion regarding column integrated water vapor changes.
2-1481	2	37	33	37	43	"column integrated water vapour" appears twice in this text. Then "total precipitable water vapour (TPW) and "TPW" appear in the caption for Fig. 2.19. Some readers might not know that these names have the same meaning. It would be best to define the various phrases that describe total column water vapour and then use the same phrase throughout the report. [Forrest Mims, USA]	Accepted. Figure 2.19 has been changed to "column integrated water vapour"
2-1482	2	37	34	37	34	delete comma after 'GPS' [Peter Burt, UK]	Accepted.
2-1483	2	37	34	37	35	The entire discussion of tropospheric water vapor deserves discussion of uncertainties and problems in the major studies and references to studies that show little or no trends. (For example: "Our approach indicates that the multi-decadal trends in water vapor content on the regional scale are not yet well understood; a conclusion was also reached by Spencer et al. [2007]." Wang, J.W., K. Wang, R. A. Pielke Sr., J. C. Lin, and T. Matsui (2008), Towards a robust test on North America warming trend and precipitable water content increase, Geophys. Res. Lett., 35, L18804, doi:10.1029/2008GL034564.) The generalization here is misleading for it is based on reprocessed radiosonde data, GPS data from mainly over land (with a few islands and oil platforms) and satellite data from mainly over oceans. There remains strong debate over the reliability of the major water vapour studies (which are unmentioned in this chapter) and uncertainty over global trends and how best to blend data from various satellites with one another and with GPS. While large areas may show moistening, other areas are stable or do not. (My 22-year time series from a fixed site shows a decline of about 0.5 mm/decade in precipitable water.) [Forrest Mims, USA]	Rejected. This study is based on NCEP reanalysis which as already discussed in this section are not considered reliable for the detection of trends in water vapor.
2-1484	2	37	34	37	36	Specify the period of these changes. [Dian Seidel, USA]	Acknowledged.
2-1485	2	37	38	40	57	sub-titles need to be consistent. Sub-section 2.3.6.1 has a title "Radiosounde", while 2.3.7.1 uses "Surface Observations". [Xuebin Zhang, Canada]	Rejected. Radiosondes are not considered surface observations. The convention for "surface observations" is consistent between humidity (2.3.5) and clouds (2.3.7.1).
2-1486	2	37	40	37	40	I suggest inserting the following between the words: "the" and "troposphere": "quasi-global" [Martin Hovland,	Editoral. Rejected.

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						Norway]	
2-1487	2	37	40	37	45	"All agree that there are significant issues" - I disagree! The raw data is the raw data, and you cannot slide away with such a casual statement. If there are reaons to question the DATA, then they must be spelled out. You must be unequivocal. A bland statement like this convinces no-one. 50 years of global radiosonde data cannot be thrown away without anything less than a very thorough analysis. [Philip Lloyd, South Africa]	Rejected. This statement refers to the studies listed in Table 2.11 and is accurate. The studies cited did do a trougough analysis of the radiosonde data and outlines the reasons for and methodology used to homogenize the data. No change has been made.
2-1488	2	37	42	38	3	This section is inconsistent in its discussion of radiosonde humidity trends: first it states that issues "preclude its use for climate analysis", but then it goes on and uses it anyway. Needs correction/clarification [Bruce Wielicki, USA]	Acknowledged. This statement has been changes to "... that preclude its use for climate analysis unless inhomogeneities due to instrumental biases are accounted for"
2-1489	2	37	47	38	1	"All remove an artificial temporal trend towards drying in the raw data" Hang on right there! Where did that weasel word 'artificial' come from? The data show a trend towards drying - so that is an observation, and observations are NOT 'artificial'!The drying may be unexpected, but that merely challenges the basis of the belief that it should not have been there - observations have always challenged beliefs, which is why we make them. But to call the unexpected 'artificial' is to rely on the belief - I think Galileo had such a problem with the inquisition. [Philip Lloyd, South Africa]	Rejected. The evidence for the drying being artificial is provided in the cited papers. No change has been made.
2-1490	2	38	2	38	4	"In each analysis, the rate of increase - - -" Sorry to hammer the point, BUT THERE WAS NO INCREASE. The measurements didn't show any. So to argue that some 'homogenized' data agrees with a Clausius Clapeyron estimate of an increase is, frankly, claptrap. None of us understands what is going on - I accept that, but we have to confess to our igonorance and hope that it will be accepted as an honest evaluation. [Philip Lloyd, South Africa]	Rejected. The evidence of the increase is presnted in the cited papers. No change has been made.
2-1491	2	38	6	38	6	behavior → behaviour [Peter Burt, UK]	Accepted.
2-1492	2	38	6			HFC-143a is not mentioned here, although its present global emissions are similar to HFC-152a (Ashford et al., 2004). Ashford, D., Clodic, D., McCulloch, A. and L. Kuijpers (2004). Emission profiles from the foam and refrig-eration sectors comparison with atmospheric concen-trations. Part 2: results and discussion, International Journal of Refrigeration, 27, 701-716. [Jos Olivier, Netherlands]	Wrong section.
2-1493	2	38	9	38	11	There are a bunch on undefined symbols and acronyms in Table 2.11. [Dian Seidel, USA]	Acknowledged. The symbols have been replaced with words.
2-1494	2	38	10			In Table 2.11 the Durre et al paper includes many records subsequently found to have major inhomogeneities by Dai et al. Here is where an assessment should discount that study. [Kevin Trenberth, USA]	Rejected. The Dai et al. study does not directly refute the main conclusions of Durre et al. Space does not permit elaboration on specific records where disparities remain.
2-1495	2	38	13	38	27	Is it really important to summarize GPS-based water vapor changes for such a short record? I think not. But if you keep this, please clarify when the observations began (early 1990's, 1997, and 1994 are all mentioned), and extend the record beyond 2006. [Dian Seidel, USA]	Editorial. Rejected.
2-1496	2	38	13	38	27	You might add the typical values of column integrated water vapour for comparison: these are up to 50 mm over the Warm Pool. Should the water vapour column trends be given in kg/m2 to be consistent with Fig 2.19? [Robert Waterland, United States of America]	Editorial. Acknowledged. The units are changed to kg/m2.
2-1497	2	38	15	38	27	It is stated that the GPS record can't be interpreted as a trend…but how about discussing agreement with the radiosonde or operational satellite temperatures. If agreement is good, doesn't this give some confidence in trends determined from these other data sources? [Karen Rosenlof, United States of America]	Acknowledge. This is discussed with reference to the Mears (2011) study.
2-1498	2	38	16			replace "non-aluminium production" by "other CF4 use sources", since this is what is meant here. [Jos Olivier, Netherlands]	This comment is mis-numbered. There is no such discussion at this point in the text
2-1499	2	38	17	38	17	Number of GPS stations: Change "over 400" to "564" (Reference: http://www.gpsmet.noaa.gov/cgi-bin/get_site_info.cgi) [Forrest Mims, USA]	Accepted. Changed to "over 500". The exact number will continue to change before the reported is

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							published.
2-1500	2	38	20	38	20	vapor → vapour [Peter Burt, UK]	Accepted.
2-1501	2	38	21	38	26	Rigby et al. also mention the lack of recent datasets of global SF6 sales data and the largest uncertainties in bottom-up emissions inventories after 1990 being: (a) unknown sources (reported as sale to utilities and equipment manufacturers), (b) production level and sales mix of China and Russia (in particular the division into sources with banking, e.g. switchgear, and others, e.g. magnesium), (c) 2004-2006 data due to incomplete surveys, and (d) the effective annual emission rates of SF6 stock in switchgear. I suggest to add some of the elements to stress the lack of bottom up data coverig the global total. [Jos Olivier, Netherlands]	Wrong section.
2-1502	2	38	29	39	30	2.3.6.3 Satellite: Radiosonde biases are briefly discussed elsewhere, but here there is no mention of satellite instrument calibration degradation and drift, orbital and positional (pointing) drift, missing coverage and the well known deleterious effects of clouds on total water vapour measurements. Satellites provide fantastic global snapshots of the distribution of water vapour, but they are by no means perfect, and this section very badly needs a disclaimer or qualifier to that effect. [Forrest Mims, USA]	Editorial. Rejected.
2-1503	2	38	31	38	32	over what period(s) were these trends observed? [Dale Hurst, United States of America]	Acknowledged.
2-1504	2	38	32	38	34	The phrase "widespread increases in lower tropospheric water vapor" seems inconsistent with the bottom panel of Fig. 2.19, which shows decrease over parts of the tropical North Atlantic and over mcuh of the Southern Hemisphere. The big increase in water vapor averaged over 60N-60S over ocean (Fig. 2.19 top) is (a) probably dominated by tropical increases, because most water vapor is in the tropics, and (b) probably explainable as one or two step-like changes rather than a smooth increase. Particularly because of (a), please reconsider the word "widespread". [Dian Seidel, USA]	Acknowledge. This has been changed to emphasize the increases at the near-global scale and their consistency with the SST changes.
2-1505	2	38	37	38	38	The Clausius/Clapeyron relationship applies only to systems in eqauilibrium. Since no part of the climate is ever in equilibrium agreement with Clausius/ Clapeyron would be unlikely. [VINCENT GRAY, NEW ZEALAND]	Editorial. Rejected. If the changes in relative humidity are small compared to the changes in equilibrium vapor pressure, then the changes in water vapor will closely follow the changes in Clausius Clapyeron. No change has been made.
2-1506	2	38	40	38	40	Figure 2.19: Could the colours and map orientation be kept the same for the maps in Figure 2.18 and Figure 2.19? Blue or green for more moisture and red or brown for less moisture would be preferable. [Kate Willett, UK]	Acknowledge.
2-1507	2	38		38		Table 2.11: It would be helpful to include in the first column one reference associated with these data sets, even if it is in the text, for instance, Durre et al. (2009). [Alice Grimm, Brazil]	Rejected. The reference is in column 1. No change has been made.
2-1508	2	38				Table 2.11: Add the time period covered by each dataset. [Birgit Hassler, USA]	Rejected. This is described in the text.
2-1509	2	38				section 2.3.6.3 The link to SST with precipitable water is in Trenberth et al 2005: Trenberth, K. E., J. Fasullo, and L. Smith, 2005: Trends and variability in column-integrated atmospheric water vapor. Clim. Dyn., 24, 741-758. Also there have been major issue in the recent satellite record because of the failure of the two SSM/I instruments. How thhis record is merged with the SSM/IS is a substantial issue and none of this is addressed here. This is also why GPCP v3 is delayed. Line 44 how can WEntz et al 2007 do a record for 1988 to 2010? The record for the past few years is non-trivial and that reference does not deal with it. [Kevin Trenberth, USA]	Acknowledged. The precipitable water products generated by Remote Sensing Systems are intercalibrated. The SOD will use version 7 of their data products which are intercalibrated for all SSMI satellites.
2-1510	2	39	1	39	14	I am suprised that there is no reference to limb-souding data from MLS or HALOE or other instruments. In AR4, I believe there were references, e.g. http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch3s3-4-2-2.html. Has nothing changed then and shouldn't that be said if this is the case? [John Remedios, United Kingdom of Great Britain & Northern Ireland]	Acknowledge.The MLS or HALOE data are in the stratospheric humidity section. No change has been made.
2-1511	2	39	1	39	31	Where is discussion of TOVS/HIRS UTS (ie Shi and Bates recent work?) Perhaps should also reference the recent Dessler and Davis paper shows that the water vapor feedback estimated from NCEP/NCAR is not in good agreement with other reanlyses. [Karen Rosenlof, United States of America]	Acknowledged+H1516. The Shi and Bates paper has been added to the discussion in this paragraph.

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2-1512	2	39	8	39	8	(Brogniez et al., 2009) → Brogniez et al. (2009) [Peter Burt, UK]	Accepted.
2-1513	2	39	8			"Brogniez et al." should not be included within the parentheses [Michael Brewer, United States of America]	Accepted.
2-1514	2	39	9	39	9	lateral mixing: instead of lateral I suggest meridional [Claudio Cassardo, Italy]	Editorial. Accepted.
2-1515	2	39	16	39	24	Here, and elsewhere, is evidence of a schizophrenic attitude toward trends derived from reanalyses. This whole issue needs to be handled more consistently. My preference would be to really limit their role in the chapter. [Dian Seidel, USA]	Editorial. The use and realiability of trends from reanalysis products differs from one vairable to the next.
2-1516	2	39	16	39	30	Simmons et al. 2010 (Simmons, A. J., Willett, K. M., Jones, P. D., Thorne, P. W. and Dee, D., 2010: Low-frequency variations in surface atmospheric humidity, temperature and precipitation: inferences from reanalyses and monthly gridded observational datasets. Journal of Geophysical Research, 115, D01110.) show 850mb fields of recent changes in specific humidity and relative humidity over land. These aren't directly compared with observations though, on the surface fields are compared. [Kate Willett, UK]	This is relevant to 2.3.5.
2-1517	2	39	16			"Partridge et al." should not be included within the parentheses [Michael Brewer, United States of America]	Accepted.
2-1518	2	39	18	39	21	"reanalysis products suffer - -" See the comment above Chap 2 p9. Can we just acept that reanlaysis products are not the answer to anyone's prayers? They are as flawed as most other models. Then there is an extraordinary statement that "radiosondes - - indicate positive trends in the tropospheric relative humidity." They dont, and wishes don't make horses. [Philip Lloyd, South Africa]	See response to 2-1489.
2-1519	2	39	20	39	20	There appears to be a stray/extraneous ")" after 2007. [Michael Mann, USA]	Word won't let me fix this.
2-1520	2	39	26	39	30	The lie oft repeated does not make a truth. Radiosonde data do NOT indicate positive trends, they are NOT consistent the a Clausius-Clapeyron calculation based on a the observed increase in global temperature, and we have a problem. You cannot go round saying there is no problem - if there is a problem, it MUST be faced. [Philip Lloyd, South Africa]	See response to 2-1489.
2-1521	2	39	30	39	30	Given the relative lack of increase in TPW shown in Fig. 2.19 (top) since about 1997, I'd suggest rethinking this "very likely" assessment. Do you anticipate the possibility that AR6 might come to a difference conclusion, and if so, should the statement be more cautious? [Dian Seidel, USA]	Editorial. That's cherry picking the start date. The entire record shows a distinct increase that is statistically significant and strongly correlated to changes in temperature. No change has been made.
2-1522	2	39	32	41	32	cloud cover specially by low and medium clouds under specific conditions related to more precipitations, this fact is absence here, so more attention about this subject and more informations from other places in the world are needed, on the other hand correlation analysis between precipitation and cloud cover data avialabe in this report my give another important points. [ALI GEATH ELJADID, LIBYA]	Editorial. Rejected. The purpose of the chapter is not to point out potentially important areas of research.
2-1523	2	39	34	39	35	About the sentence: "Clouds are important regulators of solar and infrared radiation and can provide potentially important feedbacks on changes in surface temperature." Solar radiation is composed of infrared, visible and UV radiation, so the term “infrared” needs to be specified. For example: Earth surface infrared radiation. [Rubén D Piacentini, Argentina]	Accepted. Changed "infrared" to "terrestrial".
2-1524	2	39	36		37	This Paltridge study is based on NCEP reanalyses which were shown by Trenberth et al 2005 to be totally inadequate over oceans and it must be discounted. Unrealistic trends in reanalyses for water related variables is documented in Trenberth et al 2011 J Clim see comment chapt 2 p 10 for full ref. [Kevin Trenberth, USA]	Accepted. Citation added.
2-1525	2	39	45	39	45	This sentence doesn't seem to reflect the rest of the section accurately. Results for Canada and China were confirmed, but those for Australia were not, and North America as a whole showed no change in Warren et al. 2007. [Melissa Free, USA]	Acknowledged. This sentence has been changed to indicate both the confirmation and challenges made to the previous work.
2-1526	2	39	45	40	5	For Australia, a new homogeneity-adjusted cloud dataset showed no significant changes in total cloud since the mid-twentieth century. Jovanovic, Collins, Braganza, Jakob and Jones, 2010, A high-quality monthly total cloud amount dataset for Australia, Climatic Change, DOI 10.1007/s10584-010-9992-5. [Melissa Free, USA]	Acknowledged. This work is now included in this paragraph.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1527	2	39	45	40	5	Sun, B., T.R. Karl, and D.J. Seidel, 2007: Changes in cloud ceiling occurrence frequencies and ceiling heights over the United States since the early 1950s. J. Climate, 20, 3956-3970, DOI: 10.1175/JCLI4213.1 showed increases in ceiling heights in the US below 3.6 km but not above 3.6 km. [Melissa Free, USA]	Rejected. This work is already cited.
2-1528	2	39	45	40	32	The sentence on line 45 suggests that what was understood about changes in clouds at the time AR4 was written is still the case, supported by stronger evidence. But the rest of this section, especially the paragraph beginning on p 2-40 line 13, brings up a lot of areas of uncertainty and unresolved questions. Please reconsider the language. [Dian Seidel, USA]	Acknowledged. See 2-1525
2-1529	2	39	48	39	48	I suggest adding the following after "…1971-2000": "(compared to the period 1941-1970)." [Martin Hovland, Norway]	Accepted.
2-1530	2	39	51	39	51	Replace “Duan and Wu, (2006) documented…” with “Duan and Wu (2006) documented…” [Alice Grimm, Brazil]	Accepted.
2-1531	2	39	54			Punctuation is needed ‘previously documented for China’ [Yutaka Kondo, Japan]	Accepted.
2-1532	2	40	1	40	5	This phenomenon which may be connected with an increased lapse rate and, in turn, more unstable conditions in the troposphere has very important consequences: More convective instead of stratiform clouds may lead to a positive feedback of global (ore regional) warming. [Christian-D. Schoenwiese, Germany]	Editorial. Discussion of the feedback is relevant to cloud feedback or future projections, not observations. No change has been made.
2-1533	2	40	1		17	The problems with clouds over the US and with satellite records is given in Dai, A., T. R. Karl, B. Sun, K. E. Trenberth, 2006: Recent trends in cloudiness over the United States: A tale of monitoring inadequacies. Bull. Am. Met. Soc., 87, 597-606. The work that goes through only 1997 (line 14) falls short. The reasons for the discrepancies have indeed been resolved (line 17) see Dai et al 2006 and the satellite record (ISCCP) is being reprocessed under GEWEX. A new version should be out in 2012. Watch for it. [Kevin Trenberth, USA]	Rejected. Dai et al. did not entirely resolve this discrepancy. They concluded that military stations showed inadequate spatial sampling over the US, but did not reconcile why satellite and NWS stations show conflicting trends except to note that both data sets have substantial inadequacies.
2-1534	2	40	2	40	3	Please quantify "large decrease" and "small decrease". [Dian Seidel, USA]	Accepted. The trends are now cited explicitly.
2-1535	2	40	3			The year of publication is missing [Yutaka Kondo, Japan]	Accepted.
2-1536	2	40	7	40	8	In this context or elsewhere the hypothesis sholud be discussed that a varying level of cosmic rays intensity linked with solar activity may influence aerosol nucleation and, in turn, cloud formation. See e.g. J. Kirkby (and many co-authors), 2011: Role of sulphuric acid, ammonia and galactic cosmic rays in atmospheric aerosol nucleation. Nature, 476, 429-433. [Christian-D. Schoenwiese, Germany]	Editorial. The subject of possible physical mechanisms are not part of this chapter, but is discussed in Chapter 7.
2-1537	2	40	7	40	11	It would be useful to include both the citation and what data set was used to reach the conclusions stated. [Karen Rosenlof, United States of America]	This comment is unclear. The citations are already listed.
2-1538	2	40	8	40	11	There is something wrong with Sun et al 2007, because between ~1940 and ~1980 the surface temperature did NOT increase. Perhaps delete this section - it sounds like grasping at straws. [Philip Lloyd, South Africa]	Rejected. The decades noted are 1950s to 1990s during which the surface temperature did increase. No change has been made.
2-1539	2	40	13	40	15	The two sentences here suggest that the AR4 conclusions on trends in cloud cover have not been based on satellite observations [Klaas Folkert Boersma, Netherlands]	Comment unclear. There is no suggested change No change has been made.
2-1540	2	40	15	40	15	It is not accurate to say "surface observers reported increasing trends", since surface observers take individual measurements and it is not until many years later that other people might report trends. This is part of what makes this work so troublesome. Scientists interested in trends do not control the basic experimental design or measurement protocol, and those making the measurements may have no clue that their observations will be used in this manner. All this undermines quality control in a way that would put experimentalists in other fields to shame. [Dian Seidel, USA]	Acknowledge. This has been changed to "surface observations indicate positive trends"
2-1541	2	40	25	40	26	This sentence is not rigorous enough for AR5. To whom did the changes "appear more credible"? [Dian Seidel, USA]	AR4
2-1542	2	40	40	40	40	Should "all datasets" be "all satellite datasets"? [Dian Seidel, USA]	Accepted. "satellite" is now added

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1543	2	40	53	40	53	Refer to the specific cloud section of either the most recent State of the Climate: Foster, M., S. A. Ackerman, A. K. Heidinger, and B. Maddux, 2011: [Global Chapter] Global Cloudiness [in State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S45-S46 or the State of the Climate 2009 one that is pointed to by Baringer et al. 2010. [Kate Willett, UK]	Accepted.
2-1544	2	40	55	40	55	has → have [Peter Burt, UK]	Accepted.
2-1545	2	40				of these parameters. I find that hard to believe. I suggest that it be made clear on what the AR4 conclusions w.r.t. Cloud cover were [Klaas Folkert Boersma, Netherlands]	Truncated comment is not clear what is being referred to.
2-1546	2	40				based upon. [Klaas Folkert Boersma, Netherlands]	Truncated comment is not clear what is being referred to.
2-1547	2	41	1	41	1	Refer to the specific cloud section of either the most recent State of the Climate: Foster, M., S. A. Ackerman, A. K. Heidinger, and B. Maddux, 2011: [Global Chapter] Global Cloudiness [in State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S45-S46 or the State of the Climate 2009 one that is pointed to by Baringer et al. 2010. [Kate Willett, UK]	Duplicate of 2-1543
2-1548	2	41	1	41	3	On a regional scale such long-term trends in cloud optical properties do exist and appear to be linked to changes in aerosol. See e.g. the two references inserted below. [Bennartz Ralf, US]	Acknowledged. These results are now included in this paragraph.
2-1549	2	41	1	41	3	Bennartz, R., J. Fan, J. Rausch, R. Leung, A. Heidinger, 2011: Pollution from China increases cloud droplet number, suppresses rain over the East China Sea. Geophys. Res. Lett., 38, L09704, doi:10.1029/2011GL047235. [Bennartz Ralf, US]	See 2-1548
2-1550	2	41	1	41	3	Qian, Y., D. Gong, J. Fan, L. R. Leung, R. Bennartz, D. Chen, and W. Wang, 2009: Heavy pollution suppresses light rain in China: Observations and modelling. J. Geophys. Research, doi:10.1029/2008JD011575. [Bennartz Ralf, US]	See 2-1548
2-1551	2	41	5	41	7	The attribution of SST variations to the PDO here contradicts the earlier instistance (page 7) that the chapter will not engage in any "attempt to further interpret the observed changes in terms of multidecadal oscillatory variations" [Michael Mann, USA]	Acknowledged.This has been changed to "on these time scales".
2-1552	2	41	7	41	7	SLP not yet defined [Dian Seidel, USA]	Accepted. SLP is now written explicitly.
2-1553	2	41	8	41	10	It does not make sense - less cloud = higher SST = positive cloud feedback. Negative feedback, surely - more cloud = less temperature increase. [Philip Lloyd, South Africa]	Rejected. For low clouds the reflection of solar radiation dominates that thermal absorption, so less clouds is a positive feedback. No change has been made.
2-1554	2	41	12		18	The errors in Spencer and Braswell were first documented by Trenberth et al 2011b: Trenberth, K. E., J. T. Fasullo, and J. P. Abraham, 2011: Issues in establishing climate sensitivity in recent studies. Remote Sensing, 3(9), 2051-2056, doi: 10.3390/rs3092061. [Kevin Trenberth, USA]	Rejected. The errors there are documented in several studies prior to this which are already cited.
2-1555	2	41	13	41	13	time-periods → periods (remove tautology) [Peter Burt, UK]	Accepted.
2-1556	2	41	21	41	23	I don't understand the formulation of this sentence. The trends are bound to be within their uncertainties by definition ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The text refers to the observational uncertainties in the data set, not the statistical uncertainties in the regression. (If its not too late, the text in the report could be changed to read "... what trends do exist are likely to be within the OBSERVATIONAL uncertainties for both ..."
2-1557	2	41	22	41	22	Should "satellite and observational" be "satellite and ground-based"? [Dian Seidel, USA]	Accepted. Changed to "surface-observed"
2-1558	2	41	22			Confusing to refer to both satellite and observational data. Satellite are observations (e.g. as opposed to models, though I know that's not what you're intention is here). [Drew Shindell, USA]	Accepted. Changed "observational" to "surface-observed"

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1559	2	41	25			Only in the summary do the authors acknowledge “substantial uncertainty” attributable to “large interannual variability, coupled with either short time series or uneven spatial sampling”. They should add decadal to multi-decadal variability as a source of uncertainty. This scale of uncertainty is more likely to be lead to spurious trends than the uncertainty created by variability at an annual scale. [David Sauchyn, Canada]	Editorial. Rejected.
2-1560	2	41	27	41	41	I think you should mention the reduction in relative humidity over land as it is an important conclusion that gets through to the ES. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted, the ES summarises several results descripted in the sections
2-1561	2	41	31	41	32	Elsewhere, the mid-latitudes - - " I have looked for the evidence and have failed to find it. Such a broad statement needs referencing - where has this been shown?? [Philip Lloyd, South Africa]	It is shown int he Figure 2.28
2-1562	2	41	32	41	32	It would be useful to insert here the conclusion that, compared to the conclusions in the AR4, there are many fewer statistically significant precipitation trends at the grid box level because many of the areas that showed statistically significant long term trends in the AR4 show opposite trends between the 1901–2010 period and 1979–2010 period (see beginning of page 2-33. [Alice Grimm, Brazil]	Noted, but it is too detailed for the summary
2-1563	2	41	33	41	34	Please delete "…one of..." from "water vapour is one of the most abundant greenhouse gases". As noted elsewhere in the draft (e.g., 2-35, lines 52-53) and in countless papers, water vapour is the single most abundant greenhouse gas, with a mean total column global abundance of about 24.5 mm at STP. No other greenhouse gas comes close to this. Consider that the global mean of the entire ozone layer is only 3 mm (STP). [Forrest Mims, USA]	Comment not corresponding to the section
2-1564	2	41	35	41	37	What is this? Another repetition of the lie? Who is responsible for this nonsense? [Philip Lloyd, South Africa]	comment not understood
2-1565	2	41	35	41	41	It does not seem that the plots shown (ie 2.18b) are convincing in demonstrating that water vapour has been increasing. I see a jump around 1997, then a relaxation almost to a previous value, not a trend in the land values. The marine values also show jumps, but perhaps a more convincing trend like reature. [Karen Rosenlof, United States of America]	Noted, the figure has changed to 2.30
2-1566	2	41	38	41	41	This statement is not consistent with the information in the preceding section. North America and Australia did not show significant changes in the most recent work. The statement at the end of the preceding section, that large-scale trends are likely to be within the range of uncertainties, seems more accurate and prudent. [Melissa Free, USA]	Noted, a sentence has been added to emphasize the statement.
2-1567	2	41	43			Section 2.4: It would help a lot if this section had a summary in the end [Uwe Stoeber, Germany]	Any statement that brings together all species described in 2.4 would involve RF and is more appropriate for Ch. 8.
2-1568	2	41	45	41	45	Say which gases are concerned at the beginning of section 2.4 [Katharine Law, France]	Rejected: It is a long list of species, and each sub-section lists them.
2-1569	2	41	45	53	12	Several places in section 2.4.1 and 2.4.2 radiative forcing values are discussed. Some of the statements are not consistent with Chapter 8 and some examples will be given below. Ch2 and Ch8 should discuss this overlap. [Gunnar Myhre, Norway]	All information that is shared between chapters 2 and 8 will be made consistent in the SOD.
2-1570	2	41	45			Section 2.4.1 Long Lived Greenhouse Gases. This revision is focused to the section assessing surface observations of long-lived greenhouse gases (LLGHG). Data of three main monitoring programs have been considered. All data series agree quite well except for CCl4 and HCFC-142b. Data of the major LLGHG, such as CO2, CH4 and N2O, are available since long time and cover a large area. Moreover their measurement techniques are consolidated and consistencies among the standard scales are well assessed. In contrast less information are available about trace gas measurement methods, standard scales and the area covered by the monitoring programs taken into account. I suggest to better specify this item. [Florinda Artuso, Italy]	Unfortunately, measurement techniques go beyond the scope of IPCC AR5.
2-1571	2	41	45			Section 2.4.1 should assess the quality/usefullness of the different measurement sources. Rather than quoting 3 values (e.g. in table 2.12), either one of the 3 should be picked, or a central value with a range. It appears from the text that NOAA is preferred for CO2, CH4, N20; and AGAGE preferred for the halogenated species. Is it necessary to quote UCI at all? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Preferences for any particular measurement program are removed.UCI measurements are not discussed because they were received too late. Chapter 8 now has a list of single annual means to use for RF calculation.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1572	2	41	47	41	53	Why introduce two sets of paired time periods for comparison? It would be simpler to keep the same ones. [Dian Seidel, USA]	We were asked to contrast the period used for AR4 with that for AR5.
2-1573	2	41	47	42	2	Is the comment about observational networks being sufficient specific to CH4? Even if it is, it is a questionable statement and inconsistent with what is written later in the section on CH4 (as well as other gases). In any case any such comment would be better after the discussion about sources and sinks. Uncertainties in the sinks need to be addresses somewhere. [Katharine Law, France]	The comment generally applies to all gases and is appropriate in this section on observations.
2-1574	2	41	47	42	2	It would seem important to at least mention the most important trace gases with respect to radiative forcing in this introductory paragraph. For example, I think it is crucial to mention CO2 here: increases in CO2 concentrations continue, and they continue to dominate the increase in radiative forcing. Increases in CH4 are interesting but have added only 1/15th to radiative forcing over the past 5 years compared to CO2. [Stephen Montzka, USA]	Accepted and text changed accordingly.
2-1575	2	41	48	41	51	Is it relevant that ozone depleting substances have decreased while greenhouse gas concentrations increased? To my opinion this [Klaas Folkert Boersma, Netherlands]	ODSs are GHGs.
2-1576	2	41	48	41	52	The use of a percentage increase for statements such as "resulting in a 6% increase in radiative forcing from 2005 to 2010" seems a little odd to me: it is the percentage increase not in GHG forcing, but in the additional anthropogenic forcing from GHGs. I think it would be cleaner to just state the increase in W/m2 over that time period, and, if necessary for context, compare it directly to the total W/m2 of anthropogenic forcing. And somewhere the rate of increase in W/m2/year should be compared for the 1998 to 2005 vs. the 2005 to 2010 periods... [Marcus Sarofim, USA]	Chapter 8 will report changes in RF; our focus is LLGHGs and this nicely summarizes the total change in climate-relevant gases.
2-1577	2	41	48			Space after period. [Martin Vollmer, Switzerland]	OK
2-1578	2	41	49			add "stratospheric" before "ozone" [Ruth Doherty, UK]	Rejected.
2-1579	2	41	51	41	51	Helpful to have the date for the Montreal Protocol [Peter Burt, UK]	1987 added.
2-1580	2	41	52	41	52	Add "further" before "6%" [Dian Seidel, USA]	Rejected.
2-1581	2	41	52	41	54	I presume this is computed as 2010 minus 2007 to get the average. How variable is that? If one year were added, what might that do to the average rate? [James Butler, United States of America]	No, it is RF for 2011 minus RF for 2005. It is the difference, not the absolute rate.
2-1582	2	41	52	41	54	I suggest to mention also the rate pre-1999 in order to compare the actual one with the one before the period 1999-2006. [Claudio Cassardo, Italy]	The point is made to emphasize a factor of 10 increase in the the growth rate of CH4 since AR4.
2-1583	2	41	52	41	55	It is written: "Understanding of the global CH4 budget is not good enough to determine if this recent period is anomalous or a return to the rates of increase observed prior to 1999." The rates in the 1990s were widely variable and can not be used as reference. Suggest to formulate this statement as:"Understanding of the global CH4 budget is not good enough to judge if the period of limited growth in methane between 1999 and 2006 has been an anomaly or that the renewed growth since 2007 will be of a temporary nature" [Michiel van Weele, The Netherlands]	Sentence deleted.
2-1584	2	41	53	41	53	Should include uncertainty ranges on the methane trends and indicate the data source (NOAA I guess) [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected: the point is an oder of magnitude increase in the CH4 growth rate. From table 2.1, it is clear that all programs agree well in annual means and rates of increase, so the result would be the same regardless of which program's global means were used.
2-1585	2	41	54	41	55	Replace "Understanding of the global CH4 budget is not good enough to determine if this recent period is anomalous" with "Current understanding of the global CH4 budget is insufficient to determine if this recent change in growth rate is anomalous". [Robert Waterland, United States of America]	Sentence deletd and left for Chapter 6 to discuss.
2-1586	2	41	54			add "Current" before "understanding" [Ruth Doherty, UK]	Not needed.
2-1587	2	41	54			Suggest to replace 'not good enough' by 'insufficient' [Martin Vollmer, Switzerland]	Sentence deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1588	2	41	54			This sentence is confusing, of course the rates of increase are back to those prior to 1999, this can be seen in fig. 2.21. What the authors maybe want to say is that our understanding of the budget is insufficient to say whether these elevated rates of increase will persist over the near future, or they want to say that the budget is insufficient to explain whether these recent increases are caused by the same sources/sink structures [Martin Vollmer, Switzerland]	This discussion has been removed.
2-1589	2	41		57		General comment for Chapter 2: Most of the places PM10 and PM2.5 are written as PM10 and PM2.5. I am not sure if the numbers should be made in subscript. Also, the use of ug m-3 should be checked. I suggest repleacing "ug m–3" => "µg m–3" [Yutaka Kondo, Japan]	Editorial
2-1590	2	41				part could be removed. [Klaas Folkert Boersma, Netherlands]	If this refers to ODS (1575), it can not be removed.
2-1591	2	41				section 2.3.8. It is disappointing to see no discussion either here or elsewhere of the strong correlations between temperatures and precipitation, as discussed in Trenberth and Shea 2005 Trenberth, K. E., and D. J. Shea, 2005: Relationships between precipitation and surface temperature. Geophys. Res. Lttrs., 32, L14703, doi:10.1029/2005GL022760 and Trenberth 2011: Trenberth, K. E., 2011: Changes in precipitation with climate change. Climate Research, 47, 123-138, doi:10.3354/cr00953. Actually I do see these in the last section p 86 but their basis is not established. [Kevin Trenberth, USA]	Refers to section 2.3.8.
2-1592	2	42	1	42	1	in what way(s) are they insufficient to accurately estimate regional emissions ? Spatial density? Frequency of measurements? [Dale Hurst, United States of America]	Insufficient space to describe in detail.
2-1593	2	42	4	42	4	"The abundances of all LLGHGs reported here …" [Dale Hurst, United States of America]	Reworded.
2-1594	2	42	4	42	4	2.79 W/m2, based on which period? [Christian-D. Schoenwiese, Germany]	Added that RFrelative to 1750.
2-1595	2	42	4	42	4	Where is "reported here" referring to? Table 2.12? Specify. Also, give a year for the total radiative forcing number. [Dian Seidel, USA]	1750 added.
2-1596	2	42	4	42	4	Radiative forcings are meaningless unless a time period is specified - I believe this values is relative to the pre-industrial concentrations [Keith Shine, UK]	1750 added.
2-1597	2	42	4	42	4	Replace "The abundances reported here are used to calculate radiative forcing, which totals 2.79 W m–2, in Chapter 8." with "The abundances reported here are used in Chapter 8 to calculate LLGHG radiative forcing, which totals 2.79 W m–2." [Robert Waterland, United States of America]	Change accepted.
2-1598	2	42	4		4	the radiative forcing mentioned here (2.79 W/m2) needs a date associated with it [Stephen Montzka, USA]	1750 added.
2-1599	2	42	4			"abundances reported " - be more specific [Ruth Doherty, UK]	Changed "abundances" to " mole fractions".
2-1600	2	42	5	42	8	I don't think "total" is necessary in these phrases. This redundancy may occur elsewhere. [James Butler, United States of America]	Total insures inclusion of natural and anthropogenic sources.
2-1601	2	42	6	42	6	change "production" to "in situ production" - "production" includes industrial manufacture, of SF6 for example, which is not necessarily emitted to the atmosphere the year it was "produced". [Dale Hurst, United States of America]	The point is that in many cases industrial production = emissions.
2-1602	2	42	8	42	8	"global means for LLGHGs" - global means of what? [Dale Hurst, United States of America]	Clarified.
2-1603	2	42	8	42	8	What quantity is meant by "global means"? Do you mean "global mean concentrations"? [Dian Seidel, USA]	Clarified.
2-1604	2	42	9	42	9	"of increase or decrease" [Dale Hurst, United States of America]	Increase can be negative.
2-1605	2	42	9	42	12	replace the word 'emissions' by 'sources' (except 'Emissions on l. 11) and the replace removal by sinks on line 11 [Martin Vollmer, Switzerland]	Sources is used to refer to the processs, not emissions. Sink is jargon and not clear to non-experts, so we use "removal" or "loss".
2-1606	2	42	9			rateS of increase OR DECREASE (trend) …. [Martin Vollmer, Switzerland]	OK as is.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1607	2	42	10	42	10	It may be opportune to mention that the equality of emissions and sinks (steady state), when the trend is null, does not necessarily mean that they are also in (static) equilibrium. [Claudio Cassardo, Italy]	Not sure what this comment refers to.
2-1608	2	42	11	42	11	This discusses the "lifetime" of LLGHGs. Avoid using "lifetime" due to confusion between adjustment time and residence time. Use these latter terms instead, where appropriate. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	It is clear from the context that budget lifetime is referred to.
2-1609	2	42	11	42	12	There is no mention here of the LLGHGs that are totally anthropogenic. "Factories" and automobiles, etc, are "surface sources" [James Butler, United States of America]	Emissions of all other LLGHGs are described in this section.
2-1610	2	42	16	42	16	I don't think the cited reference is where "bottom up" and "top down" emission estimates were first defined in the literature [Dale Hurst, United States of America]	Text deleted.
2-1611	2	42	17	42	17	"and rely on process-specific emission and scaling factors" [Dale Hurst, United States of America]	Text removed.
2-1612	2	42	18	42	18	Sexist language. Replace "cow" with "cattle" or some other term that includes the male emitters. [Dian Seidel, USA]	Text removed.
2-1613	2	42	18	42	20	More simply the "top down" method uses global trends and removal rates to estimate global emissions [Dale Hurst, United States of America]	Text removed.
2-1614	2	42	23	42	23	I prefer "irreversible removal" instead of "destruction" (because the next clause describes reversible removal) [Dale Hurst, United States of America]	Text deleted.
2-1615	2	42	23	42	24	Replace "lifetime is relatively straightforward when the sink processes result in the destruction of the LLGHG, but for processes involving exchange between the atmosphere and other reservoirs the concept is more complicated." with "atmospheric lifetime is relatively straightforward when the sink processes result solely from the atmospheric the destruction of the LLGHG, but for processes involving substantial exchange between the atmosphere and other environmental compartments the concept of lifetime is more complicated." [Robert Waterland, United States of America]	Text deleted.
2-1616	2	42	25	42	25	Unclear if "exchanges" is a noun or verb here, Can you find better wording? [Dian Seidel, USA]	Text deleted.
2-1617	2	42	25		28	it would seem useful to provide a bottom line here, something like "As a result, CO2 is unique among long-lived GHGs because its timescale for removal from the atmosphere cannot be adequately described by a single number." [Stephen Montzka, USA]	Text deleted.
2-1618	2	42	27	42	27	Replace "exchange (see Chapter 6) and play a major role in the long-term uptake of CO2 emitted by fossil fuel" with "exchange (see Chapter 6). Excjhange into the deep oceans plays a major role in the long-term uptake of CO2 emitted by fossil fuel". [Robert Waterland, United States of America]	Text deleted.
2-1619	2	42	28	42	30	Although "photolysis" is mentioned here, there is no delineation of the destruction of N2O, the third most influential, anthropogenic greenhouse gas, nor the major CFC's, which are the #4 and #5 greenhouse gases. The implication in these lines is that all removal is in the troposphere or at the surface. You may want to avoid sending that message. [James Butler, United States of America]	Text deleted.
2-1620	2	42	29	42	30	OH concentrations should also depend on the abundance of O3. [YUGO KANAYA, Japan]	Text deleted.
2-1621	2	42	30	42	31	It is the evolution of the (non-CO2) LLGHGs that depends on the chemistry, not the climate impact [William Collins, United Kingdom of Great Britain & Northern Ireland]	Text deleted.
2-1622	2	42	30	42	31	This final sentence of the paragraph is vague because "impacts" is undefined. [Dale Hurst, United States of America]	Text deleted.
2-1623	2	42	33	42	33	Move "systematic" to precede "atmospheric measurements" rather than "time series". [Dian Seidel, USA]	Deleted "time series".
2-1624	2	42	37	42		Why don't you call it a volume mixing ratio? [Karen Rosenlof, United States of America]	Because they are not volume mixing ratios.
2-1625	2	42	38		40	Here and many other places of 2.4.1: The text has been shortened to the extent that it becomes difficult to	Changed to "dilution by variable amounts…"

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						understand. The text reads "This eliminates dilution effects from variable amounts of water vapor…", which is non-sensical untill one inserts "the influence of" between eliminates and dilution... [Stephen Montzka, USA]	
2-1626	2	42	39	42	39	insert comma aftrer 'Here' [Peter Burt, UK]	OK
2-1627	2	42	42	42	50	All of these figures are from poorly represntative sites, usually over the ocean. There is almost a total absence of measurements on land surfaces so the quoted confidence limits are unreliable. [VINCENT GRAY, NEW ZEALAND]	Nonsense. The figures show global means based on background sites that are most representative for assessing changes in atmospheric composition over large spatial scales.
2-1628	2	42	42	44	16	UCI, AGAGE and NOAA need references to the peer-reviewed literature saying "updated" where appropriate (e.g. for AGAGE data in general use Prinn, Weiss et al, JGR, v105, 2000, updated to 2010). Also should refer to websites where the UCI, AGAGE and NOAA data are archived. [Ronald Prinn, USA]	References are given in the text, where appropriate, when describing individual species, and they are summarized in an appendix.
2-1629	2	42	43	42	44	It would be great if there could be a bit of expansion on how globally averaged abundances are calculated (and why different programs are different): how many measurement stations are involved, are flask measurements and/or mobile aircraft and ship measurements used in addition to permanent stations, are satellite measurements incorporated, are atmospheric chemistry and transport models used to make a global calculation, or is it statistical interpolation between stations, etc. etc.? [Marcus Sarofim, USA]	This information will be in references included in the text or in supplementary material. Inclusion goes well-beyond the scope of the chapter, given the space limitations.
2-1630	2	42	44	42	50	I do not understand the term "absolute uncertainty in their standard scales" [Uwe Stoeber, Germany]	The absolute uncertainty in standard scale propagates all uncertainties in the method the scale is based on (gravimetric, manometric, etc.). Since AGAGE have now provided annual means that exclude the absolute uncertainty in the standard scales, this sentence has been deleted.
2-1631	2	42	44			AGAGE may be defined here as Advanced Global Atmospheric Gases Experiment [Yutaka Kondo, Japan]	AGAGE defined in Table 2.1.
2-1632	2	42	46	42	47	Is "absolute" uncertainty defined in the IPCC lexicon? If not, it is a confusing term, as it is impossible to measure. [James Butler, United States of America]	All reported uncertainties are estimated, not measured. Sentence has been deleted.
2-1633	2	42	48	42	48	What is meant by "reasonably good"? [Dian Seidel, USA]	Reworded.
2-1634	2	42	53	43	1	Table 2.12: There are way too many "significant" figures in the AGAGE and NOAA columns of mixing ratios. The UCI columns have the correct numbers of significant figures. [Dale Hurst, United States of America]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognized were not justified. Data submitters have provided an appropriate number of significant figures for the SOD.
2-1635	2	42	53	43	1	Carbon dioxide has a data-constrained lifetime of 30-95 years (and mostly 30-60 years) based on contemporary emissions and mixing ratio data, as shown in Jacobson, M.Z., Correction to “Control of fossil-fuel particulate black carbon and organic matter,” J. Geophys. Res., 110, D14105, doi:10.1029/2005JD005888, 2005. Such a lifetime is defined in a manner consistently with the lifetimes of other GHGs shown in Table 2.12. The idea that there are multiple lifetimes for CO2 is a misconception. There is one lifetime at a given instant, but that lifetime changes over time due to changing saturation in the oceans, etc (however, two single lifetimes can bound the uncertainty of the actual lifetime at a given instant). The current lifetime and the lifetime since 1960 is constrained by mixing ratio and emissions data and is 30-95, but most consistently 30-60 years based on such data. [Mark Z. Jacobson, U.S.A.]	Loss of atmospheric CO2 is governed by multiple processes as described in chapter 6, and the paper cited in the comment does not change the state of knowledge.
2-1636	2	42	53	43	1	Table 2.12: Lifetimes of many OH-oxidized gases don't include the updated values presented in the WMO-2010 ozone assessment, Chapter 1 (see table 1-3). 100-year GWPs also do not reflect the updated lifetimes (see Chapter 5, Table 5A-1 of the same report). Notes to this table should include citations to these sources. [Stephen Montzka, USA]	Lifetimes and GWPs will be based on work from chapter 8 on radiative forcing.
2-1637	2	42	53	43	12	Since this chapter is about observations I don't think GWP values should be given in table 2.12. The updating of GWPs are based on several factors which are dealt with in Chapter 8. A reference to chapter 8 could be	There is over-whelming support for keeping GWPs in Table 2.1 (formerely 2.12).

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						given instead. If it is decided to keep GWP values in table 2.12, more coordination with chapter 8 is needed. [Jan Fuglestvedt, NORWAY]	
2-1638	2	42	53	43	12	I am not sure all the significant figures are needed, as it makes the table look very cluttered and hard to assimilate. More, I do not understand the reason for including the GWP in this table - if anything is to be included, it would seem much better to include the radiative forcing (present day minus pre-industrial) as this is a better indicator of current climate importance - the GWP is meaningless unless one knows the emissions [Keith Shine, UK]	Initially, GWPs were not included in Table 2.12, but we were asked to include them during review of the ZOD to make the table more inclusive. RF will be in Chapter 8.
2-1639	2	42	53	43	14	The GWP values in Table 2.12 is not consistent with Ch8 [Gunnar Myhre, Norway]	Initially, GWPs were not included in Table 2.12, but we were asked to include them during review of the ZOD to make the table more inclusive. They will be consistent with Chapter 8.
2-1640	2	42	53	43	20	Table 2.12 needs some revision. It should be made clear that the GWP values are referenced to carbon dioxide with a 100-year time horizon, and the column titles "2010-2005" should be replaced by "Changes from 2005-2010". [Robert Waterland, United States of America]	It already states 100 yr GWP and the lack of a value for CO2 makes it clear that is the reference. Will leave other column heads as they are.
2-1641	2	42	53			Table 2.12: The units of the changes should be indicated (per year, per decade?) [Uwe Stoeber, Germany]	It is clear that the changes are for the specified periods.
2-1642	2	42	53			Table 2.12: I do not see the benefit of reporting so many digits. Apart from that are not significant due to the large uncertainties, they also make reading much harder. [Uwe Stoeber, Germany]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognized were not justified. Data submitters have provided an appropriate number of significant figures for the SOD.
2-1643	2	42	53			This table contains only a selection of compound, this should be mentioned and maybe an explanation on the selection process for the selected compounds. [Martin Vollmer, Switzerland]	They are the key LLGHGs that contribute to RF.
2-1644	2	42	55	43	1	Update table (and associated discussion in this section) with 2011 numbers. [Michael Mann, USA]	2011 global annual means were not ready for the FOD.
2-1645	2	42	55			Table 2.12, “CH4 AGAGE”. Is it necessary to put so many figures? For me 2 significant figures for the uncertainty is enough. [Francois DANIS, France]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1646	2	42	55			I cannot find where GWP is defined [Larry Thomason, United States of America]	Will add definition.
2-1647	2	42		43		Table 2.12: Mole fractions of LLGHGs may be expressed in 1 decimal point. [Yutaka Kondo, Japan]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognized were not justified. Data submitters have provided the appropriate number of significant figures for the SOD.
2-1648	2	42		43		<Table 2.12> Too many siginificant figures after decimal points for AGAGE data. For example, 1797.398 ppt for methane could be 1797.4 ppt. [Yoko Yokouchi, Japan]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognized were not justified. Data submitters have provided the appropriate number of significant figures for the SOD.
2-1649	2	42				Table 2.12: The species lifetimes and GWP100 are listed in Chapter 8. Are they useful here as well? If so, they need to be consistent with chapter 8. [William Collins, United Kingdom of Great Britain & Northern Ireland]	All information that is shared between chapters 2 and 8 will be made consistent in the SOD. Many values in chapter 2 FOD were placeholders. Initially, GWPs were not included in Table 2.12, but I was asked to include them during review of the ZOD

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							to make the table more inclusive.
2-1650	2	42				Table 2.12: Numbers are quoted to too many decimal places, given the uncertainties in the numbers. [William Collins, United Kingdom of Great Britain & Northern Ireland]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognized were not justified. Data submitters have provided the appropriate number of significant figures for the SOD.
2-1651	2	42				Table 2.12: The species list and naming convention should be harmonised with table 8.5 in chapter 8 [William Collins, United Kingdom of Great Britain & Northern Ireland]	All information that is shared between chapters 2 and 8 will be made consistent in the SOD. Many values in chapter 2 FOD were placeholders.
2-1652	2	43	1	43	1	Obviously the GWPs and lifetimes should be updated to be consistent with the radiative forcing chapter. [Marcus Sarofim, USA]	All information that is shared between chapters 2 and 8 will be made consistent in the SOD. Many values in chapter 2 FOD were placeholders.
2-1653	2	43	11	43	11	"pre-industrial" instead of "pre-indistrial" [Florinda Artuso, Italy]	OK
2-1654	2	43	11	43	11	"industrial" is mis-spelled. [James Butler, United States of America]	OK
2-1655	2	43	11	43	11	"not measurable for all species" is unclear. Should it be for "any species"? [Dian Seidel, USA]	Reworded.
2-1656	2	43	11	43	11	"not measurable" - this is unclear - I believe it means "they are close to zero", rather than it is technically impossible to measure them. Certainly C2F6 has been measured (Worton et al. Environ. Sci. Technol. 2007, 41, 2184-2189) and been found to be <0.3 pptv, and I believe this is the case for most if not all of the other molecules on this list. [Keith Shine, UK]	Reworded to clarify.
2-1657	2	43	11			typo, and the text "are not measurable" does not allow one to distinguish between this being a comment about some measurement being below detection to having their be no measurement yet made. [Stephen Montzka, USA]	Reworded to clarify.
2-1658	2	43	15			Fig 2.20: seems highly desirable to show record of CO2 since 1958 so that the changes in slope and accelerated rise is apparemt in recent years. [Kevin Trenberth, USA]	SIO globally averaged CO2 included back to 1958.
2-1659	2	44	6	44	8	The source of these data is not clearly identified in the caption. Is it NOAA? AGAGE? UCI? A composite? [James Butler, United States of America]	Sources of data are now clear in the caption.
2-1660	2	44	10	44	16	In the legenda of fig. 2.23 it is not clearly explained what is represented in the two parts of the figure. [Claudio Cassardo, Italy]	The plots are the same except for the range of the y-axis; it needs no further explanation.
2-1661	2	44	11	44	11	It is not clear from figure caption, the diffeence between upper and lower panels [Celeste Saulo, Argentina]	The plots are the same except for the range of the y-axis; it needs no further explanation.
2-1662	2	44	11	44	16	This figure is incomplete, there is a recent publication by Vollmer et al., 2011 (JGR, doi: 10.1029/2010JD015309) which now includes AGAGE HFC-227ea and HFC-236fa measurements. Even though this are minor HFCs, they should be added, particularly because they are part of the UNFCCC CRF tables, while other substances (eg. HFC-365mfc and HFC-245fa) are not part of the CRF tables. [Martin Vollmer, Switzerland]	As stated in the caption, the figure is not meant to be an exhaustive presentation of all halogen-containing species.
2-1663	2	44	18	48	24	Section 2.4.1 -- Montreal Protocol gases and other LLGHGs. It would be helpful to the reader if the lifetimes of these gases were included in the descriptions of their amounts and trends. [James Butler, United States of America]	The lifetimes are included in Table 2.12.
2-1664	2	44	21	44	24	The two sentences in this paragraph are not well connected so the paragraph seems disjointed. [Dian Seidel, USA]	Section deleted.
2-1665	2	44	21		24	this paragraph could be readily misconstrued without some emphasis to point out that you are NOT talking about radiative forcing here, but the contribution and influence of all the CO2 and other GHGs (from natural and anthropogenic sources) in the atmosphere. In other words, the term "total greenhouse effect" is not a	Paragraph deleted.

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						common enough term to include it withoutsuggest it doesn't need some explicit definition. [Stephen Montzka, USA]	
2-1666	2	44	22	44	22	Rather than "sustain", I suggest "drive changes in" as more descriptive of the role of the LLGHG [JOHN OGREN, USA]	Text has been deleted.
2-1667	2	44	24			'begun'=> started [Yutaka Kondo, Japan]	Corrected.
2-1668	2	44	26	44	36	The IPCC has suppressed all mention of the measurements of atmospheric carbon dioxide made before 1958. These were published in peer reviewed Journals and sime of the authors were Nobel Prizewinners. You seem to be suggesting that this suppression is justified because they were not accurate, but no detailed claim has been published that this may be so. They were certainly carried out to the best levels of accurracy available at the time. Details have been published by Beck, E-G, 2007. 150 Years of Atmospheric Gas Analysis by Chemical Methods, Energy and Environment 18 259-281. It is a scandal that all this early work has been ignored and it compels the belief that full analysis of this work will be an embrassment to the "greenhouse"..theory of the climate and to the calculations that are made in this chapter. Beck's paper should be present in the bibliography [VINCENT GRAY, NEW ZEALAND]	The measurements described by Beck are not comparable with modern measurements (see Keeling, 2007 and Meijer et al., 2007). The period prior to 1958 is nicely covered by measurements of CO2 and other LLGHGs in air extracted from firn and ice cores.
2-1669	2	44	26	44	36	There is a jumble of CO2 growth rates and mixing ratios presented in this paragraph, seemingly in no particular order, making it difficult to assess how recent CO2 growth compares to the prior decade and further back in time. [Dale Hurst, United States of America]	Comment rejected.
2-1670	2	44	27			Uses of 'have since been' [Yutaka Kondo, Japan]	Not sure of the point of the comment.
2-1671	2	44	28	44	28	I think it better to add a sentence to show WMO/GAW activity of calibration, data collection and quality auurance about CO2 concentration measurement (http://www.wmo.int/pages/prog/arep/gaw/documents/GAW_194_WMO_TD_No_1553_web_low_resol.pdf). [Takashi Maki, Japan]	This comment goes beyond the scope of AR5.
2-1672	2	44	28	44	28	I consider it suitable to add a figure which shows current WMO/GAW CO2 monitoring network(http://gaw.kishou.go.jp/wdcgg.html). [Takashi Maki, Japan]	Space limitations prevent inclusion of such details. Readers will need to consult cited literature.
2-1673	2	44	28	44	28	I think it better to add a sentence to show insufficiency of current CO2 monitoring network. [Takashi Maki, Japan]	To assess radiative forcing, the current network is sufficient.
2-1674	2	44	30	44	30	What are "the NOAA CO2 data" ? NOAA produces lots of CO2 data. [Dale Hurst, United States of America]	Clarified.
2-1675	2	44	30	44	30	I think it better to show the statistical analysis result of CO2 such as WMO GHG Bulletin (http://www.wmo.int/pages/prog/arep/gaw/ghg/documents/GHG_bull_6en.pdf). [Takashi Maki, Japan]	It is impossible to insure that all programs submitting data to the WDCGG were measured on standard scales that are traceable to the WMO GAW CCL. Also, WDCGG does not properly weight data for its atmospheric "footprint" and can not be included.
2-1676	2	44	30	44	30	Please explain the scope of the "NOAA" CO2 data. Does this mean only data measured in GMD's laboratory, or does it include the GlobalView synthesis of measurements by multiple laboratories around the world? Given the importance of the CO2 trend analysis, I strongly recommend that the GlobalView synthesis be used, and that the discussion be expanded to explain that the CO2 results and uncertainties presented here represent a synthesis of measurements by many laboratories around the world. [JOHN OGREN, USA]	Text deleted.
2-1677	2	44	30	44	31	"and the 12-month increase was 2.37 ± 0.15 ppm." For comparison purposes, the increase since AR4 should be presented as a mean growth rate. This comment is applicable to all other reported increases and decreases over multiple-year periods. [Dale Hurst, United States of America]	Reworded.
2-1678	2	44	30	44	31	The statement is made "the increase was 2.37 ± 0.15 ppm.". Is this the global average for the year (i.e. over all seasons)? If so, I think that should be explicitly stated. It should be explained how this increase relates to the growth rate shown in Fig. 2.20b, which shows substantial variability in the growth rate during 2010, and the growth rate was near 2.37 ppm/yr only for the maximum during 2010. [David Parrish, USA]	Reworded.

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2-1679	2	44	30	44	31	“In 2010, globally averaged CO2 was 388.54 ± 0.16 ppm, and the increase was 2.37 ± 0.15 ppm.” In this sentence the increase from which year is year referred? [Lokesh Kumar Sahu, India]	Reworded.
2-1680	2	44	30	44	31	Why is it important to report the change for 2010? Consider omitting this tidbit. [Dian Seidel, USA]	Text deleted.
2-1681	2	44	30	44	33	increase in CH4 with respect to what ?? [Yutaka Kondo, Japan]	This section is about CO2. Text removed.
2-1682	2	44	30			The statement is made "In 2010, globally averaged CO2 was 388.54 ± 0.16 ppm,". Is this the global average for the year (i.e. over all seasons)? If so, I think that should be explicitly stated [David Parrish, USA]	Removed.
2-1683	2	44	31	44	31	the increase was 2.37 ± 0.15 ppm: with respect to the previous year? [Claudio Cassardo, Italy]	It is the increase in 2010. Reworded.
2-1684	2	44	31			First 'ppm' on this line should be 'ppm yr-1' [Drew Shindell, USA]	Per yr is implied.
2-1685	2	44	34	44	36	Such a factor as large as 2.9/0.67 = 4.3, even 2.93/0.48 = 6.1 measured at Mauna Loa, between increases of CO2 during the years 1998 and 1992 suggests a specific and important discussion. It is presumably not related to variability of anthropogenic emissions from fossil fuels. It is presumably not only related to variability of photosynthesis and land respiration either, for the reasons that follow. Remember that 1998 was a relatively hot year with an exceptionally large El Niño, whereas 1992 was a cold year related to the aerosols emitted after the volcanic eruption of the Mount Pinatubo. Remember also that the solubility of CO2 in water depends on temperature, the solubility being larger at lower temperature. The temperature dependence of the solubility is about 1.4 % per degree C around 15°C. [François GERVAIS, France]	Such a discussion is more relevant for chapters 3 and 6.
2-1686	2	44	34	44	36	Continued - The difference of temperature between the cold year of 1992 and the El Niño peak of 1998 is around ~ 1°C. The temperature dependence of CO2 solubility in water and specifically the difference of 1.4 % between 1992 and 1998 appear consistent, therefore, with the different growth rates observed in 1992 and 1998 since, as recalled in the AR5 draft, there is 60 times more CO2 in the oceans than in air. This observation can be generalized to all years as is suggested below. [François GERVAIS, France]	Chapters 3 and 6.
2-1687	2	44	34	44	36	Cont. - To this end, it is useful to compare UAH satellite temperatures or SST temperatures of Fig. 2.19 Top as well, with yearly CO2 growth rates in Fig. 2.20b. There is a close resemblance of both data sets. I suggest superposing them in a same figure for the sake of clarity and further comparison. If one considers the peak of 1998, there is even a ~ 6 months lag between both curves : instantaneous growth rates of CO2, viz. d(CO2)/dt, are found to FOLLOW temperature, like in ice core data but not at the same time scale. What emerges is the following picture : more CO2 coming from the oceans after a warmer year, and less (up to 4.3 times less, even 6.1 less if one considers the CO2 Mauna Loa data) after a cold year, in recent records. This observation suggests minimizing the anthropogenic CO2 fraction remaining in air after natural exchanges to about 1/4 of the increase observed at Mauna Loa. [François GERVAIS, France]	Chapters 3 and 6.
2-1688	2	44	34	44	36	Cont. - Indeed, if one plots the yearly CO2 growth rate measured at Mauna Loa versus UAH temperature anomaly and if one fits the data points with a simple linear regression, one finds a relationship of the form : CO2 yearly growth rate (ppm/year) = 0.95 + 3.5 x DT UAH (°C). This means that anthropogenic CO2 increase could be exactly balanced by natural dissolution into the oceans if the temperature is reduced by 0.95/3.5 = 0.27°C with respect to the average UAH temperature anomaly. The numbers are not very different by using HADCRUT data. This model is probably much too simple but it captures the observed tendency. [François GERVAIS, France]	Chapters 3 and 6.
2-1689	2	44	34	44	36	Cont. - There is another way to confirm the anthropogenic fraction of CO2 in the atmosphere. The report considers the C13/C12 ratio (d13C in Fig. 6.3) and points to the increase of anthropogenic CO2 in the atmosphere. This is correct. But why not exploit this ratio and use it to deduce the anthropogenic CO2 fraction nowadays ? It is done neither in AR4 nor in AR5. Please do it. Check it gives ~ 5-6 %, viz. ~ 20 ppm. In other words, it gives ~ 0.5 ppm of residue of additional anthropogenic CO2 remaining in the atmosphere each year, a value which is consistent with the increase found after a cold year like 1992. [François GERVAIS, France]	Chapters 3 and 6.
2-1690	2	44	34	44	36	Cont. - All this means that the ~ 3.5 ppm emitted by burning fossil fuel each year roughly disappears in 20/3.5 ~ 6 years. This is not that long. These measurements strongly minimize the lifetime of CO2 retained in the	Chapters 3 and 6.

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						models. 6 years is the correct order of magnitude compared to data of Revelle R., H.E. Suess, Tellus, 9 (1957) 18, Javorowski Z., T.V. Segalstad, N. Ono, Science Total Environment 114 (1992) 227, Segalstad, T., In Bate, R. (Ed.): Global warming: the continuing debate. ESEF, Cambridge, U.K. (ISBN 0952773422), pp. 184 (1998), Essenhigh, R.H., Energy & Fuels 23 (2009) 2773. [François GERVAIS, France]	
2-1691	2	44	35	44	35	There can't be "balance" between sources and sinks if there is growth. Use "difference" instead. [Dale Hurst, United States of America]	The point is that over the long term photosynthesis and respiration are nearly balanced.
2-1692	2	44	38	44	40	In addition, I believe the Tans (2009) paper actually states that the increase of CO2 in the atmosphere during the 20th century can be attributed almost entirely to fossil fuel emissions, if one integrates all sources and sinks over the course of the century. That would be because the terrestrial biosphere appears to have been a net source during the early part of the 20th century, but a sink during the latter half, while fossil fuels emissions have risen exponentially for the past couple of centuries. [James Butler, United States of America]	Details are for Chapter 6.
2-1693	2	44	39	44	39	"Multiple lines of observational evidence" is a vague statement that should be replaced with an example of the evidence, such as an observed change in the stable isotope or 14C composition of atmospheric CO2. [Dale Hurst, United States of America]	We want it to be clear that multiple lines of evidence exist. Further details are in Chapter 6.
2-1694	2	44	42	44	52	The discussion on AF is very interesting and also important for the ambient CO2 trend. However, this paragraph seems to hint that the AF did not really show a changing in AF. I would like to suggest to change the wording a llittle bit, to indicate that the AF might change, and if it changes, it will be essential for CO2 trends. [Xuemei Wang, China]	AF discussion deleted.
2-1695	2	44	45	44	52	Personal pet peeve: I've never really liked the "Airborne Fraction" as a metric. In my opinion, the historical mostly constant Airborne Fraction is a fluke resulting from a rate of emissions growth that coincidentally is the right amount to maintain the fraction constant: if emissions growth were to increase substantially, the airborne fraction would likely increase, and if emissions were to drop drastically, the airborne fraction would also drop. This is a result of the fact that sinks are more a function of the concentration in the atmosphere than of the change in concentration in the atmosphere, and would therefore likely not vary too much with changes in emissions... if we were to drop emissions in half suddenly, my guess would be the airborne fraction would drop to nearly zero for a few years. It would make me happy if this discussion were updated to reflect a more nuanced view of the AF than "an increasing AF means less effective sinks". [Marcus Sarofim, USA]	AF discussion deleted.
2-1696	2	44	46	44	46	delete comma after prodcution [Peter Burt, UK]	Paragraph deleted.
2-1697	2	44	46	44	47	"that remains in the atmosphere" is technically incorrect since some of the CO2 in the atmosphere has been cycled through the terrestrial biosphere. "that is present in the atmosphere" is more correct. [Dale Hurst, United States of America]	Paragraph deleted.
2-1698	2	44	48	44	49	why only for "anthropogenic CO2" ? [Dale Hurst, United States of America]	Paragraph deleted.
2-1699	2	44	51	44	52	The Knorr (2009) finding is consistent with the Tans (2009) statement that there is has been no significant change in the AF [James Butler, United States of America]	Text deleted.
2-1700	2	44	51	44	52	If you want to report change in airborne fraction, you need to include a mean value too, for context. [Dian Seidel, USA]	Deleted.
2-1701	2	44	51			it would seem that a mean trend in the Airborne Fraction since 1850 does not appropriately comment on any recent change in it. Perhaps the Knorr et al result should only be used to comment on the fact that any recent change hasn't been large enough to affect a trend in this fraction averaged over 150 years. [Stephen Montzka, USA]	Deleted.
2-1702	2	44	54	44	54	A short discussion about uncertainties in CH4 emissions is missing, what about changes in rice paddy emissions? [Katharine Law, France]	For Chapter 6.
2-1703	2	44	54	45	37	Lifetime for CH4 could be given in section 2.4.1.1.2., in coordination with other chapters (6 and 8). Adjustment time may also be given (with explanation of difference between these concepts). [Jan Fuglestvedt, NORWAY]	CH4 lifetime, consistent with Chapters 6 and 8, added.

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2-1704	2	44	54	45	38	Comments similar to 6 and 7 for CO2 are should also be considered for CH4. [David Parrish, USA]	Changed to "globally annually averaged.."
2-1705	2	44	55	44	58	AGAGE data on rates of change of CH4 do not agree well with NOAA data in the earlier years of this time series. AGAGE data should be added to Fig 2.21 with some brief text in this section on this issue. [Ronald Prinn, USA]	This was discussed in AR4 and need not be repeated here. Recent (since 2005) measurements agree well.
2-1706	2	44	57	44	57	Do we know CH4 concentrations to such accuracy? I don't think so. [Katharine Law, France]	These are the uncertainties data contributors submited.
2-1707	2	44	58	44	58	programs → programmes [Peter Burt, UK]	OK
2-1708	2	44	58	44	58	"other programs" means what? [Xuemei Wang, China]	The other programs contributing global means to table 2.12.
2-1709	2	44	58	44	58	Replace "programs" with "measurements". [Robert Waterland, United States of America]	Reworded.
2-1710	2	44	58			Programs' is kind of vague... [Larry Thomason, United States of America]	Changed.
2-1711	2	45	1	45	1	remove "it is" to read "and the second largest …" [Dale Hurst, United States of America]	Deleted.
2-1712	2	45	1	45	2	It would be good to give the end year here for this increase. [Drew Shindell, USA]	Reworded.
2-1713	2	45	1	45	7	There is no discussion of GOSAT which arguably provides superior column-averaged CH4, see Parker et al, GRL, 2011. http://www.agu.org/pubs/crossref/2011/2011GL047871.shtml. I would suggest adding a line that the situation in the near future may change with the advent of GOSAT data". My understanding is that many people are now working on GOSAT inverse modelling. The statement with regard to SCIMACHY regarding inverse modelling appears inconsistent with the the extensive quotation in Chapter 8 of Bergamaschi, P., et al., 2009: Inverse modeling of global and regional CH(4) emissions using SCIAMACHY satellite retrievals. Journal of Geophysical Research-Atmospheres, 114. !! It seems good enoiugh for use in models but not for causal analysis. This needs to be made clear. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	There are no published retrievals of global annual mean CH4 values from GOSAT to directly compare with in situ measurements.
2-1714	2	45	1	45	37	There are some interesting papers by Bloom et al which merit consideration here in terms of tropical methane wetland emissions: Bloom, A. A., P. I. Palmer, A. Fraser, D. S. Reay, and C. Frankenberg, "Large-scale Controls of Methanogenesis Inferred From Methane and Gravity Spaceborne Data," Science, 327, 322-325, 2010; 1.Bloom, A. A., P. I. Palmer, A. Fraser, and D. Reay, "Seasonal variability of tropical wetland CH4 emissions: the role of the methanogen-available carbon pool," Biogeosciences Discuss., 9, 387-409, 2012. (URL) [John Remedios, United Kingdom of Great Britain & Northern Ireland]	This study is appropriate for Chapter 6.
2-1715	2	45	2	45	2	insert 'data' after 'reported' [Peter Burt, UK]	Rejected.
2-1716	2	45	2	45	2	Should explicitly state what IPCC assesses as the best estimate of the trend. Is it NOAA or an average of NOAA and AGAGE. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Each increase listed in Table 2.12 is equally valid.
2-1717	2	45	2	45	3	Why report an increase "since 2005" when "The increase began in 2007" ? [Dale Hurst, United States of America]	We are covering the period from AR4 to present.
2-1718	2	45	3	45	3	"near" should be "nearly" [James Butler, United States of America]	Changed.
2-1719	2	45	4	45	7	Do Frankenberg et al offer any uncertainties on their trend numbers? Given that the satellite uncertainties are likely much larger than the surface-based values, it might be better to simply state that SCIAMACHY results are "consistent" with the surface-based trends (if, and only if, however, they are independent). [James Butler, United States of America]	SCIAMACHY trends deleted.
2-1720	2	45	5	45	5	I am not sure irradiances is the correct term here as it refers to the total amount of radiation present, at all frequencies whereas I am sure Schiamachy does not measure this. I suggest replacing “irradiances” with “radiances”. Also note incorrect spelling of “precise” on line below. [Roger Saunders, United Kingdom]	Corrected. Irradiances should have been radiances.

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2-1721	2	45	5	45	5	"precise" misspelled [Dian Seidel, USA]	Corrected.
2-1722	2	45	6	45	6	"precise" instead of "presice" [Florinda Artuso, Italy]	Corrected.
2-1723	2	45	6	45	6	Replace "presice" with "precise" [Mihai Dima, Romania]	Corrected.
2-1724	2	45	6	45	6	"presice" is misspelled [Dale Hurst, United States of America]	Corrected.
2-1725	2	45	6	45	6	Replace "presice" with "precise". [Robert Waterland, United States of America]	Corrected.
2-1726	2	45	6			presice = precise [Philip Lloyd, South Africa]	Corrected.
2-1727	2	45	7	45	7	Technically, the causes for increasing CH4 are not that unclear, as the most probable mechanisms are discussed just below. So I [Klaas Folkert Boersma, Netherlands]	Sentence deleted.
2-1728	2	45	7	45	7	Remove sentence: "Causes for the increase in atmospheric CH4 since 2007 are unclear." In view of the historic increase in CH4 since 18th century it would be more appropriate to state than the pause in the CH4 growth between 1999 and 2006 remains unclear. Emission inventories show uninterrupted growing anthropognic emissions although with variable growth rates. [Michiel van Weele, The Netherlands]	Reworded.
2-1729	2	45	7	45	7	The last sentence says "Causes for the increase in atmospheric CH4 since 2007 are unclear.". The following three paragraphs seems to give very clear causes! [Robert Waterland, United States of America]	Reworded.
2-1730	2	45	9	45	9	"to explain the CH4 observations" - which observations? [Dale Hurst, United States of America]	The observations that are discussed throughout this section.
2-1731	2	45	9	45	15	Rigby et al (2008) used the 4% number in their CH4 inversion which was consistent with their CH3CCl3 analysis. The 14% error estimate refers to their OH estimate not to their CH4 inversion. The 4% number is also not statistically inconsistent with the Montzka et al CH3CCl3-based OH analysis particularly taking into account the incomplete error analysis in the latter paper. Please correct text. [Ronald Prinn, USA]	Text deleted.
2-1732	2	45	9	45	15	Why did the methane increase stall from 1999-2006 as seen in figure 2.21? [Karen Rosenlof, United States of America]	The decline in CH4 GR to near zero during this period was discussed in AR4.
2-1733	2	45	9	45	37	These three paragraphs could be shortened a lot. What is gained by this recitation of results? [Dian Seidel, USA]	Reworded.
2-1734	2	45	10	45	10	add explanation that OH is the predominant sink for CH4 [Dale Hurst, United States of America]	Reworded.
2-1735	2	45	10	45	10	The emissions themselves don't depend on assumptions in [OH], rather it is the derived emissions change that depended on [OH]. [JOHN OGREN, USA]	Deleted.
2-1736	2	45	10	45	10	Consider adding "estimated" before "emissions change" [Dian Seidel, USA]	Deleted.
2-1737	2	45	10	45	14	These considerations are all based on the paper of Rigby et al. (2008)? [Claudio Cassardo, Italy]	Deleted.
2-1738	2	45	11	45	15	But there will be interannual variability in OH- as stated on lines 14/15. I find the sentence "If an OH decrease ..." a little confusing. Does the sentence below contradict that? [Ruth Doherty, UK]	Deleted.
2-1739	2	45	12	45	15	The sentences are difficult to understand. The first sentence tells that 4% decrease of [OH] had been determined from the CH3CCl3 measurements, while the second sentence tells that more than a few % decrease of [OH] is unlikely based on the CH3CCl3 measurements. [Yoko Yokouchi, Japan]	The 4% decrease in [OH] used in one of two scenarios in Rigby et al. is not consistent with the analysis of Montzka et al. Mention of the Rigby OH result was deleted.
2-1740	2	45	13	45	14	northern hemisphere → Northern Hemisphere [Peter Burt, UK]	OK
2-1741	2	45	14		15	Montzka et al., paper focused on interannual variability in OH, this qualifier needs adding: "Changes in OH of more than a few percent interannually are unlikely…" [Stephen Montzka, USA]	Reworded.

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2-1742	2	45	17	45	21	It would be good to note that this is inferred from global measurements of atmospheric methane, e.g., "Dlugokencky . . . Inferred from observations of atmospheric trends and distributions that the most likely drivers . . . ." [James Butler, United States of America]	Re-written.
2-1743	2	45	17	45	29	These two paragraphs discuss the same topics and can be combined [Dale Hurst, United States of America]	Re-written.
2-1744	2	45	19	45	19	Replace "where" with "since". [Robert Waterland, United States of America]	Text deleted.
2-1745	2	45	27	45	27	" . .. not consistent . . ." between each other or with atmospheric observations? [James Butler, United States of America]	Text deleted.
2-1746	2	45	29			qualifiers needed to retain accuracy in this sentence: "They found that changes in global annual mean [OH] during these 2 years were less than…" [Stephen Montzka, USA]	Reworded.
2-1747	2	45	32	45	32	insert "much larger" before "emissions" [James Butler, United States of America]	Text deleted.
2-1748	2	45	33	45	33	..changes to methane's rate of... → '...changes in the rate of increase of CH4 [Peter Burt, UK]	Text deleted.
2-1749	2	45	34	45	35	"trends in global emissions from specific sources cannot be quantified" - I think some regional emissions trends can be quantified with existing observation networks [Dale Hurst, United States of America]	Text deleted, but that was not the point.
2-1750	2	45	34	45	37	"…. trends in emissions …. cannot be quantified from existing observation networks." Text inconsistent with what's written in the introduction to section 2.4 (see comment 3). Also, are the long-term observations in the Arctic really good enough to draw such general conclusions? Which climate feedbacks? (papers on CH4 from permafrost?). [Katharine Law, France]	Current observations are sufficient to show that, so far, there are not significant changes in CH4 emissions in the Arctic.
2-1751	2	45	35	45	37	The discussion here of methane feedbacks seems beyond the remit of this chapter. Any such discussion should be reserved for chapter 6 (Carbon and Biogeochemical Cycles) [Michael Mann, USA]	Text deleted.
2-1752	2	45	35	45	37	I suggest to add more explanation about the feedback of climte feedback on Arctic CH4 emission, it was supposed to be an issue. [Xuemei Wang, China]	Detailed discussion for Chapter 6.
2-1753	2	45	35		37	Observations of atmospheric CH4 allow one to comment primarily on total CH4 sources (human and natural), perhaps add "Observations of atmospheric CH4 and an isotopologue suggest (Dlugokencky et al)…" [Stephen Montzka, USA]	Text deleted when sectioned shortened.
2-1754	2	45	36	45	37	This sentence assumes that "anomalously high temperatures in the Arctic in 2007" (Line 18) were not a climate feedback [Dale Hurst, United States of America]	The reviwer is confusing the anomalous emissions in 2007 with persistent long-term changes in Arctic CH4 emissions.
2-1755	2	45	39	34	48	As with climate models, model intercomparison studies have shown that different models (land surface and hydrological models) show considerable variability in the magnitude and timing of the hydrological variables simulated, and some studies suggest that the ensemble mean (of all models) provides a more reliable estimate than any single model (e.g. Guo et al., 2007; Gudmundsson et al., 2011). Thus, care should be taken interpreting the outcome from only one large-scale model. [Lena M. Tallaksen, Norway]	I agree. Unfortunately, there are not ensembles of inverse models optimizing LLGHG emissions, and, even if there were, the problem is still poorly constrained by observations.
2-1756	2	45	40	45	48	Add AGAGE results for N2O here and in Fig 2.22 and briefly discuss agreements or disagreements. [Ronald Prinn, USA]	Figures updated to include all known global annual means.
2-1757	2	45	42	45	43	This is an incomplete statement. It should be noted here that the soil emissions of N2O result ultimately from fertilizer applications, not natural sources. [James Butler, United States of America]	Reworded.
2-1758	2	45	43	45	43	Explain why soils contribute to N2O changes. Is this the fertilizer source? [Dian Seidel, USA]	It is described below.
2-1759	2	45	43	45	46	This statement on N2O is not entirely correct and not consistent with Ch8 [Gunnar Myhre, Norway]	Using the RF equations in Ch. 8, I get N2O third and CFC-12 4th largest contributers to RF.
2-1760	2	45	45	45	46	"the third most important LLGHG contributing to radiative forcing." - What LLGHG does not contribute to RF ? [Dale Hurst, United States of America]	What is your comment?

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2-1761	2	45	47	45	47	The final sentence suddently mentions ozone depleting sustances, not having explained that some of the other species already discussed are involved in ozone chemistry too. A better segue is needed. [Dian Seidel, USA]	Sentence removed.
2-1762	2	45	50	45	50	insert comma after 'sources' [Peter Burt, UK]	OK
2-1763	2	45	50	45	50	delete comma after 'remainder' [Peter Burt, UK]	OK
2-1764	2	45	50	45	53	The given uncertainty range (55-70%) for the relative contribution of natural N2O emissions relative to total net flux to the atmosphere is a bit larger than estimated by Crutzen et al., 2008 (5.6–6.5 Tg N2O-N anthropogenic vs 9.3-10.2 Tg N2O-N natural) based on a strict budget closure for N2O at both present-day and pre-industrial time. This paper is referred to in the same sentence but in another context and should be included for the given range estimate, maybe sligthly reducing the uncertainty. Consistency needed with Chapter 6, section 6.3.4.2 [Michiel van Weele, The Netherlands]	Discussion left for Chapter 6.
2-1765	2	45	50	45	54	Adjustment time for N2O may also be given here, in addition to lifetime (coordinated with chapters 6 and 8), with explanation of difference between these concepts. [Jan Fuglestvedt, NORWAY]	Prefer to leave discussion of perturbation lifetime to Chapter 8 and only discuss budget liftime here, since it is most closely related to current observations.
2-1766	2	45	52	45	53	Does this remark about potential future trends belong in this chapter? [Dian Seidel, USA]	Deleted.
2-1767	2	45	56	45	57	If you want to give values for the polar regions, you should provide the northern subtropical value too, for context. [Dian Seidel, USA]	Values for each region change annually; those given are relative to the maximum to give the reader a feel for the observed zonal distribution.
2-1768	2	45				think 'unclear' is a bit too strongly put. I suggest to rephrase it such 'Causes for the increase in atmospheric CH4 since 2007 are [Klaas Folkert Boersma, Netherlands]	Reworded.
2-1769	2	45				subject to scientific debate'. [Klaas Folkert Boersma, Netherlands]	Reworded.
2-1770	2	46	5	46	6	Consider moving ", not seasonality in emissions, " to precede "is the dominant contributor", for clarity [Dian Seidel, USA]	CLA prefers as is.
2-1771	2	46	12	46	14	This sentence fails to identify what these numbers represent - presumably the annual contributions of STE to global tropospheric N2O mixing ratios ? [Dale Hurst, United States of America]	Discussion of emissions deleted.
2-1772	2	46	12	46	14	These trends and uncertainties do not make sense, and the confidence intervals do not encompass the best estimates. [Dian Seidel, USA]	Text deleted.
2-1773	2	46	14	46	14	Do you mean 67% instead of 68% ? [Ronald Prinn, USA]	No.
2-1774	2	46	15	46	15	If you want to use the acronym STE, then also use the phrase "stratosphere/troposphere exchange" [Dian Seidel, USA]	STE already defined.
2-1775	2	46	15	46	16	What source of uncertainty other than "uncertainties in air mass exchange between the stratosphere and troposphere" could possibly contribute to the STE fluxes of N2O ? [Dale Hurst, United States of America]	Irrelevant comment.
2-1776	2	46	18	46	19	Which are the conclusion of this analysis? [Claudio Cassardo, Italy]	Will make more clear.
2-1777	2	46	18	46	19	No result is reported from the Ishijima et al. study, so this sentence need not be included. [Dian Seidel, USA]	Deleted.
2-1778	2	46	21	46	21	delete comma after PFCs [Peter Burt, UK]	Rejected.
2-1779	2	46	21	47	32	section 2.4.1.1.4: Coordination with chapter 8 is needed regarding lifetimes. [Jan Fuglestvedt, NORWAY]	Request for information from Chapter 8 not met.
2-1780	2	46	21			Section 2.4.1.1.4 should explicitly state which sources are used for the halogenated species measurements and trends [William Collins, United Kingdom of Great Britain & Northern Ireland]	Should now be clear.
2-1781	2	46	21			Section 2.4.1.1.4: Numbers are quoted to too many decimal places give the uncertainties in the numbers	For the FOD, we used the number of significant

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[William Collins, United Kingdom of Great Britain & Northern Ireland]	figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1782	2	46	22	46	22	"and their contributions to radiative forcing are …" [Dale Hurst, United States of America]	…in Chapter 8.
2-1783	2	46	22	46	22	Consider adding reference to Sec. 2.4.1.2. [Dian Seidel, USA]	I am not sure what sense the reviewer thinks they should be referenced to the section on Montreal Protocol gases.
2-1784	2	46	25		26	agreement among measurement programs can be assessed only for 1 chemical, HFC-134a (see WMO 2010, Table 1-15), so this general point doesn't seem valid but could be made for that compound. [Stephen Montzka, USA]	Deleted, but also true for SF6.
2-1785	2	46	25			This is my most important comment. Our work with HFC-245fa and HFC-365mfc (Vollmer et al., 2011, JGR) has shown, that the projections by Velders for these two compounds for 2010 is reasonable, however, the work by Vollmer et al., 2011 also clearly shows that the RCP projections (used by the modelers for predictions) are wrong for HFC-245fa and HFC-365mfc for some parts of the predicitions, and grossly overestimated for other parts. Even if the contribution to the radiative forcing from these 2 compounds is minor, this requires correction. For example, the RCPs for HFC-245fa have emissions for the year 2000, that is before this compound was even released to the atmosphere. Could you forward this comment to those working with the RCPs? In needs to be fixed somehow, not at last because it is a potential platform for sceptisism of the whole issue. Thank You. [Martin Vollmer, Switzerland]	Forward comment to ....
2-1786	2	46	28	46	45	Add the relative rate of increase in %/year for HFC-134a and HFC-23. [Robert Waterland, United States of America]	This would make the discussion too cumbersome.
2-1787	2	46	29	46	29	"In 2010, it reached 57.6 ± 0.6 ppt" - What is it? The global mean surface mixing ratio? This problems exists throughout 2.4.1.1.4 and 2.4.1.2 (e.g., "HFC-125 increased by 4.479 ± 0.372 ppt ") [Dale Hurst, United States of America]	Is it unfair for the LA to expect the reader to think about what they are reading. Everything discussed in this section is a globally averaged, annual mean value. Does it have to say that every single time? No.
2-1788	2	46	30	46	30	delete comma after 'Europe' [Peter Burt, UK]	No.
2-1789	2	46	33	46	34	Too many significant figures. "4.413 +/- 0.475 ppt" could be "4.4 +/- 0.48 ppt". [Yoko Yokouchi, Japan]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1790	2	46	33	46	45	A recent study by Keller et al (GRL, 2011) estimated HFC-23 emissions from Europe in more detail. Some of their results should be added here as they complete the picture a little better [Martin Vollmer, Switzerland]	Added.
2-1791	2	46	33	46	45	Observations have clearly shown that majority of HFC-23 has been emitted from China (Yokouchi et al., GRL, 2006; Stohl et al., ACP, 2010). This should be stated in this paragraph. [Yoko Yokouchi, Japan]	OK
2-1792	2	46	33	48	24	Here and throughout 2.4.1.1.4 and 2.4.1.2 - many numbers are reported with way too many figures that are not significant - e.g., "HFC-125 increased by 4.479 ± 0.372 ppt" [Dale Hurst, United States of America]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1793	2	46	33			increased FROM [Martin Vollmer, Switzerland]	It is OK with "since".
2-1794	2	46	34	46	34	quantify the uncertainties [Peter Burt, UK]	Uncertainties (90% c.l.) are in table 2.1.
2-1795	2	46	35	46	35	Here the term "inverse model" is used. Elsewhere, "top down" and "bottom up" models are mentioned. I	Terms were defined P42, L16-20, but deleted to save

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						assume an inverse model is a bottom up model. If no, best just to use a single term consistently. If not, then clarify the difference. [Dian Seidel, USA]	space. Bottom-up inventory remains, but its meaning is clear from the context.
2-1796	2	46	35	48	40	How can "decreased emissions from developed countries" … (since the late 1990s)... "followed by increased emissions from developing countries" explain the observations that "emissions increased …, peaked in 2006 then … decreased" (2006-2009) ? [Dale Hurst, United States of America]	Read Miller et al. (2010).
2-1797	2	46	39	48	41	"voluntary destruction" belongs in the next sentence with the Clean Development Mechanism [Dale Hurst, United States of America]	CDMs are for developing countries.
2-1798	2	46	47	47	13	In this paragraph, mixing ratios for HFCs and PFCs are given with too many significant figures. [Yoko Yokouchi, Japan]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1799	2	46	47	48	20	Significant figures are a little out of control here. Cut them back by one or two digits. [James Butler, United States of America]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1800	2	46	47	48	20	I strongly disagree with the reporting of 4 (sometimes 5 or 6) significant figures for data that stem from observations, e.g. p. 47, line 46 (CFC-11 was 239.822 +- 4.152). For compounds where measurements are <0.1%, a fourth figure may be debatable, but in general I think anything beyond 3 significant figures is not useful and statistically probably incorrect and somewhat missleading. [Martin Vollmer, Switzerland]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1801	2	46	47			Can the data in Fig. 2.23 be extended to at least 2010 so that the 2010 concentrations discussed in the text are actually shown on the graph? [David Parrish, USA]	Figures for SOD will include data that are as recent as labs can provide.
2-1802	2	46	49	48	51	The sentence starting with "These estimated emissions" is very poorly written. Instead: "These emission estimates are within about 20% of those reported to the UNFCCC plus estimates of the non-reported emissions from East Asia." [Dale Hurst, United States of America]	Slightly reworded.
2-1803	2	46	52	46	55	The discussion of "new HFCs" measurements could be made more complete, perhaps along the lines of: "New measurements of several HFCs have been reported since AR4. These top-down emission estimates show good agreement amongst themselves, but importantly they show a mix of poor to good agreement with various bottom-up estimates. Earlier emission estimates for HFC-365mfc for 2003-2004 (Stemmler et al., 2007) were later shown to be reasonably consistent with a 1985-2010 emission estimate (Vollmer et al., 2010), both of which are significantly higher than the previously published bottom-up estimates of Ashford et al. (2004). After an initial sharp increase, emissions during 2006-2010 appear to be slowing or even decreasing. Similarly, for HFC-245fa, previously increasing emissions appear to be leveling or decreasing in 2006-2010, and again, this top-down approach has yielded emissions that are about a factor of two above the bottom-up (Ashford et al., 2004). In contrast, top-down emission estimates (Vollmer et al., 2010) for HFC-227ea show continuing increase, show good agreement with those of Laube et al. (2010), and are several times higher than that reported to the UNFCCC, but also many times lower than the bottom-up of Ashford et al. (2004). Top-down emissions of Vollmer et al. (2010) for HFC-236fa were in good agreement with inventories reported to the UNFCCC. Ashford, P., D. Clodic, A. McCulloch, and L. Kuijpers (2004), Emission profiles from the foam and refrigeration sectors comparison with atmospheric concentrations. Part 2: Results and discussion, Int. J. Refrig., 27 (7), 701-716, doi:10.1016/j.ijrefrig.2004.08.003. Vollmer, M. K., Miller, B. R., Rigby, M., Reimann, S., Mühle, J., Krummel, P. B., O'Doherty, S., Kim, J., Rhee, T. S., Weiss, R. F., Fraser, P. J., Simmonds, P. G., Salameh, P. K., Harth, C. M., Wang, R. H. J., Steele, L. P., Young, D., Lunder, C. R., Hermansen, O., Ivy, D., Arnold, T., Schmidbauer, N., Kim, K.-R., Greally, B. R., Hill, M., Leist, M., Wenger, A. and Prinn, R. G., (2011), Atmospheric histories and global emissions of the anthropogenic hydrofluorocarbons	These minor contributors to RF will be covered in an appendix to section 2.2.1.

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						HFC-365mfc, HFC-245fa, HFC-227ea, and HFC-236fa, J. Geophys. Res., 116, D08304, doi:10.1029/2010JD015309." [Benjamin R. Miller, United States of America]	
2-1804	2	46	52	46	55	Need to mention the 4 new HFCs published in Vollmer et al, JGR, v116, 2011 [Ronald Prinn, USA]	OK
2-1805	2	46	53	46	56	A new study within the AGAGE network (Vollmer et al., 2011, JGR doi 10.1029/2010JD015309) has now included the first measurements of HFC-236fa, which is the second long-lived HFCs. This study also includes HFC-365mfc, HFC-245fa, and HFC-227ea, with longer and more comprehensive trends of e.g HFC-227ea compared to that of Laube [Martin Vollmer, Switzerland]	OK
2-1806	2	47	1	47	1	Why mention that these gases absorb IR? All GHGs do, by definition. [Dian Seidel, USA]	The point is, they are **strong** absorbers.
2-1807	2	47	1	47	13	The recent works of Oram et al. (ACP, 12, 261-269, 2012) and Ivy et al. (ACPD 12, 4165-4184, 2012) should now be included here also. [Martin Vollmer, Switzerland]	Added to appendix.
2-1808	2	47	1	47	13	Only PFCs are shown in molecular formulas (e.g. CF4, C2F6) in place of PFC-xx (e.g. PFC-14, PFC-116). [Yoko Yokouchi, Japan]	Yes, that is correct.
2-1809	2	47	1	47	13	The information about PFC-218 and PFC-318 is missing. Saito et al.(ES&T, 2010) reported hige frequency measurements of PFC-218 and PFC-318 as well as PFC-116, and their emission estimates from East Asia. [Yoko Yokouchi, Japan]	Added to appendix.
2-1810	2	47	2	47	2	"are emitted as byproducts" [Dale Hurst, United States of America]	OK
2-1811	2	47	3	47	5	This is far too much detail. In fact, sections 2.4.1.1.4 and 2.4.1.2 contain far too much detail compared to the sections for CO2, CH4 and N2O that include nothing about measurement techniques and calibration scales. [Dale Hurst, United States of America]	CO2, CH4, and N2O budgets are discussed in Chapter 6; this is the only place in AR5 where budgets of other LLGHGs are discussed.
2-1812	2	47	3		5	the "new instrumental developments" are much more than "that can pre-concentrate trace species from ambient air at -165C", recommend removing this last phrase. [Stephen Montzka, USA]	Deleted.
2-1813	2	47	6	47	6	preindustrial → pre-industrial (consistency with rest of document) [Peter Burt, UK]	OK
2-1814	2	47	8			Many of the quantities and their confidence limits given in this chapter are given to many more significant figures than justified by the accuracy and precision; some reasonable rounding should be performed (i.e. 78.263 ± 1.922 would be better given as 78.3 ± 1.9) [David Parrish, USA]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1815	2	47	11	47	12	Emissions of extremely long-lived gases can be "inferred" from their measured atmospheric burdens, so no need to "estimate" them [Dale Hurst, United States of America]	OK
2-1816	2	47	15	47	15	Insert "The" before "global". [James Butler, United States of America]	Rejected.
2-1817	2	47	15	47	16	Consider adding mention of SF6 sources. [Dian Seidel, USA]	OK
2-1818	2	47	15	47	20	Deeds et al. (2008) suggested some natural background of SF6: that would be good to include here. (also, maybe, hexafluoroethane from Worton et al.: I'd suggest a brief paragraph noting that nearly all fluorinated gases are anthropogenic, then listing the few exceptions of CF4, SF6, hexafluoroethane, and any others that I don't know about) [Marcus Sarofim, USA]	This issue has been discovered in previous reports and there is no space to cover it again here.
2-1819	2	47	15	47	20	Like CF4, SF6 has also a natural source. This has sometimes led to some confusion and may be worth mentioning here. Natural SF6 has been discovered in fluorites (Harnisch et al., GRL, 1998, 25, No13, p.2401--2404) and confirmed by Deeds et al (2008, see ref in CF4). The pre-anthropogenic SF6 in the atmosphere has been estimated at <0.0063 ppt from analysis of Antarctic firn air (Vollmer and Weiss, Marine Chemistry, 2002, 78, 137--148, see p. 144). Hence the pre-anthropogenic SF6 is just about 1 thousands of the current SF6 in the atmosphere, so the natural contribution is negligible, unlike CF4, where about half of the atmospheric abundance is of natural origin. [Martin Vollmer, Switzerland]	This issue has been discovered in previous reports and there is no space to cover it again here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1820	2	47	16	47	17	The second half of this sentence should be moved to the previous paragraph, just before the discussion of inferred emissions of CF4 and C2F6. [Dale Hurst, United States of America]	This text was for the references that followed.
2-1821	2	47	19	47	19	they' = ? Give auhtor details [Peter Burt, UK]	Reworded.
2-1822	2	47	22	47	32	I'm not sure if GWP values should be given here. The updating of GWPs are based on several factors which are dealt with in Chapter 8. A reference to chapter 8 could be given instead. If it is decided to keep GWP values in table 2.12, more coordination with chapter 8 is needed. (In addition, it is not obvious that GWP-100 should be used as indicator of climate impact of these gases. In chapter 8 we try to show the impacts by alternative indicators in addition to the traditionally used GWP). [Jan Fuglestvedt, NORWAY]	Text moved to appendix w/o GWPs.
2-1823	2	47	23	47	23	In what sense is NF3 "missing"? Missing from the scientific literature? Missing in the atmosphere? Missing in some budget calculations? Would "previously not considered" be more accurate? [Dian Seidel, USA]	Reworded.
2-1824	2	47	23	47	24	"with a potentially large impact" [Dale Hurst, United States of America]	OK
2-1825	2	47	26	47	27	"growing from almost zero in 1978." [Dale Hurst, United States of America]	OK
2-1826	2	47	27			IN 1978 [Martin Vollmer, Switzerland]	OK
2-1827	2	47	28	47	28	There should probably be a reference for the "bottoms up inventories". [Dian Seidel, USA]	The appropriate references are in Weiss et al.
2-1828	2	47	34	47	34	delete comma after HCFCs [Peter Burt, UK]	No.
2-1829	2	47	34	48	24	Section 2.4.1.2.Montreal Protocol Gases.Global annual mean values of CCl4 (p. 47-line 55), HCFC-22 (p. 48, line 14), and HCFC-141b (p. 48 line 15) are reported with a number of significant figures different than the corresponding uncertainties. In my opinion values and uncertainties should have the same number of digits. [Florinda Artuso, Italy]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1830	2	47	34	48	24	section 2.4.1.2.: Coordination with chapter 8 is needed regarding lifetimes. [Jan Fuglestvedt, NORWAY]	Will coordinate with Chapter 8.
2-1831	2	47	34			Section 2.4.1.2: Numbers are quoted to too many decimal places give the uncertainties in the numbers [William Collins, United Kingdom of Great Britain & Northern Ireland]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1832	2	47	36	47	53	This paragraph doesn't seem to have a main point. Each sentence seems disconnected from the rest. [Dian Seidel, USA]	Disagree.
2-1833	2	47	38	47	38	"emissions of ODSs by about 11 Pg yr–1 CO2 equivalent" [Dale Hurst, United States of America]	OK
2-1834	2	47	38			"including offsets"... I think it important to indicate which ones are included here. [Stephen Montzka, USA]	Added strat O3 and HFCs.
2-1835	2	47	41			I don't believe the phrase "are predominantly anthropogenic" accurately characterizes our understanding. They are predominantly if not entirely anthropogenic… I would also suggest reference to a broader range of studies, given that this isn't a new conclusion--perhaps back the Martinerie et al citation with one to the recent WMO report [Stephen Montzka, USA]	Reworded and added cittation to WMO O3 assessment.
2-1836	2	47	43	47	44	Too many figures. Same comment than 2-42 line 55: is it useful to put so many irrelevant figures. And it carries on along all this section. [Francois DANIS, France]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1837	2	47	43	47	44	I think that there is some inconsistency in this statement "Since AR4, its global annual mean mole fraction declined 10.388 ± 0.243 to 532.824 ± 44 8.956 ppt in 2010." [Celeste Saulo, Argentina]	Statement accurate as is.

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2-1838	2	47	43	48	20	In these paragraphs, mixing ratios for CFCs and HCFCs are given with too many significant figures. [Yoko Yokouchi, Japan]	For the FOD, we used the number of significant figures provided by the data submitters, which we recognize are not justified. Data submitters will be asked to provide only the appropriate number of significant figures for the SOD.
2-1839	2	47	43			Despite what Prather and Hsu might have called it, describing NF3 as a "missing" greenhouse gas seems odd, inappropriate. Remove? [Stephen Montzka, USA]	Line 23: Reworded to include missing from the Kyoto protocol for clarity.
2-1840	2	47	45	47	47	It is necessary to give peer reviewed references for AGAGE and NOAA results (e.g. Prinn, Weiss et al, JGR v105,2000, updated to 2010 for AGAGE) [Ronald Prinn, USA]	They are included in the appendix.
2-1841	2	47	47		50	This concept was described in the recent WMO report, Chapter 1--support these assertions with citation to it? [Stephen Montzka, USA]	Citation added.
2-1842	2	47	52	47	52	Future emissions of CFCs will largely come from “banks” - isn't this the case already for many CFCs ? [Dale Hurst, United States of America]	Yes.
2-1843	2	47	55	47	55	What is the meaning of "again" here? Like CFC-11? Like AR4? [Dian Seidel, USA]	Reworded.
2-1844	2	47	55	47	56	Is there a reason to prefer the NOAA or AGAGE trend, or can the two sources be used to derive a central value and uncertainty? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Avergaes of NOAA and AGAGE now given.
2-1845	2	47	55	47	56	About the sentence: "AGAGE reported that the CCl4 global annual mean decreased by 5.54 ± 0.209 ppt since 2005, which again is smaller than NOAA reports (–6.36 ± 0.34), to 86.268 ± 3.018 ppt in 2010". Please verify this last scientific result (and similar results in this Chapter) indicated with an uncertainty (or error) having 4 significant digits. If there is an uncertainty of 3 in 86 in 2010 (which is about 3.5%), it has no meaning to indicate the uncertainty as 3.018. I suggest to the Authors that, even if this result was extracted directly from the AGAGE report, a criteria about the way in which the uncertainties will be included in the present AR5 must be defined. [Rubén D Piacentini, Argentina]	Justifiable number of significant figures will be used in SOD.
2-1846	2	47	55	48	2	Far too much detail here [Dale Hurst, United States of America]	Disagree.
2-1847	2	47	55	48	2	For AGAGE CCl4 results please reference latest CCl4 paper (Xiao et al, ACP, v10, 10421-10434, 2010 (updated)). Need similar reference for NOAA results. [Ronald Prinn, USA]	Reference not used.
2-1848	2	48	1		3	This concept was described in the recent WMO report, Chapter 1--support these assertions with citation to it? [Stephen Montzka, USA]	Citation added.
2-1849	2	48	1			about lifetime of CCl4. Somehow, the process to destroy CFC-11 and CCl4 should be comparable. If the lifetime of CFC-11 has gone from 45 to 64years (an increase of 42%) what happens if the lifetime of CCl4 is increased by the same amount (42%)? It seems it would help to sort out the discrepancy you are mentioning. And I believe stratospheric circulation help to determine lifetime of one CFC from another CFC. If one lifetime is wrong (CFC-11), the other one (CCL4) should be wrong by the same %. No time to check for a reference but Bill Sturges (UEA) or Jim Elkins (AGAGE) could help. [Francois DANIS, France]	Thank you.
2-1850	2	48	2	48	2	If you use a technical term like "exponentially", maybe an e-folding rate should be given. Otherwise, maybe a non-technical term like "rapidly" would be best. [Dian Seidel, USA]	Exponentially provides more information than rapidly. E-folding time is given in appropriate references.
2-1851	2	48	2	48	2	declined exponentially for about a decade' appears somewhat miss-leading, hasn't it declined exponentially for much longer. If the earlier decline wasn't exponential, then maybe this should be rephrased to something like: CH3CCl3 has declined since 19XX where as the decline for the last decade was exponential' [Martin Vollmer, Switzerland]	It is the combination of exponential decline and small modern emissions that make it useful for assessing the MC lifetime.
2-1852	2	48	4	48	4	insert 'the' after 'with'' [Peter Burt, UK]	Rejected.
2-1853	2	48	4	48	9	Could this material be deleted? [Dian Seidel, USA]	No. Understanding OH is critical to our understanding of atmospheric composition.

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2-1854	2	48	6	48	6	"and small contemporary emissions" [Dale Hurst, United States of America]	OK
2-1855	2	48	8	48	8	"is consistent with estimates based on other species including CH4 ..." [Dale Hurst, United States of America]	OK
2-1856	2	48	11	46	20	Is the Montzka paper the reference for all the trends noted here? Also, it would be easier to read if they were put in a table rather than embedded in text, [Karen Rosenlof, United States of America]	Trends are in Table 2.1.
2-1857	2	48	12	48	12	..be phased oput. But global..' → '..be phased out, but global..' [Peter Burt, UK]	Reworded.
2-1858	2	48	12		13	This concept was described in the recent WMO report, Chapter 1--support these assertions with citation to it or to the original data published on line and regularly updated by UNEP [Stephen Montzka, USA]	Citation included.
2-1859	2	48	14	48	14	The statement "HCFC-22 increased by 36.945 ± 1.16 ppt since 2005 to 36.945 ± 1.16 ppt in 2010" is wrong because the same abundance is indicated in both years. Indicate the right values. [Florinda Artuso, Italy]	Corrected.
2-1860	2	48	14	48	14	"HCFC-22 increased by 36.945 ± 1.16 ppt since 2005 to 36.945 ± 1.16 ppt in 2010": the numbers are the same, evidently a mistake. [Claudio Cassardo, Italy]	Corrected.
2-1861	2	48	14	48	14	"increased by 36.945 - - " Some increase!! Perhaps meant "was undetectable in 2005 and 36.9 in 2010" 0r some such. [Philip Lloyd, South Africa]	Corrected.
2-1862	2	48	14	48	14	About the sentence: "HCFC-22 increased by 36.945 ± 1.16 ppt since 2005 to 36.945 ± 1.16 ppt in 2010." Please verify, since both quantities and uncertainties are exactly the same. The only possibility is that HCFC-22 was zero, before 2005. In this case it must be explained in a more simple form. [Rubén D Piacentini, Argentina]	Corrected.
2-1863	2	48	14	48	20	Data from AGAGE website and most recent AGAGE paper for HCFC-22, 141b and 142b should also be quoted here: O'Doherty et al, JGR, v109, 2004 updated in AGAGE website. [Ronald Prinn, USA]	Data came from AGAGE contributor.
2-1864	2	48	14			Twice the same numbers, one set must be wrong. [Francois DANIS, France]	Corrected.
2-1865	2	48	22		24	if some comment is to be retained about the radiative impact of halons, I would think it should be on their indirect effects owing to the destruction of stratospheric ozone--these influences are much larger than their direct radiave influence. [Stephen Montzka, USA]	Moved to appendix and reworded.
2-1866	2	48	24	48	24	"	Added "emissions".
2-1867	2	48	26	48	26	The section title should be changed to "Short-Lived Greenhouse Gases and Aerosols", because aerosols are not greenhouse gases. [JOHN OGREN, USA]	We have two separate sections, short lived greenhouse and other climate relevant gases; and a separate section on aerosol.
2-1868	2	48	28	48	29	Why is there nothing on water vapour in the troposphere? [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	included earlier in hydrological cycle, new section xxx
2-1869	2	48	29	48	29	vapor → vapour [Peter Burt, UK]	done.
2-1870	2	48	29	48	29	It looks like surface & column aerosols are in 2.4.3, not 2.4.2. So I'd recommend deleting their mention here. (otherwise, I'd recommend retitling the section to "short lived greenhouse substances" instead of gases) [Marcus Sarofim, USA]	We have two separate sections, short lived greenhouse and other climate relevant gases; and a separate section on aerosol.
2-1871	2	48	29	48	29	Why discuss aerosol in this section on SLGHGs? [Dian Seidel, USA]	We have two separate sections, short lived greenhouse and other climate relevant gases; and a separate section on aerosol.
2-1872	2	48	31	48	31	What's a "standard method"? [Karen Rosenlof, United States of America]	rephrased: single accepted method.
2-1873	2	48	32	48	34	But the effects on uncertainties should be assessed, even if it is not possible to quantify. [Jan Fuglestvedt, NORWAY]	Indeed we have stated that these issues add an unknown uncertaint.

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2-1874	2	48	36	49	29	In section 2.4.2.1, mention the absolute values of O3 concentration about which trends occur. This approach is adopted extensively in previous chapter 2 sections on greenhouse gases, and makes the messages clearer. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	We haved added a sentence: Annual average ozone levels range from less than 20 ppbv at the South Pole to more than 70 ppbv at Mt Happo, Japan.
2-1875	2	48	36	50	29	I'd think the role of CH4 in background O3 levels would be worth mention somewhere in this section. [Marcus Sarofim, USA]	Due to space restrictions we were not able to address this remark; it is however discussed in Chapter 8
2-1876	2	48	36	50	30	Owen Cooper has carefully reviewed all references and discussion in this section. He has shared his comments with me. I have read his comments and support them. Below I will add my own additional comments. [David Parrish, USA]	we have included Mr. Coopers comments.
2-1877	2	48	36			Do you want to include some information on new, very short-lived, HFCs in Seection 2.4.2? These are designed as very low GWP replacements for current use refrigerants. DuPont and Honeywell are commercializing HFO-1234yf as a replacement for HFC-134a in automotive refrigeration. If you want to include something you could use some of the material in the latest WMO assessment (Scientific Assessment of Ozone Depletion-2010). For example here is some material from that report "Since the previous Assessment, new fluorocarbons have been suggested as possible replacements for potent HCFC and HFC greenhouse gases. For example, HFC-1234yf (Ozone Depletion Potential (ODP) = 0; 100-year GWP = 4) is proposed to replace HFC-134a (ODP = 0; 100-year GWP = 1370) in mobile air conditioning. Preliminary analyses indicate that global replacement of HFC-134a with HFC-1234yf at today's level of use is not expected to contribute significantly to tropospheric ozone formation or produce harmful levels of the degradation product TFA (trifluoroacetic acid). References: Hurley, M.D., T.J. Wallington, M.S. Javadi, and O.J. Nielsen, Atmospheric chemistry of CF3CF=CH2: Products and mechanisms of Cl atom and OH radical initiated oxidation, Chem. Phys. Lett., 450 (4-6), 263-267 (2008) and Luecken, D.J., R.L. Waterland, S. Papasavva, K.N. Taddonio, W.T. Hutzel, J.P. Rugh, and S.O. Andersen, Ozone and TFA impacts in North America from degradation of 2,3,3,3-tetrafluoropropene (HFO-1234yf), a potential greenhouse gas replacement, Environ. Sci. Technol., 44 (1), 343-348 (2010). [Robert Waterland, United States of America]	There is insufficient space to add VSL HFCs.
2-1878	2	48	38	50	29	this section comes across as a review, not an assessment… it also seems inordinantly long relative to the other sections in 2.4. [Stephen Montzka, USA]	We have completely revised this section, and substantially reduced the length.
2-1879	2	48	38			Here it would be helpful to point out that because ozone is so variable, long time series are needed to confidently calculate trends. Fischer et al. (2011) used ozone measurements from Mt. Bachelor Oregon to determine that the detection of an ozone trend of 1% per year requires 13 years of data. To detect a trend of 1.5% per year requires 10 years of data, and to detect a trend of 0.5% per year requires 20 years of data. So unless ozone is changing rapidly, a 10 year ozone time series is not very useful for calculating a trend. E. V. Fischer, D. A. Jaffe, and E. C. Weatherhead, Free tropospheric peroxyacetyl nitrate (PAN) and ozone at Mount Bachelor: potential causes of variability and timescale for trend detection, Atmos. Chem. Phys., 11, 5641-5654, 2011 [Owen Cooper, USA]	This is a useful remark, in our supplementary table we have included the 95 % signficicance, as well as the standard deviations of the trends. These confirm the statement of the reviewer
2-1880	2	48	39	48	39	Specify that the lifetime of a few weeks only applies to tropospheric ozone, and not stratospheric: "…with the relatively short average atmospheric lifetime of O3 (a few weeks in the troposphere) and its…." [Owen Cooper, USA]	Due to space restrictions we were not able to address this remark; it is however discussed in Chapter 8
2-1881	2	48	39	48	39	The ozone lifetime (against photochemical loss) varies from a few days to several weeks. Since ozone is also produced photochemically over source regions and downwind (PAN decomposition), the concept of lifetime is not that useful but the dependence of ozone on sources and sinks does lead to large variability. This needs to be clarified in the text. [Katharine Law, France]	Due to space restrictions we were not able to address this remark; it is however discussed in Chapter 8
2-1882	2	48	40	48	40	"long-term tropospheric O3 trends" [Dale Hurst, United States of America]	the text now mentions long-term regional trends
2-1883	2	48	42	48	42	just checking here, were there really ozone observations in "the late 19th century" ? [Dale Hurst, United States of America]	yes, we are referring to the so-called Schoenbein methodology.
2-1884	2	48	42	48	42	"semi-quantitative observations" is very unclear [Dale Hurst, United States of America]	we changed semi-quantitative=>low accuracy
2-1885	2	48	43	48	43	Here the term "background tropospheric ozone" is used. In the UN's recent Hemispheric Transport of Air	we have deleted this statement.

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						Pollution 2010 Assessment Report, hemispheric background ozone is defined as ozone from natural sources only. According to HTAP, baseline ozone refers to the ozone measured at a location with no recent influence from local sources of emissions. So for coastal sites where measurements influenced by local emissions can be removed, the resulting data set is referred to as baseline. The term baseline seems more appropriate for this particular part of the IPCC report. Dentener F., T. Keating and H. Akimoto (eds.) Hemispheric Transport of Air Pollution 2010, Part A: ozone and Particulate Matter, Air Pollution Studies No. 17, United Nations, New York and Geneva, ISSN 1014-4625, ISBN 978-92-1-117043-6. [Owen Cooper, USA]	
2-1886	2	48	44	48	45	"suggest an overall upward trend of 0.3–0.5% yr–1" [Dale Hurst, United States of America]	Due to space restrictions we we have removed this quantification.
2-1887	2	48	44	48	45	This sentence about 2-3 decade trend could be misleading. It could be interpreted at a global trend, even though the observations are spatially limited and tropopsheric ozone is not well mixed. Also, it's unclear if the coastal observations are at the surface or aloft, and if the aircraft data are at flight level or on ascent/descent. So there is lots of room for misunderstanding. Furthermore, the "flattening of the this trend between 2000-2010" would represent about 1/2 to 1/3 of the overall 2-3 decades, so does it even make sense to talk about an increase over the longer period? [Dian Seidel, USA]	This statement was rephrased as follows:Surface sites with significantly increasing ozone are not always associated with regional increases in anthropogenic emissions. In East Asia, where emissions are growing faster than any other region on Earth, almost all surface sites show increasing ozone. However, in the western USA where emissions are decreasing, springtime ozone is increasing at rural coastal sites and at half of the available inland rural sites, possibly due to its location downwind of Asia (Jacob et al., 1999; Cooper et al., 2012). Ozone is also increasing in winter at nearly half of the rural sites in the eastern USA for as yet unknown reasons (Cooper et al., 2012). Ozone increased in Europe from the 1950s and 1970s until approximately the year 2000. Emissions increased in Europe and North America up until the 1980s, then levelled off and began to decrease in the 1990s. The continued increase of ozone during the 1990s is unexpected considering Europe's decreasing emissions (Logan et al., 2012). Surface ozone increases have also been detected in remote locations such as the Canadian Arctic (Alert), Hawaii (Mauna Loa), the Western North Atlantic (Bermuda) in winter and summer, the South Atlantic mid-latitudes, the Eastern South Atlantic tropics, and southern Australia (Cape Grim).
2-1888	2	48	45	48	46	"…but a flattening of this trend…in many (but not all) locations.." This phrase is too general and gives the worng impression - differences between what is happening in Asia versus Europe and N; America should at least be mentioned. [Katharine Law, France]	This statement was rephrased as follows:Surface sites with significantly increasing ozone are not always associated with regional increases in anthropogenic emissions. In East Asia, where emissions are growing faster than any other region on Earth, almost all surface sites show increasing ozone. However, in the western USA where emissions are decreasing, springtime ozone is increasing at rural coastal sites and at half of the available inland rural sites, possibly due to its location downwind of Asia (Jacob et al., 1999; Cooper et al., 2012). Ozone is also increasing in winter at nearly half of the rural sites in the eastern USA for as yet unknown reasons (Cooper et al., 2012). Ozone increased in Europe from the 1950s and 1970s until approximately the year 2000. Emissions increased in Europe and North America up until the 1980s, then levelled off and began to decrease in the 1990s. The continued increase of ozone during the 1990s is unexpected considering Europe's decreasing emissions (Logan et al., 2012). Surface ozone increases have also been detected in remote locations such

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							as the Canadian Arctic (Alert), Hawaii (Mauna Loa), the Western North Atlantic (Bermuda) in winter and summer, the South Atlantic mid-latitudes, the Eastern South Atlantic tropics, and southern Australia (Cape Grim).
2-1889	2	48	49	48	49	"between decadal ozone changes" [Dale Hurst, United States of America]	Due to space restrictions we we have removed this sentence
2-1890	2	48	49	48	56	I recommend this section on Europe be rewritten. Begin with the trend values from Wilson et al. 2011, who give annual and seasonal trends. They found positive ozone trends on an annual basis and for winter, spring and summer, but negative trends for autumn. Also point out the regional differences in trends. Point out that these trends are only for 1996-2006 and when longer records are used different results can be obtained. For example Pozolli et al 2011 calculate ozone trends for 1990-2005 and find significant increases in winter but no significant change in summer. Then use the Arkona-Zingst, Arosa and Hohenpeissenberg trends reported in the new paper by David Parrish and colleagues. At the time of this writing the paper is still in preparation but will be submitted to ACPD or JGR very soon and should be ready for the next draft of WG1 (please contact David.D.Parrish@noaa.gov for a copy). Arkona-Zingst is on the German Baltic coast and is representative of boundary layer ozone in northern Europe, showing strong increases in ozone in all seasons from 1956-2010. Hohenpeissenberg is on a hill 200-300 m above the surrounding countryside of southern Germany, is surrounded by cities, is right in the middle of Europe and is entirely representative of the western European boundary layer. Hohenpeissenberg is not a remote background monitoring site for Europe such as Mace Head or Zugspitze. Parrish et al 2012 show that ozone increased in all seasons during 1971-2000 at Hohenpeissenberg but from 2000-2010 there has been no increase in ozone in any season. Arosa is in a Swiss Valley and also shows strong ozone increases from the 1950s until 2000, but no increase since 2000. If anything, ozone has decreased at Arosa since 2000. [Owen Cooper, USA]	Following the suggestion of this reviewer we completely revised the tropospheric ozone section
2-1891	2	48	50	48	53	Here ozone trends are discussed for Europe, stating that ozone is increasing in winter and decreasing in summer during 1990-2005. Both Pozzoli et al. 2011 and Jonson et al. 2006 are given as references. But the reported findings from these papers aren't entirely accurate. Pozzoli et al report trends for 1990-2005 that show significant ozone increases in winter, but their reported ozone decreases for summer are not statistically significant. Jonson et al. show trend lines for 1990-2002 (not 2005) that suggest ozone has decreased in summer and increased in winter. But because they do not report the uncertainty of the slopes we cannot be sure if the summer decreases are statistically significant. Seeing as the Jonson results are not reported in Table 2.13, this part of the text would be clearer if the Jonson et al reference were dropped. [Owen Cooper, USA]	We dropped the references to Pozzoli and Jonson.
2-1892	2	48	51	48	52	author names out of alphabetical order [Peter Burt, UK]	We dropped the references to Pozzoli and Jonson.
2-1893	2	48	52			explain why there would be positive winter trends. [Ruth Doherty, UK]	we have mentioned 'as for yet unknown reasons
2-1894	2	48	53	48	53	Changes are *likely* to be due to reductions in precursor emissions [Katharine Law, France]	we agree, but as not completely clear, we have mentioned 'as for yet unknown reasons'
2-1895	2	48	53	48	54	It would be easier to understand things if +- numbers consistently were 2 standard errors, as here (but not everywhere). [Peter Guttorp, USA]	we consistently used 2 sigma.
2-1896	2	48	54	48	54	0.16+/-0.03 should be 0.16+/-0.02 [Owen Cooper, USA]	corrected
2-1897	2	48	55	48	55	th' as superscript (x 2) [Peter Burt, UK]	due to space restrictions sentence has been deleted.
2-1898	2	48	56	48	57	Here it's okay to mention the results from Chan 2009, but it would be much clearer if you first reported the trends from Pozzoli et al 2011, who use longer time series and don't make any temperature adjustment. In the five US regions analyzed by Pozzoli et al. only the northeast US has a significant positive trend in winter during 1990-2005. The other 4 regions have no winter trend. In summer the only significant trends are upward in the western US and downward in the Mid-Atlantic states. Also, Oltmans et al. 2006 show that ozone on top of Whiteface Mountain in northern New York State	due to space restrictions sentence has been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						increased from 1973-1990 with little change during 1990-2004, on an annual basis. During spring-summer ozone at the site declined, while it increased in winter. Oltmans et al. (2006), Long-term changes in tropospheric ozone, Atmospheric Environment, 40, 3156–3173. [Owen Cooper, USA]	
2-1899	2	48	57	48	57	I think I know what is meant by 'temperature-adjusted' trends, but I don't think most readers will understand this immediately. I [Klaas Folkert Boersma, Netherlands]	due to space restrictions sentence has been deleted.
2-1900	2	48	57	48	57	reports → reported [Peter Burt, UK]	due to space restrictions sentence has been deleted.
2-1901	2	48	57	48	57	What is meant by "temperature-adjusted decreasing trends" and would anyone not working in this field understand why trends (or is it ozone concentrations) would require (or warrant) temperature adjustment? [Dian Seidel, USA]	due to space restrictions sentence has been deleted.
2-1902	2	48				suggest to include some explanation. [Klaas Folkert Boersma, Netherlands]	due to space restrictions sentence has been deleted.
2-1903	2	49	1	49	5	explain if these are annual-mean trends. [Ruth Doherty, UK]	due to space restrictions sentence has been deleted.
2-1904	2	49	10	49	10	less-polluted → less polluted [Peter Burt, UK]	due to space restrictions sentence has been deleted.
2-1905	2	49	12	49	12	MOZAIC does not provide a 3D representation of anything. It gives profiles that are specific to only certain airport locations, so would be biased towards urban areas for profiles, and to flight corridors at flight levels between ~32kft and 42 kft. [Karen Rosenlof, United States of America]	The sentence has been removed. Appendix Table uses MOZAIC data; e.g. Schnadt Poberai
2-1906	2	49	13	49	13	Does "from the west" mean from oceanic regions? Could it be misinterpreted as from developed countries? [Dian Seidel, USA]	due to space restrictions sentence has been deleted.
2-1907	2	49	16	49	17	I think the way this sentence is phrased gives too much weight to the Hess and Zbinden study. It is true that natural variability has a role to play in ozone inter-annual variabilty but we also know that emissions have increased in general, at least up to the early 2000s in most regions of the Northern Hemisphere. [Katharine Law, France]	The sentence has been removed. Appendix Table uses in one case the results from Hess and Zbinden.
2-1908	2	49	17	49	17	"stratospheric ozone transported into the troposphere" [Dale Hurst, United States of America]	due to space restrictions sentence has been deleted.
2-1909	2	49	17	49	17	The wording "stratospheric ozone transport in the troposphere" is unclear. When stratospheric ozone enters the troposphere, doesn't it become tropospheric ozone? Perhaps you mean "tropospheric transport of ozone originating in the stratosphere"? [JOHN OGREN, USA]	due to space restrictions sentence has been deleted.
2-1910	2	49	17	49	17	Should "in the troposphere" be changed to "to the troposphere". [Dian Seidel, USA]	due to space restrictions sentence has been deleted.
2-1911	2	49	19	49	20	You refer to Mace Head, Zugspitze and Hohenpeissenberg as background sites. If you filter the data at Mace Head to remove events with local effects, or filter data at Zugspitze to remove events with strong boundary layer influence you can produce baseline (or background) ozone records. But Hohenpeissenberg is well within the south German daytime boundary layer and does not represent baseline air flowing into Europe, rather it can be described as a regionally representative site. [Owen Cooper, USA]	we show Hohenpeissenberg data filter for local pollution
2-1912	2	49	20	49	20	timeseries → time series [Peter Burt, UK]	OK
2-1913	2	49	22	49	23	why differentiate between summer and winter trends if they are not statistically different ? [Dale Hurst, United States of America]	In the text we do not longer discuss seasonal trends.
2-1914	2	49	25	49	25	change text to 'The anlaysis of Logan et al. (2011), combining..' [Peter Burt, UK]	sentence has been rephrased.
2-1915	2	49	25	49	28	The sources of the data for the trends reported here are incorrect. These particular trends are for just the three combined alpine sites of Zugspitze, Jungfraujoch and Sonnblick, and do not include MOZAIC or sondes. [Owen Cooper, USA]	corrected
2-1916	2	49	25	49	28	I think that the three decadal increases given by Logan et al., 2011 needs revision. The problem is that these	To our understanding this problem has been solved in

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						increases are based upon linear regressions to successive 10-year segments of data, and the linear regressions do not connect with each other in a physically reasonable manner. As a consequence the sum of the three decadal increases does not equal the total increase over 30 years. This issue is being discussed in the review process of this paper. [David Parrish, USA]	the final version of Logan (2012)
2-1917	2	49	26	49	26	Consider replacing "corrected" with "adjusted", unless you are sure all inconsistencies have been removed and the data are now "correct". [Dian Seidel, USA]	sentence has been rephrased.
2-1918	2	49	32	49	35	Here you state that Oltmans et al. (2008) conclude that local O3 production rather than long-range transport contributed to the trends at Lassen and Yreka. I could not find such a specific statement in Oltmans et al., rather I think the best summary statement they make regarding these two sites is in the abstract: “Two inland locations (Yreka and Lassen Volcanic National Park) in northern California with surface ozone data records of 20 years or more are more difficult to interpret because of possible influences of local or regional changes.” A short-coming of Oltmans et al. (2008) is that they did not discuss the west coast ozone trends in terms of US emissions. Granier et al. (2011, in the Chapter 2 reference list) show that ozone precursor emissions in the US have declined since 1980, and it is difficult to reconcile increasing ozone in the western USA with declining US ozone precursor emissions. Therefore it seems more likely that the increase in western US ozone is due to baseline influences. [Owen Cooper, USA]	sentence has been rephrased.
2-1919	2	49	32	49	35	The analysis of Oltmans et al. (2008) for Yreka in California is described as an analysis of a remote site. However, as discussed in detail by Parrish et al. (2009) the Yreka site is actually better described as a polluted urban site. It is located in a town immediately adjacent to the major north-south interstate highway (I-5) that connects urban centers along the west coast of the United States (see map in Fig. 9 of Parrish et al.). Further, this town is located within a narrow valley, which serves to trap the local emissions. These local emissions titrate ozone, and the influence of this titration is clearly reflected in the strong diurnal cycles seen in the data; on many nights ozone is reduced to near zero. It is my strong opinion that the Yreka data must not be included in any discussion of hemispherically or regionally representative tropospheric O3. [David Parrish, USA]	Reference to this site has been deleted
2-1920	2	49	36	49	36	do → did [Peter Burt, UK]	sentence has been deleted
2-1921	2	49	38	49	38	Which curve in Fig. 2.24 is this trend referring to? The purple one doesn't clearly show 8 stations. And over what time period is the trend, given the different periods of data records? [Dian Seidel, USA]	In the SOD a revised set of stations is shown.
2-1922	2	49	39	49	40	"…buf fletten in Europe" - again too general and not consistent with what written in the text about Wilson et al (2011) (page 48) which concerns analysis of stations in rural or remote locations. [Katharine Law, France]	The statement now reads: Significant decreases in surface ozone have occurred in the regions with strong decreases in local emissions: Europe since 2000; median values in rural eastern USA in spring and summer since 1990; and highest ozone values at many urban sites across the USA since 1980 (Lefohn et al. 2010).
2-1923	2	49	40	49	40	Here you cite Parrish et al. 2009 and state that the season with the greatest ozone trend along the west coast is winter with a trend of 0.45 +/-0.13. This must be in error. I could not find the value 0.45+/-0.13 ppbv/year anywhere in the paper, and according to Table 5, the greatest trend is 0.46+/-0.13 ppbv/year during spring. [Owen Cooper, USA]	thank you, it is correct now.
2-1924	2	49	43	49	44	"trends were stronger in air with traceable boundary layer influences in south and east Asia." [Dale Hurst, United States of America]	sentence has been deleted
2-1925	2	49	45	49	45	remove "increase" from "with significant increase rates " [Dale Hurst, United States of America]	sentence has been deleted
2-1926	2	49	47	49	47	delete comma after 2006 [Peter Burt, UK]	sentence has been deleted
2-1927	2	49	49	50	18	This discussion of surface ozone trends should be shorter. It should make an expert assessment of the trends rather than listing all literature studies. It should also explain why this is relevant to IPCC (evidence of anthropogenic influence on ozone, air quality impacts in chapter 11). [William Collins, United Kingdom of Great	Section has been written, and relevance to IPCC is made clear.

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						Britain & Northern Ireland]	
2-1928	2	49	51	49	56	It could be interesting to discriminate the remote stations from the others. I presume that Pacific rim sites are not remote. [Claudio Cassardo, Italy]	In table appendix a clear distinction has been made among regions.
2-1929	2	49	51			There is much more information on ozone trends in east Asia than what has been included in Table 2.13. Following is my summary: For Japan, Naja and Akimoto [2004] report springtime ozone increases in the boundary layer from the period 1970-1985 to the period 1986-2002 at three ozonesonde sites. However, in more recent years through 2006 no increase in boundary layer ozone has occurred at these sites [Naja and Akimoto, 2004; Oltmans et al., 2006]. Due west of Japan, Beijing (40º N) experienced ozone increases in the planetary boundary layer of 5-8 ppbv between 1995-1999 and 2000-2005, as measured by MOZAIC aircraft profiles [Ding et al., 2008]. Further south in the region of Taipei, Taiwan (25° N), springtime surface ozone increased during 1994-2003 at the rate of 0.71, 0.58 and 1.17 ppbv year-1 at coastal, elevated and urban locations, respectively [Chou et al., 2006]. Analysis of a background ozone monitoring site in northern Taiwan yielded an annual ozone rate of increase of 0.58+/-0.21 ppbv per year for 1994-2007 [Lin et al., 2010]. In southern Taiwan ozone has increased by 26% from 1997-2006 [Li et al., 2010]. In southern China, average yearly ozone increased by about 50% between the late 1980s and 1990s at an urban monitoring site in Hong Kong (22º N) [Chan et al., 2003]. A coastal site southeast of Hong Kong shows an ozone rate of increase of 0.58 ppbv year-1 for yearly data during 1994-2007 [Wang et al., 2009]. Chan, et al., Urban and background ozone trend in 1984-1999 at subtropical Hong Kong, South China, Ozone-Science & Engineering, 25, 513-522 (2003). Chou, et al., The trend of surface ozone in Taipei, Taiwan, and its causes: Implications for ozone control strategies, Atmos. Environ. 40, 3898–3908 (2006). Ding, et al., Tropospheric ozone climatology over Beijing: analysis of aircraft data from the MOZAIC program, Atmos. Chem. Phys., 8, 1–13 (2008). Li et al., (2010)Meteorologically adjusted long-term trend of ground-level ozone concentrations in Kaohsiung County, southern Taiwan, Atmos. Environ., 44, 3605-3608. Naja, M., and H. Akimoto, Contribution of regional pollution and long-range transport to the Asia-Pacific region: Analysis of long-term ozonesonde data over Japan, J. Geophys. Res., 109, D21306, doi:10.1029/2004JD004687 (2004). Wang, et al. Increasing surface ozone concentrations in the background atmosphere of Southern China, 1994-2007, Atmos. Chem. Phys., 9, 6217-6227 (2009). [Owen Cooper, USA]	New information has been included in the Appendix table.
2-1930	2	49	53	49	54	The sentence "… are also larger than anywhere else in the world" could be changed to "… are also larger than anywhere else in the world, suggesting increasing Asian anthropogenic emission." [Yoko Yokouchi, Japan]	sentence has been deleted
2-1931	2	49	54	49	54	"Pacific rim sites" is not descriptive at all. They could be on the Chilean coast, Alaska, etc. [Dale Hurst, United States of America]	sentence has been deleted
2-1932	2	49	54	49	55	Need to add something about what is shown in HTAP (2010) on the Japenese sites. A paper on this is going to be submitted soon (Parrish et al., 2012), which includes an update to Figure 2.24. Data at Rishiri Island at least shows positive trends. [Katharine Law, France]	Paper has now been utilized.
2-1933	2	49	54	49	56	The summary of Tanimoto et al 2009 is not quite accurate. They looked at 8 low altitude sites around Japan and found that all showed positive ozone trends over 10 years (1998-2007), but only one was significant. [Owen Cooper, USA]	is addressed in Appendix Table on O3 trends
2-1934	2	50	1	50	29	Is the discussion of satellite total ozone complete? There is no reference to the combined TOMS, GOME-GOME-2, SCIAMACHY record being worked on in Europe which I believe are showing substantial progress. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	The reference to satellite data has been substantially reduced
2-1935	2	50	2	50	2	I suggest changing "springtime" with the corresponding months "March-April-May" or "boreal springtime" [Celeste Saulo, Argentina]	Where this was possibly confusing it was corrected
2-1936	2	50	2	50	9	Figure 2.24. Don't we want to show some globally remote sites like Mauna Loa, Samoa, Cape Point, Cape	done.

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						Grim, Baring Head, and South Pole? These are pretty far from local influences and can serve as a reference point for other sites. [James Butler, United States of America]	
2-1937	2	50	5	50	5	(Fig. 2.24 caption) Typo "Hohenpeissenberg (not "Hohenpiessenberg"). [Christian-D. Schoenwiese, Germany]	corrected
2-1938	2	50	5	50	5	Spelling mistake, change 'Hohenpiessenberg' to 'Hohenpeissenberg'. Suggest to search the entire document for this, it re-occurs p. 153, l. 7 [Martin Vollmer, Switzerland]	corrected
2-1939	2	50	11	50	12	The ozonesonde record at Hilo Hawaii began in 1982, but the surface ozone measurements at Mauna Loa began in 1973. So on line 12 you need to say 1973-2004. [Owen Cooper, USA]	corrected
2-1940	2	50	12			explain what is meant by "dynamical effects" [Ruth Doherty, UK]	sentence has been deleted
2-1941	2	50	13	50	13	"possibly due to dynamical effects." What kind of effects? Increases in STE (since MLS is a high-altitude site - should be mentioned) [Dale Hurst, United States of America]	sentence has been deleted
2-1942	2	50	14	50	17	Here you mention 3 sites with increasing ozone but do not specify which sites. I assume you are talking about the mid-latitude sites? Also the increase mainly occurs in spring. Where did you get the trend values of 0.3-0.5% per year? I could not find these values in the paper, other than the value for Cape Point. This part would be more accurate if you paraphrased the following sentence from the conclusion of Oltmans et al (2006): “At mid latitudes of the S.H. three time series of moderate length (20 years) agree in showing increases that are strongest in the austral spring (August–October).” I don’t agree with the interpretation that the other sites are broadly consistent with the behavior of precursor emissions. Oltmans et al. could give no explanation for the decrease of ozone at Samoa, as CO had not changed. [Owen Cooper, USA]	sentence has been deleted
2-1943	2	50	17	50	17	Here you say that the HTAP 2010 report states that models qualitatively reproduce the ozone trends in the southern hemisphere. But the HTAP 2010 report deals only with the northern hemisphere so I don’t see how you can draw this conclusion. The recent paper by Lamarque et al. does compare modeled ozone trends to observations in the southern hemisphere but the results are not very good: Lamarque, J.-F., et al. (2010), Historical (1850–2000) gridded anthropogenic and biomass burning emissions of reactive gases and aerosols: methodology and application, Atmos. Chem. Phys., 10, 7017-7039. [Owen Cooper, USA]	sentence has been deleted
2-1944	2	50	17	50	18	Strange sentence to be included here - either add a more complete discussion or remove or link to another chapter. In any case, I do not agree that models reproduce the trends "qualitatively". What does this mean? Cite recent papers like Lamarque et al (2010) or Wild et al. (2012). [Katharine Law, France]	sentence has been deleted
2-1945	2	50	20	50	21	just checking here, are satellite retrievals really used to derive "trends of tropospheric ozone columns" ? [Dale Hurst, United States of America]	yes, although the accuracy is not very good.
2-1946	2	50	20	50	21	This is a useful remark. There are probably lots of areas in which progress has not been made. Was there any effort to identify those and make similar remarks elsewhere in the chapter? [Dian Seidel, USA]	Unfortunately, due to space limitation this sentence was remvoed.
2-1947	2	50	20	50	28	Please state why there has been no progress (ie, issues with clouds, calibrations, combining data sets). [Karen Rosenlof, United States of America]	Unfortunately, due to space limitation this sentence was remvoed.
2-1948	2	50	20	50	29	I am surprised that SBUV (Since about 1978) and GOME-1/2, Schiamachy since 1995/2002 are not mentioned here. Maybe its better not to mention instruments and just refer to the measurement techniques (e.g. solar UV/VIS) and IR. [Roger Saunders, United Kingdom]	We had to limit our discussion to mention just two key papers.
2-1949	2	50	21	50	23	Salellite data, if reliable, are more important for climate assessment and hence should not be ignored. Why are the tropospheric ozone data in the recent publication “Ziemke, J. R., S. Chandra, G. J. Labow, P. K. Bhartia, L. Froidevaux, and J. C. Witte, 2011：A global climatology of tropospheric and stratospheric ozone derived from Aura OMI and MLS measurements. Atmos. Chem. Phys., 11, 9237–9251.” not used? [Xiaobin Xu, China]	This paper, along with Ziemke's 2005 paper have now been used.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1950	2	50	21			ENSO is mentioned only here. Some text on ENSO and NAO modulation of tropospheric O3 and CO would be useful and fit in with text elsewhere in the chapter. Several papers by Ziemke on ENSO. For the NAO and O3 modulation see Lamarque/Hess (GRL 2004), Creilson et al. 2003 ACP,Eckhardt et al. acp-3-1769-2003, 2003. 3135, Pausata et al. ACPD, 2012, Vol.12, pp. 3131-3167 [Ruth Doherty, UK]	Unfortunately, this chapter does not have extensive space for discussion of drivers of trends.
2-1951	2	50	29			Given that ozone trends around the world are so variable it would be nice to have a few summary sentences to highlight the strongest overall findings. [Owen Cooper, USA]	done.
2-1952	2	50	33	50	42	This section looks brief and hastily put together in comparison to the folloiwng section. Papers like randel et al, 2011 would provide a good starting point for a more complete but concise update. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	The section on stratospheric ozone trends has been updated from the WMO 2011 assessment, and more substantial discussion has been presented.
2-1953	2	50	33	50	42	The section on Stratospheric Ozone (2.4.2.2) is only 10 lines of text whereas tropospheric ozone is 2 pages of text. This surprised me but maybe all the stratospheric ozone issues are in other sections. [Roger Saunders, United Kingdom]	The section on stratospheric ozone trends has been updated from the WMO 2011 assessment, and more substantial discussion has been presented.
2-1954	2	50	33	50	48	There's a wealth of literature, including the recent ozone assessment, discussing stratospheric ozone, recovery and relation to climate and assorted climate indices. Is our understanding really just "medium"? Also, note in figure, 60n-60S is not midlatitude. [Karen Rosenlof, United States of America]	The section on stratospheric ozone trends has been updated from the WMO 2011 assessment, and more substantial discussion has been presented.
2-1955	2	50	34	50	34	insert 'times' after pre-industrial [Peter Burt, UK]	this sentence has been removed
2-1956	2	50	35	50	35	Is "Ajavon et al." the typical way of citing the WMO/UNEP assessments? [Dian Seidel, USA]	this sentence has been removed
2-1957	2	50	36			Consistent with what? [Drew Shindell, USA]	this sentence has been removed
2-1958	2	50	39			above or below? [Shouraseni Roy, USA]	this sentence has been removed
2-1959	2	50	49	50	49	I found it strange to find the stratospheric water vapour section "hidden away" (e.g. it was not in the contents list) in the short-lived greenhouse gas section - I was initially confused as to why Section 2.3 did not discuss stratospheric water vapour, and I believe 2.4.2.3 belongs in Section 2.3. At the very least Section 2.3 should tell the reader, at the start, that SWV is elsewhere, but I can see no justification for classing it as short-lived. Indeed, tropospheric water vapour is much shorter lived than stratospheric water vapour! And unlike the other gases in 2.4.2, its short-lived-ness, at least in the lower stratosphere, is as a result of circulation rather than chemistry. [Keith Shine, UK]	Comment rejected.
2-1960	2	50	51	50	57	Aside from the chemical role with halogen compounds, there is also a chemical role in regards to the impact on PSCs and ozone loss. In regards to description of trends, it is better summed up in the bullets in chapter 4 of the ozone assement in regards to the 2000-2001 change, the increase from 1980-2000 and the radiative impact (and that confidence in model predictions is low). Also on line 56, O3 should be H2O. [Karen Rosenlof, United States of America]	Current statement reflects only impact of X in presence of PSCs; not sure what other chemical role reviewer refers to. Will reword.
2-1961	2	50	52	50	52	A self-serving comment, but to reference Solomon et al (2010) as a source of information on stratospheric water vapour cooling the stratosphere is not adequate, as that paper hardly mentions the issue - some of the older Forster and Shine papers make the warming-troposphere, cooling-stratosphere issue much more clearly. [Keith Shine, UK]	Additional citation added.
2-1962	2	50	52			strictly speaking, it's not the H2O which cause the stratosphere to cool but H2O's effect on O3... [Francois DANIS, France]	Statement refers to radiative effect.
2-1963	2	50	53	50	53	delete comma after) [Peter Burt, UK]	OK as is.
2-1964	2	50	53			halogenated compounds are always present in the stratosphere; so it's not the conditional “when...”. [Francois DANIS, France]	Rewritten.
2-1965	2	50	55	50	55	Does 'This' refer to AR4, or AR5? [Peter Burt, UK]	Reworded.
2-1966	2	50	55	50	56	"identifies" implies this assessment found these trends; "recognizes" is more appropriate. "O3" should be	Changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						"H2O" [Dale Hurst, United States of America]	
2-1967	2	50	55	50	57	This is an attribution statement. Is there any evidence that the change in stratospheric water vapour 2000-2001 actually impacted surface temperatures? 10.3.2. says "the relatively short and sparse record of stratospheric water vapour has inhibited formal trend detection and attribution." [Gareth S Jones, UK]	Offending phrase deleted.
2-1968	2	50	56			“decrease in stratospheric O3”. While it is possibly true, you probably want to speak about H2O instead of O3. [Francois DANIS, France]	Corrected.
2-1969	2	50	56			O3 should be H2O [Drew Shindell, USA]	Corrected.
2-1970	2	51	1	51	8	There is no mention of any non-US satelite H2O measurement here…there are multiple satellites that do measure H2O that are from different countries. Also, offsets in absolute value do not mean that there are significant offsets in trends. [Karen Rosenlof, United States of America]	A more helpful comment would have stated which non-US satellites should be included.
2-1971	2	51	6	51	7	Does that the water vapour mixing ratios trends do not always agree undermines the conclusions of the following two paragraphs? [Gareth S Jones, UK]	Text re-written and conclusions should be consistent with trends.
2-1972	2	51	8	51	8	the period of HALOE-MLS overlap was about 16 months, or 1.25 years [Dale Hurst, United States of America]	Modified.
2-1973	2	51	11	51	20	This seems like unnecessary detail about the Boulder water vapor analysis [Dian Seidel, USA]	Section re-written and in situ measurements de-emphasized.
2-1974	2	51	11	51	33	Four instances of citation "Hurst (2011)" should be changed to "Hurst et al. (2011)", including one in the reference section [Dale Hurst, United States of America]	Citations inserted with EndNote.
2-1975	2	51	11		20	much detail about altitude-specific trends are reported here, but the bottom line is missing, unemphasized. [Stephen Montzka, USA]	A summary is given at the end of the section.
2-1976	2	51	16	51	16	30-year → 30 year [Peter Burt, UK]	OK.
2-1977	2	51	22	51	24	This is another attribution statement that is inconsistent with the statements at the start of the chapter (Page 2-7 L1 to L7) [Gareth S Jones, UK]	Disagree. This statement in the introduction appies to physical climate parameters and is necessarily violated throughout the composition section.
2-1978	2	51	22	51	24	Solomon 2010 did NOT "show" that the stratospheric water vapour slowed surface temperature warming. I recommend you read 10.3.2 (and Solomon 2010). [Gareth S Jones, UK]	Text deleted.
2-1979	2	51	22	51	36	This paragraph is not very clear - which periods are being referred to? Discuss possible causes for strat. H2O changes [Katharine Law, France]	Text deleted.
2-1980	2	51	22	51		Change "showed" to "estimated". Also, this paragraph is hard to read. It needs a figure showing what the various different periods discussed look like…perhaps one from the Hurst et al. paper? [Karen Rosenlof, United States of America]	Text deleted.
2-1981	2	51	22			Rather than 'showed', this should be 'suggested' as this is not so unequivocal that it's 25% precisely. [Drew Shindell, USA]	Text deleted.
2-1982	2	51	23	51	23	Consider adding "could have" before "slowed the rate" [Dian Seidel, USA]	Reworded.
2-1983	2	51	24	51	24	"GHGs" should be "LLGHGs" for consistency [Dale Hurst, United States of America]	Text removed.
2-1984	2	51	30	51	30	add a sentence: "Despite the poor trend agreement during the 1990s, the Boulder record shows a ~1 ppmv difference between stratpspheric water vapor mixing ratios measured during the early 1980s and 2010." [Dale Hurst, United States of America]	Text re-written.
2-1985	2	51	30	51	30	Break old paragraph and start new paragraph with "The Boulder and HALOE records both show negative trends during 2000-2005 that have been explained by anomalously low tropical tropopause temperatures (Fueglistaler and Haynes, 2005; 32 Randel et al., 2006; Rosenlof and Reid, 2008; Randel and Wu, 2010)"	Text re-written.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Dale Hurst, United States of America]	
2-1986	2	51	33	51	33	author names out of alphabetical order [Peter Burt, UK]	Yes
2-1987	2	51	35	51	36	Change this sentence to "The cause of the increase in stratospheric water vapour seen since 2005 in both the FPH and Aura MLS records has not yet been established." [Dale Hurst, United States of America]	Re-written.
2-1988	2	51	36	51	36	In regards to 'have not been established": look at fig 2.26…the 2006-2010 values are positive. [Karen Rosenlof, United States of America]	Text re-written.
2-1989	2	51	38	51	41	I'm not sure that uncertainties have been characterized, but differences between in situ measurements have been. It should perhaps be noted that in a recent field campaingn (MACPEX) that differences between instruments is smaller than in the past, perhaps reflecting improvements made thanks to a previous lab campaign (AQUAVIT). [Karen Rosenlof, United States of America]	We are aware of aquavit, but there were no published papers at time of AR5 FOD.
2-1990	2	51	38	51	41	Note that large discrepancies discussed are mainly between a few select aircraft measurements and the balloon frostpoint. Current satellite measurements by and large agree well with validation ballloon flights. Also, during the recent MACPEX experiment there were smaller differences that in the past...possibly thanks to AQUAVIT> [Karen Rosenlof, United States of America]	OK
2-1991	2	51	43	51	43	Add "spatially limited" before "balloon-borne" [Dian Seidel, USA]	Re-worded.
2-1992	2	51	44	51	45	What is meant by "a discrepancy exists in trends…". Descrepany between instruments, between obs and theory? (I think you mean between obs and theory as regards to observed temperatures, but am not sure, it should be clearly stated. [Karen Rosenlof, United States of America]	Statement deleted.
2-1993	2	51	45	51	47	This sentence unfairly compares a 30-year trend to an 18-year trend. It would be better to compare trends over the same time period. Break the one sentence into two. "The balloon-borne observations over Boulder, Colorado indicate a net increase of 0.7 ppm for 1990–2010 while the adjusted global satellite data suggest no net increase during this same period. Both data sets show a step-like decrease after 2000 and increases since 2005." [Dale Hurst, United States of America]	Summary re-written.
2-1994	2	51	47	51	47	What is the 0,2 ppm (and it should be ppmv) trend based on? Which satellites? There is no continuous satellite data set from 1992 to 2010. [Karen Rosenlof, United States of America]	ppm, an abbreviation for μmol mol^{-1}, is the correct SI unit, not ppmv. Summary re-written.
2-1995	2	51	48	51	48	insert commas either side of 'unfortunately' [Peter Burt, UK]	Text re-written.
2-1996	2	51	49	51	49	Is the community in agreement about this "good understanding", or are there not still a lot of questions about stratospheric water vapor? I'd suggest changing "good understanding" to acknowledgment of the "correlation" between water vapor and tropical tropopause temperature changes. [Dian Seidel, USA]	Reworded to better support this claim.
2-1997	2	51	51	51	51	replace "period 3" with "2000-2005" [Dale Hurst, United States of America]	This text deleted.
2-1998	2	51	55	51	57	There needs to be an explanation as to how this figure was constructed. What is the base period for the anomalies. How were the two satellite data sets matched? [Karen Rosenlof, United States of America]	Information now in caption and reference included in caption.
2-1999	2	52	1	52	6	As in the previous comment, I wonder about these short trend periods. [Dian Seidel, USA]	Indeed the section rather prefer to discuss interannual variability. OH is highly dependent on variability of driving processes.
2-2000	2	52	2	52	25	It needs to be stated at the beginning of this section that OH trends cannot be measured - they are derived and subject to large uncertainty to due to the extremely short OH lifetime and very large variabilty; Large uncertainties also surround its sources and sinks. Several papers have appeared in the last few years discussing this topic (e.g. Hofzumahaus et al., 2009); It is also worth noting that halogens can impact oxidizing capacity locally (e;g. Thornton et al., 2010). [Katharine Law, France]	Text modified, shortened, and included in CH4 section (2.2.1.1.2).
2-2001	2	52	2			Section 2.4.2.4 on OH may fit better under the methane discussion (2.4.1.1.2) rather than as a short-lived greenhouse gas [William Collins, United Kingdom of Great Britain & Northern Ireland]	Text modified, shortened, and included in CH4 section (2.2.1.1.2).

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2-2002	2	52	4	52	5	Is current confidence in reported trends (low) lower than when AR4 reported no change? If so, please clarify that. It's important to document an increase in uncertainty, if that's the case. [Dian Seidel, USA]	Text modified, shortened, and included in CH4 section (2.2.1.1.2).
2-2003	2	52	7	52	10	These "trend" periods are very short. Do these studies even belong in this report? [Dian Seidel, USA]	Yes. OH trends, despite there large uncertainties and short assessment periods, are pivital to understanding GHG budgets.
2-2004	2	52	9	52	9	Delete comma after 'uncertainties' [Peter Burt, UK]	Text modified, shortened, and included in CH4 section (2.2.1.1.2).
2-2005	2	52	9		10	"no statistically significant trend" mentioned, but what time interval is being referred to here? [Stephen Montzka, USA]	Text modified, shortened, and included in CH4 section (2.2.1.1.2). There it states there is no new information to change conclusion from AR4 for 1979 to 2004.
2-2006	2	52	12	52	12	"analyzed time series of 6 tracers with predominant OH sink mechanisms to show" [Dale Hurst, United States of America]	Reworded and moved to section 2.2.1.1.2.
2-2007	2	52	12	52	12	Is it really necessary to introduce the acronym IAV? [Dian Seidel, USA]	IAV is an abbreviation.
2-2008	2	52	12	52	17	Add a remark that limited IAV does not preclude a trend in CH4 lifetime on decadal time scales and add for this a reference for isotopic constraints on methane lifetime in recent decades that suggests a methane lifetime decrease which is also shown by (some?) models in Chapter 8, section 8.2.3.3, figure 8.11 Monteil et al., Atmos. Chem. Phys., 11, 9141-9153, doi:10.5194/acp-11-9141-2011, 2011. See also similar discussion in Ch.6, section 6.3.3.3 [Michiel van Weele, The Netherlands]	Text modified and moved. The suggested citation does not provide a strong constraint on OH trends.
2-2009	2	52	14		17	I agree that "Small IAV in global mean [OH] does not preclude larger regional variations…" but it is important to mention that it also does not preclude larger variations on timescales much longer than 1 year (decadal or centuries). [Stephen Montzka, USA]	Text modified, shortened, and included in CH4 section (2.2.1.1.2). Longer time scales go beyond the scope of this section.
2-2010	2	52	14			since 1985 (not 1984), according to Montzka et al. (2011b). [YUGO KANAYA, Japan]	Good catch, but this text deleted.
2-2011	2	52	15	52	15	(Manning et al., 2005) → Manning et al. (2005) [Peter Burt, UK]	OK
2-2012	2	52	15	52	22	Reference style of (Wang et al., 2008) [Yutaka Kondo, Japan]	OK
2-2013	2	52	19	52	25	Are the Wang (2008) and Montzka (2011) OH values equally likely? What is the expert assessment? If the longer times are credible they should be fed into the GWP calculations in chapter 8. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Montzka 2011 only looked at IAV. Wang et al. deleted in modified text.
2-2014	2	52	19			OH initiates oxidation of these gases, yes, but perhaps more relevant to connect for the reader OH oxidation to limiting the climate impacts of these gases. The point is made earlier, would seem good to reinforce here. [Stephen Montzka, USA]	Point now made in section 2.2.1.1.2.
2-2015	2	52	20	52	20	I'm not sure what is meant by "concentration combined". Do you mean "concentration, combined with rate coefficients"? [James Butler, United States of America]	Corrected.
2-2016	2	52	20	52	20	Is there an extra, or missing, word here? [Dian Seidel, USA]	Corrected.
2-2017	2	52	20	52	20	Replace "and rate coefficients" with "with rate coefficients". [Robert Waterland, United States of America]	Corrected.
2-2018	2	52	21		25	The Wang study is subject to substantial uncertainties in emissions and transport that were important to the analysis of CH3CCl3 observations during 1988-1994. This result is less relevant to today because emissions have since declined dramatically and the resulting exponential decay observed for atmospheric CH3CCl3 since 1998 has improved our understanding of its budget. Uncertainties in accurately deriving OH from CH3CCl3 remain, but they stem from uncertainties in the OH-CH3CCl3 rate constant and in our understanding of non-OH losses for CH3CCl3. The Wang paper does not address/constrain unceratinties related to these issues. [Stephen Montzka, USA]	Wang et al. deleted.

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2-2019	2	52	22	52	22	(Wang et al., 2008) → Wang et al. (2008) [Peter Burt, UK]	Lay-out questions will be dealt with at the final stage of the report
2-2020	2	52	24			Changes in lifetimes would also affect GTPs not only GWP; so "… and GTPs" could be added. [Jan Fuglestvedt, NORWAY]	Mention of GWPs removed.
2-2021	2	52	27	52	37	The section on CO is very weak as it stands and requires improvement. Some discussion about regional variations in CO trends is required. In addition, the general statement about a general decline in CO concentrations is not consistent with increasing emissions over Asia - also shown in Granier et al. (2011). [Katharine Law, France]	The section on CO has been integrated with the one on Nox, remark on Chinese emissions taken into account.
2-2022	2	52	27			I'm not sure section 2.4.2.5 is needed at all [William Collins, United Kingdom of Great Britain & Northern Ireland]	The section on CO has been integrated with the one on Nox, remark on Chinese emissions taken into account.
2-2023	2	52	29	52	29	Replace "affect" with "effect" [Mihai Dima, Romania]	OK
2-2024	2	52	29	52	29	Change "affect" to "effect" [Dian Seidel, USA]	OK
2-2025	2	52	33	52	33	like → such as [Peter Burt, UK]	thank you
2-2026	2	52	33	52	33	"like biomass and fossil fuels" can probably be deleted without harm [Dian Seidel, USA]	sentence rephrased.
2-2027	2	52	33	52	35	Data for CO from AGAGE should also be quoted and differences noted (Prinn et al, JGR, 2000, updated). [Ronald Prinn, USA]	FOLLOW UP WITH PRINN
2-2028	2	52	35	52	35	If the only data available are for the short period 2006-2010, (1) does that explain why AR4 did not assess CO trends, (b) is this a long-enough period to represent trends, or (3) is inclusion of this section premature? [Dian Seidel, USA]	There are more data available now also mentioned in the section.
2-2029	2	52	37	52	37	and → et [Peter Burt, UK]	corrected
2-2030	2	52	37	52	37	should be et l (not and al). [Karen Rosenlof, United States of America]	correct
2-2031	2	52	39	52	39	The section on NO2 observations focuses on analyses of satellite data. However, there is no discussion about surface obseravations or about the uncertainties related to satellite retrieval of NO2 which are influenced by the presences of aerosols, clouds etc. [Katharine Law, France]	We are not aware of published studies discussing these uncertainties, is mentioned in text.
2-2032	2	52	39			Section 2.4.2.6 on NO2 may fit better under the ozone discussion (2.4.2.1) rather than as a short-lived greenhouse gas in its own right. [William Collins, United Kingdom of Great Britain & Northern Ireland]	We decided to combine the precursor gases of ozone in a separate section.
2-2033	2	52	43	52	34	Is it necessary to mention ESRL GMD? [Dian Seidel, USA]	yes
2-2034	2	52	49	52	49	(Richter et al., 2005) → Richter et al. (2005) [Peter Burt, UK]	Appropriate formating will be performed at the final stage of IPCC
2-2035	2	52	52	52	52	Chartography → Cartography [Peter Burt, UK]	OK
2-2036	2	52	54			small changes after 2003 in W. Europe but in the US ponounced changes after 2004. This seems important, why are the emission abatements more effective in the US? [Ruth Doherty, UK]	The strong changes in the US are related to legislation for power plants.
2-2037	2	52				Section 2.4.2.4: consistency needed with chapter 6, section 6.3.3.3 and Chapter 8, section 8.2.3.3 [Michiel van Weele, The Netherlands]	Noted
2-2038	2	53	1	53	2	(Lamsal et al., 2011) → Lamsal et al. (2011) [Peter Burt, UK]	Appropriate formating will be performed at the final stage of IPCC
2-2039	2	53	4	53	4	The paper by de Ruyter de Wildt et al. has been published in GRL (2012) in the meantime. de Ruyter de Wildt, M., H. Eskes, and [Klaas Folkert Boersma, Netherlands]	due to space limitatations the sentence has been removed.

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2-2040	2	53	4	53	4	(de Ruyter de Wildt et al., (2011) → de Ruyter de Wildt et al. (2011) [Peter Burt, UK]	due to space limitatations the sentence has been removed.
2-2041	2	53	6	53	6	Soon, a paper will be published in Scientific Reports on reductions in NO2 pollution throughout Europe in 2004-2010. The paper shows [Klaas Folkert Boersma, Netherlands]	Appropriate reference made
2-2042	2	53	13	53	23	There should be here a cross-reference to chapter 8 for trends in volcanic aerosols. [Olivier Boucher, France]	done
2-2043	2	53	15	53	23	Observations over clean high-altitude stations during calm or weak-wind conditions represent reasonably natural aerosols which can be used as base-line data for seggregating time series for antropogenic aerosols for trend analysis. In order to obtain realistic estimates of such time series, measurements need to be quantified in local / regional / hemispheric / global scales. [Panuganti China Sattilingam Devara, India]	thank you for this comment, given space limitations we have not been able to include this comment
2-2044	2	53	17	53	17	delete comma after 'seasalts' [Peter Burt, UK]	done
2-2045	2	53	21	53	22	"Long-term measurements from aerosol components and parameters measured in-situ and obtained from remote sensing will be discussed" is a very awkward statement. Instead: "Long-term in situ and remotely sensed measurements of aerosol components and parameters will be discussed" [Dale Hurst, United States of America]	The section has been completed revised.
2-2046	2	53	22	53	22	Instead of "will be discussed", this is a good chance to succinctly summary findings. [Dian Seidel, USA]	Summary is moved to the end of the section
2-2047	2	53	25	53	25	The section on aerosols focuses on a discussion about aerosol optical depth data and surface data. Whilst this is useful and fits well in the chapter, some mention should be made about the need for information about the vertical distribution of aerosols since this is important for characterising their radiative impacts. Reference can be made to papers dealing with analysis of CALIPSO data which provide information on the vertical distribution of aerosols. [Katharine Law, France]	thank you for this comment, Calipso data do not yet provide reliable trend information, Calipso is further discussed in Chapter 8.
2-2048	2	53	25	53	55	In this section, focus is on AErosol RObotic NETwork (AERONET). However, coverage of AERONET over several regions is poor. Recent years since AR4 witnessed the expansion of several other regional networks such as ARFINET covering India (Moorthy et al., 2009; Satheesh et al., 2009; Lawrence and Lelieveld, 2010), AEROCAN over Canada (O'Neill et al., 2008; Eck et al., 2010), SKYNET over Japan (Kim et al., 2005), PHOTONS covering France and part of Africa and so on. It is important to mention these and use data from these networks for trend analysis. [S K Satheesh, India]	Thank you, we will mention this in the Appendix.
2-2049	2	53	25	55	27	In section 2.4.3.1, there should be clear links to the assumptions about relatively recent aerosol optical depth made in the RCP aerosol and greenhouse gas forcing data sets. This might involve cross referral to other chapters. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	The evaluation of recent trends is partly included in chapters 9 and 11.
2-2050	2	53	25			I like the section on aerosol trends. Two general comments however: long-term trends over the ocean from satellite are uncertain (page 53, lines 32-34), yet are discussed without much caveat on page 55, lines 3-6. It would be good to discuss the trends and their uncertainty / confidence level in one single place. Maybe this could be done at the end in a summary paragraph for 22.4.3. Likewise it should be stated that surface networks are heavily biased towards some regions of the world. [Olivier Boucher, France]	we included a sentence: AERONET (AErosol RObotic NETwork) is a global sun photometer network, with largest coverage over Europe and North America. Furthermore we have substantially improved the section downplaying the role of ocean trends.
2-2051	2	53	27	53	35	Whether the data series correspond to surface-level or altitudinal or column-integrated, multi-regression statistical analyses are commonly used to delineate the long-term changes and trends. But these methods are biased to beginning and end values of the data series, so biases due to such effects need to be removed in the alternative methods such as piecewise linear trend, merging short-term data series and long-term observed or re-constructed continuous observational records of aerosol distributions. Moreover, quantification of errors due to ageing of sensors and thereby drifting of performance, periodic calibration with standard sources is needed. Further aspects such as natural variability in aerosol behavior over different environments in order to quantify their effects due to anthropogenic forcing need to be known clearly. [Panuganti China Sattilingam Devara, India]	Thank you for the comments. In this section we rely on trend analysis performed in the peer-reviewed litetratue. So we can't. We agree, there is a box 2.2 which is discussing issues and choice for is trend method in the chapter. Further we pput caveats that this add to the uncertainty of the trend.
2-2052	2	53	27	53	35	This paragraph does not provide the data source of long-term trend of long-term AOD. As I know, only NOAA AVHRR provided AOD since 1980s over oceans. However, this paragraph and executive summary indicate	AVHRR data refer to uncalibrated data. We have focussed the discussion more on MODIS and

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						these long-term trends are over land. This is inappropriate. [Kaicun Wang, China]	SEAWIFS in the revised section.
2-2053	2	53	29	53	29	What is meant by "long-term" here? [Dian Seidel, USA]	changed sentence to mention ca. 15 years.
2-2054	2	53	30	53	30	delete comma after 'Europe' [Peter Burt, UK]	OK
2-2055	2	53	32	53	32	"Some studies report negative trends" - it is not clear if the "trends" are for AOD or anthropogenic aerosol emissions [Dale Hurst, United States of America]	Due to space restrications sentence was removed.
2-2056	2	53	33	53	33	delete comma after 'record' [Peter Burt, UK]	Due to space restrications sentence was removed.
2-2057	2	53	33	53	34	Why start italicisng the statistical details now? [Peter Burt, UK]	Lay-out questions will be dealt with at the final stage of the report
2-2058	2	53	37	53	38	"which addresses aerosol amount" [Dale Hurst, United States of America]	Due to space restrications sentence was removed.
2-2059	2	53	41	53	53	This seems like too much detail on AOD. The final sentences of this paragraph (following these lines) are much more informative, even if they don't report exciting results. [Dian Seidel, USA]	We have revised this section and removed redundant text.
2-2060	2	53	41	53	55	In addition to routine measurements of aerosol spectral optical depth, there have been improvements in the algorithms to derive column-averaged size distribution from measurements of sky radiance as a function of scattering angle (Dubovik et al., 2006). However it is important to mention that these inversion products have not been comprehensively validated yet. [S K Satheesh, India]	appendix
2-2061	2	53	41	53	55	I suggest mentioning the results of a 16-year AOD climatology here. Michalsky, J., F. Denn, C. Flynn, G. Hodges, P. Kiedron, A. Koontz, J. Schlemmer, and S. E. Schwartz (2010), Climatology of aerosol optical depth in north-central Oklahoma: 1992–2008, J. Geophys. Res., 115, D07203, doi:10.1029/2009JD012197. [Beat Schmid, USA]	thank you for the reference. It is a valuable paper, but in the light of space limitiation we have in this section not used results from single sites, where network data are more instructive.
2-2062	2	53	47	53	47	Replace "Presently there are only few stations" with "A modest number of stations". [Robert Waterland, United States of America]	Due to space restrications sentence was removed.
2-2063	2	53	47	53	48	edit sentence to start with 'However'. Insert 'a' after 'only' [Peter Burt, UK]	Due to space restrications sentence was removed.
2-2064	2	53	50	53	51	It might be appropriate to reference Dubovik et al (2002) here. Full reference: Dubovik, O., B. Holben, T.F. Eck, A. Smirnov, Y.J. Kaurman, M.E. King, D. Tanre, and I. Slutsker, 2002. Variability of absorption and optical properties of key aerosol types observed in worldwide locations, J. Atmosph. Sci. 59, 590-608. [Ralph Kahn, United States of America]	Due to space restrications sentence was removed.
2-2065	2	53		56		The 2.4.3 Section of this Chapter deals with long-term changes and trends in aerosols (surface as well as atmosphere) of natural and anthropogenic origin. Besides surface-level and columnar parameters, vertical distributions of aerosols are also needed to identify the role of atmospheric layers, which is essential for proper accounting of radiative forcing exerted by aerosols. The active remote sensing systems like lidars, although they provide these distributions with super spatio-temporal resolution, their operation is mostly limited to clear-sky conditions. The passive remote sensing systems such as hyper-spectral radiometers provide columnar aerosol optical, microphysical and radiative characteristics but their operation is mostly limited to daytime and clear-sky conditions. So, developments in data retrieval schemes involving cloud-screening algorithms will improve the availability of proper data sets for the study of long-term changes and trends. In this context, besides technological improvements in instrumentation for observing more finer details of aerosols, networking-cum-intercomparison and accurate ground-truth for satellite retrievals are highly essential. [Panuganti China Sattilingam Devara, India]	We generally agree with statement include essence in the text, unfortunately we have very little space to discuss all aspect of aerosol retrieval.
2-2066	2	53				K. F. Boersma (2012), The global economic cycle and satellite-derived NO2 trends over shipping lanes, Geophys. Res. Lett., 39, [Klaas Folkert Boersma, Netherlands]	reference included
2-2067	2	53				L01802, doi:10.1029/2011GL049541. [Klaas Folkert Boersma, Netherlands]	reference included
2-2068	2	53				that European air quality has improved as a consequence of environmental policies, but that the economic	reference included

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						recession in 2009 was [Klaas Folkert Boersma, Netherlands]	
2-2069	2	53				probably as important in reducing NO2 concentrations. The ultimate conclusion is that representative concentration pathways do [Klaas Folkert Boersma, Netherlands]	reference included
2-2070	2	53				not follow simple linear trends, but instead reflect a compilation of environmental policies and economic activity. The citation is: [Klaas Folkert Boersma, Netherlands]	reference included
2-2071	2	53				Castellanos, P. & Boersma, K.F. Reductions in nitrogen oxides over urope driven by environmental policy and economic recession. [Klaas Folkert Boersma, Netherlands]	reference included
2-2072	2	53				Sci. Rep. 2, 265; DOI:10.1038/srep00265 (2012). The paper is scheduled for publication on 16 february 2012. The paper holds more [Klaas Folkert Boersma, Netherlands]	reference included
2-2073	2	53				detailed information on changes in Europe than presented by Figure 2.27, which has not been published in the peer-reviewed [Klaas Folkert Boersma, Netherlands]	reference included
2-2074	2	53				literature. I strongly advocate including the conclusions of this paper in Section 2.4.2.6. [Klaas Folkert Boersma, Netherlands]	reference included
2-2075	2	54	1	54	1	"satellite sensors have provided continuous" [Dale Hurst, United States of America]	Sentence was removed.
2-2076	2	54	1	54	19	Since satellites provide data on global scale and over sufficiently longer periods, they are very valuable in the assessment of aerosol-climate interaction programs. But in the light of discripancies between the satellite products and multi-platform real-time observations, the sensor technology and data retrieval algorithms need to be improved for obtaining reasonable matching between them. In these exercises, observational methods also need to be supported by the laboratory experiments. Accurate measurements of aerosol absorption, surface solar reflection and evaluation of weighting functions with respect to atmospheric pressure and temperature variations will improve the data quality. [Panuganti China Sattilingam Devara, India]	We agree- but it is out of scope for this section.
2-2077	2	54	1	55	27	Trends in aerosol loading are gaining increased interest due to its importance to global climate change. However, there are several concerns in using satellite data to study trends in aerosols over land due to the large uncertainties involved in the satellite retrieval of aerosol optical depth especially over land. These are attributable to the complex surface reflectance, cloud contamination and aerosol models used in the retrieval methods. Zhang et al. (2005), Kahn et al (2007), Shi et al. (2010), Levy et al. (2010) and several others have suggested that one must be wary of mistakenly interpreting noises and biases in satellite aerosol products as legitimate signals in long term trends analysis. Such studies are also inundated by calibration issues where the calibration drifts can be mistakenly interpreted as trends. Despite the increased proficiency and use of realistic models in the aerosol retrieval algorithms, several studies have shown that discrepancies still exist between retrievals of aerosol optical depth even over ocean regions (e.g., Jeong et al., 2005). In summary, retrieval of aerosol optical depth over land continues to be a challenge especially over complex terrains such as mountains. Ground-based sun photometers (preferable a network) and/or in situ measurements of aerosol mass is best suited for trend analysis [S K Satheesh, India]	We agree, and hope this is reflected in well in the revised text.
2-2078	2	54	2	54	2	"using changes in sunlight reflected to space" [Dale Hurst, United States of America]	OK
2-2079	2	54	3	54	4	"when they are validated" [Dale Hurst, United States of America]	sentence deleted
2-2080	2	54	4	58	46	Other than the 3 comments above I found the aerosol section to be relevant, accurate and complete. [Lorraine Remer, USA]	thank you
2-2081	2	54	6	54	6	Is it appropriate to even discuss this (attribution) in Ch. 2? [Dian Seidel, USA]	Thank you the sentence was removed, but similar statement providing context have been retained.
2-2082	2	54	6	54	19	Aerosol layer height is usually derived with great precision from space-borne lidar, but horizontal sampling is very poor on a global basis. The MISR acquires imagery at nine view angles between 70.5 degrees forward and backward of nadir. Stereoscopic image matching of red band data at 275-m horizontal spatial resolution can provide information on aerosol plume heights (Kahn et al., 2008). The elevation at which wildfire smoke is	We agree, but too much detail for this section

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						injected into the atmosphere has a strong influence on how the smoke is dispersed, and is a key input to aerosol transport models (Labonne et al., 2007). It is useful to include trends in aerosol layer height using MISR data. [S K Satheesh, India]	
2-2083	2	54	8	54	10	I'm thinking the natural contribution to aerosol load *globally* is considerably more than half -- maybe around 80% to 90%, actually. The result is based on modeling simulations, which are of course uncertain, but "half" seems way too low. This does not diminish the importance of anthropogenic aerosol *regionally*. [Ralph Kahn, United States of America]	Due to space restricition the sentence was removed
2-2084	2	54	8	54	10	On average about half of today's atmospheric AOD and most of the AOD variability can be linked to natural aerosol (e.g., dust, sea-salt, volcanoes). Please provide reference. [S K Satheesh, India]	Due to space restricition the sentence was removed
2-2085	2	54	10	54	10	are → is [Peter Burt, UK]	Due to space restricition the sentence was removed
2-2086	2	54	12			SkyNET -> SKYNET (correct) [Tadahiro Hayasaka, Japan]	Corrected and moved to appendix
2-2087	2	54	12			GAW needs to be defined here. It is defined later at P56, L8. [Yutaka Kondo, Japan]	for appendix
2-2088	2	54	16	54	19	Can stratospheric AOD actually be much bigger following major eruptions than the tropospheric AOD for anthropogeinc aerosols? Chin et al, Journal Atmos. Sci. 2002, says global AOD could be 0.04 for sulphate aerosol. This compares to a AOD of 0.15 following Pinatubo (IPCC WG1 2007, chap 2). [Gareth S Jones, UK]	We crossreference to Chapter 8
2-2089	2	54	21	54	21	I don't think Figure 2.28 is very useful. In any case it is unclear to me what we are supposed to learn from this figure. [Klaas Folkert Boersma, Netherlands]	Figure was removed.
2-2090	2	54	22	54	22	Why is the 2004 figure larger than the others?, do you want to highlight something in particular?. [Celeste Saulo, Argentina]	Figure was removed.
2-2091	2	54	22	54	22	(Fig. 2.28 caption) Explain AOD. [Christian-D. Schoenwiese, Germany]	Figure was removed.
2-2092	2	54	26	54	26	Again, is it appropriate to even discuss this (attribution) in Ch. 2? [Dian Seidel, USA]	Sentence removed
2-2093	2	54	26	54	30	In order to understand better the current findings of aerosol trends, improvements should take place in both directions of normalization of instrument performance from time to time, and removal of natural variability induced by solar parameters, ENSO, QBO and volcanic eruptions. [Panuganti China Sattilingam Devara, India]	We agree- unfortunately no space to discuss these issues.
2-2094	2	54	27	54	29	This statement: "Satellite retrievals over oceans (but not over land) can usually make some distinctions between AOD from larger and smaller aerosol" is true for single-view, multi-spectral data, e.g., from MODIS or AVHRR. However, much more aerosol type information can be derived from multi-angle imaging, e.g., aerosol size (three-to-five size bins) and fraction optical depth non-spherical are retrieved operationally over land and ocean from MISR data. Just two example references (there are many more): Over land: Dey, S., and L. Di Girolamo, 2010. A climatology of aerosol optical and microphysical properties over the Indian subcontinent from nine years (2000-2008) of Multi-angle Imaging SpectroRadiometer (MISR) data. J. Geophys. Res., doi:10.1029/2009JD013395. Over water: Kalashnikova, O.V., and R.A. Kahn, 2008. Mineral dust plume evolution over the Atlantic from combined MISR/MODIS aerosol retrievals. J. Geophys. Res. 113, D24204, doi:10.1029/2008JD010083. [Ralph Kahn, United States of America]	thank you, unfortunately due to space limitation we have not retained the discussion on changes in size from satellite.
2-2095	2	54	32	54	48	It is not clear what the overall message of this paragraph is. [William Collins, United Kingdom of Great Britain & Northern Ireland]	The concluding sentence now reads : Recent ground and satellite based remote sensing reported both positive (E. and Southern Asia) and negative AOD trends for regions affected by anthropogenic pollution (Europe and Eastern USA) (*medium agreement, robust evidence*). Trends in regions with strong aerosol load inter-annual variability, e.g. due to wild-fires or dust are less robust. Vast regions of the world do not display significant aerosol trends over the last decades, and consequently no global tropospheric AOD

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							trend can be detected.
2-2096	2	54	32	54	48	This paragraph provides unnecessary detail. [Dian Seidel, USA]	We have updated the whole section, removing details
2-2097	2	54	35	54	35	delete comma after 'bias' [Peter Burt, UK]	Sentence removed
2-2098	2	54	38	55	12	The Reference "Zhang and Reid, 2010", cited in the text, is not included in the List of References. [Panuganti China Sattilingam Devara, India]	Issue appeared by the use of EndNote library
2-2099	2	54	46	54	47	You might mention here that the absorbing aerosol index from TOMS is a *qualitative* measure of aerosol absorption, that depends sensitively on aerosol vertical distribution. [Ralph Kahn, United States of America]	Reference to TOMS has been removed.
2-2100	2	55	1	55	7	The aerosol section does not note any of the long-term aerosol data sets from AATSR. However, perhaps the biggest comment is on the statement that AVHRR show decreasing aerosol over the ocean. This may be due to other factors such as sampling time. See Thomas et al, Validation of the GRAPE single view aerosol retrieval for ATSR-2 and insights into the long term global AOD trend over the ocean, ACP, 2010. http://www.atmos-chem-phys.net/10/4849/2010/acp-10-4849-2010.pdf [John Remedios, United Kingdom of Great Britain & Northern Ireland]	A sentence was included.
2-2101	2	55	2	55	2	I think you mean the "open ocean" rather than the "deep ocean". [Olivier Boucher, France]	yes
2-2102	2	55	2	55	2	the 23 year data record: please specify which years are considered here. [Claudio Cassardo, Italy]	sentence deleted
2-2103	2	55	3	55	4	It is not clear in that sentence if with the hemispheric separation hemispheric differences in AOD were detected or why exactly a hemispheric separation was performed. [Birgit Hassler, USA]	will be deleted
2-2104	2	55	3			It's not clear which hemisphere this refers to. [Drew Shindell, USA]	will be deleted
2-2105	2	55	4	55	4	"a decrease of 0.02 for total AOD over oceans is evident." [Dale Hurst, United States of America]	Not sure what this comment refers to- but section has been revised.
2-2106	2	55	6	55	6	"starting from the beginning of 1990s" is very awkward. Instead: "beginning in the early 1990s" [Dale Hurst, United States of America]	sentence deleted
2-2107	2	55	9	55	10	Detection of … is possible for aerosol dedicated satellite sensors … This view is contested by more recent ... --> actually it is not a contestation but an observation, so I suggest a rephrase in: In principle, detection of … could be possible for aerosol dedicated satellite sensors … However, more recent ... [Claudio Cassardo, Italy]	sentence deleted
2-2108	2	55	9	55	10	Does "at least over oceans" refer to the detected trends, or (less likely) deployment of satellite sensors? If the former, consider moving the phrase closer to what it modifies. What view does "This view is contested…" refer to? There is no "view" put forth here. [Dian Seidel, USA]	sentence deleted
2-2109	2	55	9	55	14	It is not clear what the conclusion of this paragraph is. Are there trends or not? [William Collins, United Kingdom of Great Britain & Northern Ireland]	The overall conclusion:Recent ground and satellite based remote sensing reported both positive (E. and Southern Asia) and negative AOD trends for regions affected by anthropogenic pollution (Europe and Eastern USA) (*medium agreement, robust evidence*). Trends in regions with strong aerosol load inter-annual variability, e.g. due to wild-fires or dust are less robust. Vast regions of the world do not display significant aerosol trends over the last decades, and consequently no global tropospheric AOD trend can be detected.
2-2110	2	55	9	55	22	This can be stated more clearly. Is the conclusion that while there are no significant global (ocean?) AOD trends for 2000-2009, there are significant regional trends? Please clarify. [Norman Loeb, United States of America]	The overall conclusion:Recent ground and satellite based remote sensing reported both positive (E. and Southern Asia) and negative AOD trends for regions affected by anthropogenic pollution (Europe and Eastern USA)

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							(*medium agreement, robust evidence*). Trends in regions with strong aerosol load inter-annual variability, e.g. due to wild-fires or dust are less robust. Vast regions of the world do not display significant aerosol trends over the last decades, and consequently no global tropospheric AOD trend can be detected.
2-2111	2	55	10			Confusing sentence "This view is contested"? [Ruth Doherty, UK]	sentence deleted
2-2112	2	55	17	55	19	There is a common misunderstanding of the Kaufman, Boucher, Tanre et al. (2005) paper that coarse mode corresponds to natural aerosol and fine mode to anthropogenic. Actually that paper demonstrates that knowing fine and coarse mode AOD allows for ESTIMATION of natural and anthropogenic fraction. Note that dust is 50% fine mode. The words "often associated" should be "may allow for estimation of". Also that paper is never cited in the chapter. [Lorraine Remer, USA]	We agree, however there is no longer a discussion on size
2-2113	2	55	17	55	19	I'm fairly sure that MISR's retrieval of particle size information is as good over land as over ocean, and it does better than distinguish between fine and coarse modes. The sentence as stated now is firmly biased by MODIS and there are other satellites up there. [Lorraine Remer, USA]	We agree, however there is no longer a discussion on size
2-2114	2	55	19	55	19	check context of 'tbc' [Peter Burt, UK]	Sentence removed
2-2115	2	55	19	55	27	In recent years, there has been a substantial increase in interest in discriminating the climate influence of natural aerosol sources from its anthropogenic counterparts. However, validation programs for these advanced products have yet to be developed and initial assessments indicate some systematic errors, suggesting that the routine differentiation between natural and anthropogenic aerosols from satellite retrievals remains very challenging. This aspect may be included. [S K Satheesh, India]	We agree, however there is no longer a discussion on size
2-2116	2	55	21	55	22	It is not clear why the following sentence is here: "Using MODIS retrieval at different visible wavelengths a separation of coarse (assumed natural) and fine (assumed anthropogenic) aerosol is possible." As written, the statement is also incorrect: transported dust, natural wildfire smoke, and the tails of sea salt and dust storm partilce distributions all contain "fine-mode" particles. [There is a profound issue with some in the community taking Kaufman et al., (GRL 2005) literally -- it is an oversimplification that has created a great deal of confusion.] [Ralph Kahn, United States of America]	We agree, however there is no longer a discussion on size
2-2117	2	55	21	55	22	(assumed natural) -(assumed anthropogenic). I know of ABSOLUTELY no evidence for this assumption - in fact it is plain wrong! I can show you <1 micron salt particles that have nothing to do with humans at all. [Philip Lloyd, South Africa]	sentence deleted
2-2118	2	55	21	55	22	This statement applies to over the oceans, but not over the land. [JOHN OGREN, USA]	sentence deleted
2-2119	2	55	22	55	23	Uncertainties arise from more than assumptions of aerosol absorption and surface reflectance. Uncertainties arise from assumptions of particle properties including both absorption and scattering properties, plus surface reflectance. We need to know the phase function, as well as the absorption. Also I prefer the terms "retrieval assumptions" to "retrieval model". [Lorraine Remer, USA]	sentence deleted
2-2120	2	55	24			Figure 2.29 seems very strange that given the similarity of the upper and lower aerosol trend figures that the lower box anthropogenic trends are not significant (red boxes), while the upper total aerosol trends are. Or are the boxes the same? If so, the red boxes should be marked on both figures to clearly show statistical signficance. If not: need to comment on lack of statistical signficance of anthropogenic aerosol trends. [Bruce Wielicki, USA]	Figure 2.29 graphical quality improved, and uncertainty only discussed in qualitative terms
2-2121	2	55	25	55	27	Figure 2.9: care should be taken with the use of the word "trends" when refering to a 10-year data record. [Katharine Law, France]	There is a section in Chapter outlining the concept of trends; indeed 10 years is sometimes to short when variability exceeds trends.
2-2122	2	55	29	57	36	Central Pollution Control Board (CPCB), Government of India is executing a nation-wide programme of ambient air quality monitoring known as National Air Quality Monitoring Programme (NAMP). The network	We have used this report, but there was fairly little quantitative information, supporting the reviewers

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						consists of 342 monitoring stations covering 127 cities/towns in 26 States and 4 Union Territories of the country. A recent report by Ministry of Environment and Forests (MoEF), Government of India (State of Environment, Report, 2009; http://moef.nic.in/soer/2009/SoE%20Report_2009.pdf) have reported a decreasing trend in particulate mass contraction over several cities while no trend was observed over many other locations. More details are available in this report. [S K Satheesh, India]	statement.
2-2123	2	55	29	57	55	"PM2.5" and "PM10" refer to different size ranges of particles, and should not be use as synonyms for the particulate mass concentration in those size ranges. [JOHN OGREN, USA]	Has been improved where appropriate.
2-2124	2	55	29	58	17	studies in north america and Europe gives explanation in regional scale, so more studies from Asia, Africa, Oceans, my give another important points, I recommended that this section needs more attention by increasing results of spcific studies related to this subject. [ALI GEATH ELJADID, LIBYA]	The SOD will have include some data on India and China, however there is not many time trends available in these regions
2-2125	2	55	29			Section 2.4.3.2: PM10 and PM2.5 need to be defined [William Collins, United Kingdom of Great Britain & Northern Ireland]	We now clarify this in the text
2-2126	2	55	31	55	43	Are these trends based on measurements in urban or rural areas? How do the trends change if only rural data, which are more regionally-representative, are included in the trend analysis? [JOHN OGREN, USA]	We now clarify this in the text
2-2127	2	55	31	57	36	Convention is PM10 and PM2.5 [Peter Burt, UK]	We now clarify this in the text
2-2128	2	55	32	55	37	Here "downward trends" are quantified with negative values, which could be interpreted as an upward trend. Please be careful about adjectives and numerical signs here and elsewhere. [Dian Seidel, USA]	We have included this consistently now.
2-2129	2	55	34	55	35	"indicated a downward trend on the order of 0 to –2.5% yr–1" - I don't believe that 0% yr-1 is a "downward trend" [Dale Hurst, United States of America]	text has been changed.
2-2130	2	55	34	55	43	Is the downward AOD trend in the USA specific to the eastern part of the country, and possibly along the west coast, or the entire lower 48? A *refernce* here would be helpful here. (I'm thinking the available data relate primarily to the eastern part and maybe a few places in California.) [Ralph Kahn, United States of America]	The section has been revised.
2-2131	2	55	34	57	55	This section on surface aerosol trends is a new addition in AR5. It is currently rather long and would benefit from being made more concise with clear messages. There also needs to be a clearer link with Chapter 7 including a brief discussion of the components of PM2.5 and PM10 (which need to be defined) that are relevant to climate so not only SO4 and BC. The study by Hirdman et al. (2010) which examined Arctic EBC and SO4 trends can also be mentioned (lines 53-53, page 57). [Katharine Law, France]	We have substantially revised this section; Hirdman is now included
2-2132	2	55	34		36	PM2.5 and PM 10 hould be explained here and not in lines 55 and 57. [Jean Poitou, France]	done
2-2133	2	55	34			suddenly PM2.5 or PM10 appear... do they need an introduction? [Francois DANIS, France]	done
2-2134	2	55	34			PM2.5 should be introduced here, not in l. 55 [Uwe Stoeber, Germany]	done
2-2135	2	55	35	55	35	PM2.5 needs defining here. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	done
2-2136	2	55	36			What about the other locations? [Uwe Stoeber, Germany]	Trends were not significant; text revised.
2-2137	2	55	40	55	40	Consider changing "at best" to "at the earliest" [Dian Seidel, USA]	The section has been revised.
2-2138	2	55	45	55	57	Discriminating natural aerosols from anthropogenic origin, based on satellite-derived Angstrom coefficients, needs comaparison with in-situ measurements of size-seggregated aerosols over locations of known environments coupled with meteorology. [Panuganti China Sattilingam Devara, India]	We agree; we place less emphasis on the discriminiation of nature
2-2139	2	55	49	55	52	Relating total column to near-surface AOD requires *additional* informaiton about aerosol vertical distribution. Simply assuming the surface load represents the total column is *not* adequate for radiative forcing and most other global-scale, climate-related appliactions. Van Donkelaar et al (note typo in the reference on line 51) used the GEOS-Chem model, constrained by CALIPSO, to provide vertical distribution. And elevated layers are common not only over ocean; to take just a few examples, wildfire smoke is often injected directly into the	The section has been revised, and the sentence is no longer there.

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						free troposhpere, and within a few hundred km downwind of the sources, North African desert dust is often advected or self-lofted away from the surface. [Ralph Kahn, United States of America]	
2-2140	2	55	51	55	51	You say 'several studies' and cite one (and the e.g. should be in the bracket) [Peter Burt, UK]	Sentence is removed.
2-2141	2	55	52	55	52	Can "marine outflow conditions" be replaced with a less jargon-y term? [Dian Seidel, USA]	sentence about marine outlfow has been removed.
2-2142	2	55	52	55	55	This sentence gives the impression that elevated aerosol layers above the BL exist only or mainly in marine outflow conditions. An example of an elevated layer of biomass burning aerosol transported over a long distance is discussed by Schmid et al., 2006 and could be cited here. Schmid B., R. Ferrare, C. Flynn, R. Elleman, D. Covert, A. Strawa, E. Welton, D. Turner, H. Jonsson, J. Redemann, J. Eilers, K. Ricci, A. G. Hallar, M. Clayton, J. Michalsky, A. Smirnov, B. Holben, J. Barnard. How well do state-of-the-art techniques measuring the vertical profile of tropospheric aerosol extinction compare? J. Geophys. Res. 111, D05S07, doi:10.1029/2005JD005837, 2006. [Beat Schmid, USA]	sentence about marine outlfow has been removed.
2-2143	2	55	53	55	54	The References "Clarke and Kapustin, 2002" and "Osborne and Haywood, 2005", cited in the text, are not included in the List of References. [Panuganti China Sattilingam Devara, India]	Reference have been removed
2-2144	2	55	55	55	55	The definition of PM2.5 should appear in line 34 [Peter Burt, UK]	defintion appears at first entry
2-2145	2	55	55	55	55	um → μm [Peter Burt, UK]	corrected.
2-2146	2	55	55	55	55	"of PM2.5 (particulate matter with diameter <2.5 um)" - this definition needs to appear earlier, before any discussion of PM2.5 [Dale Hurst, United States of America]	defintion appears at first entry
2-2147	2	55	55	55	55	clarification about PM2.5 meaning (particulate matter with diameter <2.5 um) should be used before, in line 34 [Celeste Saulo, Argentina]	defintion appears at first entry
2-2148	2	55	55	55	55	The definition of PM2.5 here is too late. PM2.5 is used earlier in the section. [Dian Seidel, USA]	defintion appears at first entry
2-2149	2	55	55	55	57	PM 2.5 and PM10 definition should be moved earlier (lines 34,36) [Ruth Doherty, UK]	defintion appears at first entry
2-2150	2	55	57	55	57	um → μm [Peter Burt, UK]	corrected.
2-2151	2	55	57	55	57	same as comment #29, regarding PM10 [Celeste Saulo, Argentina]	defintion appears at first entry
2-2152	2	55	57	55	57	Rather than introduce and define the variable "d", just say "diameter" [Dian Seidel, USA]	corrected.
2-2153	2	56	6	56	6	Please explain your criteria for a "true global network" so that the reader understands why you don't consider the GAW network to be global. [JOHN OGREN, USA]	the sentences is now moved to the Appendix and reads: . The monitoring and observations of aerosols are still to a large degree uncoordinated on continental and global scale
2-2154	2	56	10	56	11	See comment under No. 1 above. It is wrong to say that the total coarse fraction is assumed to be natural and the entire fine fraction is assumed to be anthropogenic. The Kaufman et al., (2005) paper never says that. [Lorraine Remer, USA]	This discussion has been removed in the abbreviated version.
2-2155	2	56	12	56	18	I'd suggest this paragraph could easily be shortened to a single sentence. [Dian Seidel, USA]	We have removed a lot of material to the appendix
2-2156	2	56	13	56	14	To change "CAWNET and CARSNET" to "CAWNET, CARNET, and SACOL", Reference: Huang J., W. Zhang, J. Zuo, et al., 2008: An overview of the Semi-Arid Climate and Environment Research Observatory over the Loess Plateau, Advances in Atmospheric Sciences, 25(6), 1-16. [Jianping Huang, China]	Definitions appear in the APPENDIX.
2-2157	2	56	13	56	20	Full abreviation of CAWNET, CARSNET, and EMEP may be mentioned at least once in text. [Yutaka Kondo, Japan]	Definitions appear in the APPENDIX.
2-2158	2	56	36	56	36	Caption of Fig 2.30: PM2.5 units should be micrograms per m-3 (instead of ug per m-3) [Celeste Saulo, Argentina]	corrected

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2-2159	2	56	36			Replace ug m–3 => µg m–3 [Yutaka Kondo, Japan]	corrected
2-2160	2	56	38	56	41	Please add here (or in the legenda of Fig. 2.31) the reference period on which the PM10 trends have been evaluated. [Claudio Cassardo, Italy]	figure replaced, time periods mentioned
2-2161	2	56	52			Suggest to add a section on trends of other aerosol properties, such as aerosol loght scattering coefficient. Example: High alpine mountain station Jungfraujoch (3450 m asl): Increasing (not decreasing, interestingly) trend (0.33%/yr at 550 nm) for time period 1995-2006 (M. Collaud Coen, E. Weingartner, S. Nyeki, J. Cozic, S. Henning, B. Verheggen, R. Gehrig, U. Baltensperger, Long-term trend analysis of aerosol paramenters at the high-alpine site Jungfraujoch, J. Geophys. Res., 112, D13213, doi:10.1029/2006JD007995 2007). The community is working on a paper to provide more data on trends from as many stations as possible [Urs Baltensperger, Switzerland]	There is some material of this paper in the section, provided that the submission is in time.
2-2162	2	57	1	57	23	The unit of sulfate aerosols may be written as µg(S) m-3 yr-1 (for consistency with Figure 2.33) [Yutaka Kondo, Japan]	In the SOD there is only one figure with consistent units.
2-2163	2	57	1			Section 2.4.3.2.2: This section reads like a list of measurements rather than an assessment [William Collins, United Kingdom of Great Britain & Northern Ireland]	The section has been cleaned-up.
2-2164	2	57	2	57	13	Please use the correct chemical notation for sulfate. It is not SO4, it is SO42-. [JOHN OGREN, USA]	corrected.
2-2165	2	57	5	57	5	Reference required [Peter Burt, UK]	EMEP report or better.
2-2166	2	57	7	57	20	The reliance on Pozzoli regarding sulfate fluxes over the US seem to me misplaced. The US National Atmospheric Deposition Programme, through its National Trends Network, has a huge wide range of pristine sampling sites giving the precipitation chemistry. The CASTNet sites are only dry deposition sites. A few references: Civerolo, K., Hogrefe, C., Zalewsky, E., Hao, W., Sistla, G., Lynn, B., Rosenzweig, C., and Kinney, P.L. 2010. Evaluation of an 18-year CMAQ simulation: Seasonal variations and long-term temporal changes in sulfate and nitrate. Atmospheric Environment 44(31): 3745–3752, doi: 10.1016/j.atmosenv.2010.06.056. Dayan, U., and Lamb, D. 2008. Influences of atmospheric circulation on the variability of wet sulfate deposition. International Journal of Climatology 28:1315-1324.Dayan, U., and Lamb, D. 2007. Influences of atmospheric circulation on the variability of wet sulfate deposition. International Journal of Climatology, DOI: 10.1002/joc.1648. Lehmann, C.M.B., Bowersox, V.C., Larson, R.S., and Larson, S.M. 2007. Monitoring long-term trends in sulfate and ammonium in U.S.precipitation: Results from the National Atmospheric Deposition Program/National Trends Network. Water, Air, and Soil Pollution: Focus 7:59-66.Dennis, R., Haeuber, R., Blett, T., Cosby, J., Driscoll, C., Sickles, J., and Johnston, J. 2007. Sulfur and nitrogen deposition on ecosystems in the United States.EM December 2007:12-17. [Philip Lloyd, South Africa]	Thank you for the comment. The main focus of the discussion is on IMPROVE data, using the references to Hand et al, and Murphy et al. 2011.
2-2167	2	57	8	57	8	CASTNET is purely dry deposition - sulfates are definitely present in wet deposition. [Philip Lloyd, South Africa]	Thank you for the comment. The main focus of the discussion is on IMPROVE data, using the references to Hand et al, and Murphy et al. 2011.
2-2168	2	57	10	57	22	Please use the correct notation for the prefix "micro", it is not "u" but rather the Greek letter "m". [JOHN OGREN, USA]	corrected
2-2169	2	57	11	57	11	Do attribution statements belong in this chapter? [Dian Seidel, USA]	The attribution statement is to provide context on the reliability of trend numbers.
2-2170	2	57	17	57	17	Same as comment #31 [Celeste Saulo, Argentina]	We were not able to address this question, sincewe do not have numbering of this author
2-2171	2	57	19			This section on the Global Mean Radiation Budget could easily be shortened. [Dian Seidel, USA]	accepted
2-2172	2	57	20	57	20	The trends for the USA in Figure 2.33 are for non-urban sites - just to allow the comparison of these data with the European ones, the stations selected in the EU map are also non-urban, or some of them are urban? [Claudio Cassardo, Italy]	only non-urban sites are used

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2-2173	2	57	20	57	31	In both Sickles & Shadwick and Pozzoli et al., the actual measurements presented are primarily along the east coast, with a few distributed elsewhere, mainly along the west coast. The more extensive results presented by Pozzoli et al. are from the ECHAM5 model, not from measurements, and might be effectively extrapolated from very limited data. My thought is that this paragraph seems to overstate the confidence with which we know near-surface trends in sulfate aerosol concentration; this is an important subject, as you indicate, and more measurements are needed, whereas overstating here could actually discourage future efforts to fill the observational gaps. [Ralph Kahn, United States of America]	we now focus on improve data, which are more homogeneously distributed over the country.
2-2174	2	57	21	57	21	Change "on" to "of" [Dian Seidel, USA]	corrected
2-2175	2	57	22	57	22	ug → µg [Peter Burt, UK]	corrected
2-2176	2	57	24	57	24	Delete "Measurements of". Presumably it is the TSI that is better known. [Dian Seidel, USA]	we did not find these words in the text.
2-2177	2	57	29	57	30	) , (→ ; [Peter Burt, UK]	will be corrected by IPCC.
2-2178	2	57	30	57	30	SO4 → SO4 [Peter Burt, UK]	SO4 is now mentioned as SO42-
2-2179	2	57	31	57	31	SO4 → SO4 [Peter Burt, UK]	SO4 is now mentioned as SO42-
2-2180	2	57	38	57	55	You did a good job referring to "equivalent black carbon" in 2.4.3.2, but revert to the qualitative term "black carbon" in 2.4.3.2.3. Please use the "EBC" terminology in 2.4.3.2.3. [JOHN OGREN, USA]	we try to be conistent throughout the text.
2-2181	2	57	38	57	55	Indian Space Research Organisation (ISRO) is making long term measurements of aerosol optical depth and black carbon at 33 locations over India. These measurements show an increase in trend in optical depth for the last three decades and a decreasing trend in black carbon mass concentration (WMO Report, 2009). [S K Satheesh, India]	We have added some references in the main text and the appendix, to a new submitted paper, and the WMO report.
2-2182	2	57	39	57	39	"The terms black carbon (BC) and elemental carbon (EC) refer to the operational analysis methods" - Are BC and EC the same entity measured in different ways or are BC and EC actually different? This sentence does nothing to explain this and is therefore quite confusing. [Dale Hurst, United States of America]	sentence now read: refer to the analysis method: optical methods (aerosol light absorption) or filter measurements using thermal methods, respectively.
2-2183	2	57	39	57	39	What do you mean by "operational analysis methods". Is "measurement technique" a better term? [Dian Seidel, USA]	sentence now reads: refer to the analysis method: optical methods (aerosol light absorption) or filter measurements using thermal methods, respectively.
2-2184	2	57	43	57	44	This sentence doesn't make much sense - why compare the number of measurements with the number of long-term time series? [Dian Seidel, USA]	we have removed this sentence.
2-2185	2	57	46	57	46	The use of "long-term" for a series beginning in 2001 is questionable. The term should be consistently used in the whole chapter, indeed in the whole AR5. [Dian Seidel, USA]	long-term is deleted from sentence
2-2186	2	57	46	57	48	Murphy et al. (2011) showed that black carbon declined in the US over a period when IMPROVE used consistent protocols [Daniel Murphy, United States of America]	we make reference to this paper now.
2-2187	2	57	47	57	49	This statement is incorrect, as I confirmed with a phone call to Bret Shichtel (IMPROVE coordinator) on 2012-02-09. IMPROVE is measuring EC/OC with a combustion technique, they aren't reporting "BC". [JOHN OGREN, USA]	Sentence is now corrected
2-2188	2	57	49	57	49	Definition of total carbon confusing. Define black carbon and organic carbon for the reader (some black carbon is organic!) [Peter Burt, UK]	The full definition is given in the Appendix.
2-2189	2	57	50	57	50	Hand et al (2010) is missing from the References [JOHN OGREN, USA]	Problem due to EndNote; should be solved in next version
2-2190	2	57	50	57	55	For BC, you mention the lack of data in the previous paragraph, but here you report trends based on a few single-station values, which might leave the impression that they represent regional behavior -- sphecifically, large decreases in near-surface BC concentration. For the purposes of an assessment, I think the bottom line	concluding sentence; . Furthermore, there is limited evidence on downward trends in the USA and the Arctic from scarce long-term time series on light

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						is that we don't know much about these trends; they vary *enormously* from region to region, and are sometimes dominated by single, large sources, such as the Four Corners power plant in the western US, and the oil sands operation in Alberta, Canada. Emphasizing the need for more extensive monitoring, to capture the regional behavior and variability, would be helpful here, in my opinion. Note that next-generation satellite observations (multi-angle, multi-spectral, polarized imaging + High Spectral Resolution Lidar) could be especially helpful in filling some of the largest gaps. (I wrote a commentary recently for Surveys of Geophysics that concludes with a discussion of this topic in the context of constraining climate forcing. It might be of help: Kahn, R.A., 2011. Reducing the uncertainties in direct aerosol radiative forcing. Surveys in Geophysics, doi:10.1007/s10712-011-9153-z.) [Ralph Kahn, United States of America]	absorbing aerosol, while elsewhere in the world time series are lacking or not long enough. We further agree with Dr. Kahn, but could not extend the section to include these interesting statements.
2-2191	2	57	56			Where do trend analyses of archives go? Example: Kaspari, S. D., M. Schwikowski, M. Gysel, M. G. Flanner, S. Kang, S. Hou, and P. A. Mayewski (2011), Recent increase in black carbon concentrations from a Mt. Everest ice core spanning 1860–2000 AD, Geophys. Res. Lett., 38, L04703, doi:10.1029/2010GL046096. [Urs Baltensperger, Switzerland]	IPCC WG1 has not yet reached agreement on where to place the ice-core observations.
2-2192	2	57				Section 2.4: Summary for setion 2.4 may be added like in other sections [Yutaka Kondo, Japan]	a summary sentence is now included
2-2193	2	58	1	59	20	The claim that the climate can be exclusively dominated by exchanges of radiation is the major fallacy of this report. It ignores traditional meteorology, which over a period of sevral hundred years has found that the climate is controlled by air pressure, air and ocean movements, by convection and evaporation/precipitation of water, by cyclones, anticyclones, wind speed and direction, by ocean oscillations and , by land surface irregulirity. Instead we are expected to believe that the earth can be considered to be flat, that the sun shines all day and even all night, with the same intensity, that every radiant emitter has a constant temperature, and that the input and output radiation is "balanced". All these concepts are absurd and it is no wonder that they do not lead to an effective substitute ffor traditional weather forecasting, despite its limitations. [VINCENT GRAY, NEW ZEALAND]	Taken into account, climate is not exclusively dominated by radiation. However, radiation plays a cenral role in the genesis and evolution of the planetary climate. Solar radiation states the only significant energy source of the planet..The amount of absorption of solar radiation by the sun determines the effective temperature of a planet, and thermal radiation further modulates the surface temperature (compare eg. climate of Venus and Earth). The major climate zones (e.g. tropical versus polar climates) are a consequence of the differental radiation balance, as well as the strong seasonal amplitudes in the extratropics). None of these first order climate effects can be compensated by any other climate process. Nevertheless, we revise the sentence as follwos: Old: "The radiation budget of the Earth is the key energy driver of climate." New: "The radiation budget of the Earth is A key energy driver of climate."
2-2194	2	58	3	58	4	Replace "The radiation budget of the Earth is the key energy driver of climate. In the mean, radiative processes alternately warm the surface and cool the atmosphere" with "The radiation budget of the Earth is the key energy driver of the climate system. On average, radiative processes warm the surface and cool the atmosphere". [Robert Waterland, United States of America]	Accepted-text revised
2-2195	2	58	4	58	48	The word "alternately" makes it sound like the surface cooling and atmospheric warming happen at different times, which is incorrect. Suggest that you delete "alternately". [JOHN OGREN, USA]	Accepted-text revised
2-2196	2	58	6	58	7	This view is too narrow; there is no a priori reason why radiative changes are most important; regionally land use change can be a larger human interference; what matters for people is regional change. [Marcel Crok, The Netherlands]	Taken into account, the statement has been weakend according to the suggestion of reviewer comment 2-2197. Note that also land use changes, to the extent they alter the albedo of the surface, alter climate through a modification of the surface radiation balance.
2-2197	2	58	6	58	7	Replace "Anthropogenic interference with climate occurs first of all through a perturbation of the components of the Earth radiation budget." with "Anthropogenic influence on climate occurs primarily through a perturbation of various components of the Earth radiation budget." [Robert Waterland, United States of America]	Accepted-text revised

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2-2198	2	58	9	58	9	Replace "defines" with "constrains". [Robert Waterland, United States of America]	Accepted-text revised
2-2199	2	58	11	58	11	Replace "thermal outgoing" with "thermal outgoing long-wave". [Robert Waterland, United States of America]	Thermal longwave is a duplication of terms.
2-2200	2	58	19			Section 2.5.1: This section is an incomplete descritption of Figure 2.34. Sensible heat flux and solar radiation absorbed by the atmosphere are not discussed. [Uwe Stoeber, Germany]	Taken into account, sensible heat flux and absorbed solar radiation is now also discussed in this section.
2-2201	2	58	21	58	21	insert comma after 'AR4' [Peter Burt, UK]	Editorial
2-2202	2	58	21	58	21	Space → space [Peter Burt, UK]	Editorial
2-2203	2	58	21	58	32	After reading about the downward adjustment of solar irradiance, the question immediately comes to mind, what this means in [Klaas Folkert Boersma, Netherlands]	Without knowing precisely which of the many AR4 conclusions the reviewer is referring to, it should be noted that the decrease in mean solar irradiance from 1365 Wm-2 to 1361 Wm-2 does not change our understanding of how solar irradiance varies. The variation from solar minimum to maximum is still approximately 0.1%, so any conclusions about the relative influence of solar irradiance variations on climate (e.g., compared to LLGHs) remain unchanged since AR4.
2-2204	2	58	21	58	32	Consistent TSI values should be used. Chapter 1 uses 1368 W/m2, Chapter 2 seems to prefer the new value of 1361 W/m2 (Kopp, G., and J. L. Lean (2011), A new, lower value of total solar irradiance: Evidence and climate significance, Geophys. Res. Lett., 38, L01706, doi:10.1029/2010GL045777), and Chapter 8, while discussing the new values, sticks with 1365 W/m2. [Georg Feulner, Potsdam]	All chapters refer now the new value of 1361 Wm-2 consistently.
2-2205	2	58	21	58	32	Are there any larger implications of this correction in incoming radiation? [Marcus Sarofim, USA]	The correction in incoming solar radiation corresponds to 0.29% less solar radiation reaching Earth than what was assumed prior to SORCE-TIM. In a global annual mean sense, this corresponds to 1 Wm-2 (340 Wm-2 vs 341 Wm2-). If Earth's albedo and emitted thermal radiation were known perfectly, the correction in incoming solar irradiance would correspond to a 1 Wm-2 decrease in the planetary imbalance. Given that our best estimate of the planetary imbalance derived from in-situ ocean temperature profiles is 0.5 Wm-2, this would be an alarming result. However, absolute uncertainties in Earth's albedo and emitted thermal radiation contribute ~4 Wm-2 to the imbalance, so the refinement to solar irradiance, while helpful, gets lost in the larger uncertainties of the other terms in the Earth's energy budget.
2-2206	2	58	21	59	20	About global mean radiation budget. In this version, the estimation of atmospheric downward longwave radiation are substantially higher than previous publications, such as IPCC AR4. This requires surface latent heat flux and precipitation about 15% higher than previous estimations, including GPCC (observation of precipitation). To claim this, the authors should provide more solid support from observations. I question the method used here: the estimation of atmospheric downward longwave radiation are based on model calculation, not observation. Another problem of this section, it cited two many unpublished studies. [Kaicun Wang, China]	Evidence from the higher downward longwave radiaton than used in previous IPCC reports comes consitently from 2 independent sources: 1) new estimates near 345 Wm-2 based on active space-born measurements which newly incorporate radar/lidar-derived cloud profiles and associated cloud base heights, and which are critical for an accurate determination of the downward thermal radiation (Kato et al., 2011, JGR, Stephens et al. J. Climate 2012) 2) compared to available direct downward longwave radiation observations from the BSRN and GEBA database, climate models and reanaylses tend to

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							systematically underestimate the longwave downward observations (Wild et al. 1995, 2001 J. Climate, Wild 2008 Tellus). Specifically, the IPCC AR4 models, with a global multimodel mean value mean value of 338 Wm-2, underestimate the direct observed fluxes by 6 Wm-2 on average, suggesting a most realistic value exceeding 340 Wm-2 (Wild 2008 Tellus). The lower value used in the previous IPCC reports is not a direct estimate, but determined indirectly as a residual term of the energy balance, and has no direct observational support. It may comprise uncertainties inherent in in the other surface energy balance components.
2-2207	2	58	22	58	22	Replace "born" with "borne". [Robert Waterland, United States of America]	Editorial
2-2208	2	58	24	58	24	change "are in operation" to "have been in operation" [Karen Rosenlof, United States of America]	Text revised according to 2-2209
2-2209	2	58	24	58	24	Replace "which are in operation since" with "which began operation near". [Robert Waterland, United States of America]	Accepted-text revised
2-2210	2	58	26	58	28	What is being compared here? Are you comparing the valuemeasured by SORCE/TIM in 2008 with the value that sensor measured during earlier parts of the solar cycle, or are you comparing the SORCE/TIM value in the 2008 solar minimum with the value during earlier solar minima measured with different sensors? [JOHN OGREN, USA]	We are comparing 2008 values for both SORCE-TIM and from missions launched prior to SORCE-TIM that were still in operation in 2008. To clarify this, we've reworded the sentence as follows: "During the 2008 solar minimum, SORCE/TIM observed a solar irradiance of 1360.8 ± 0.5 W m–2 compared to 1365.5 ± 1.3 W m-2 for instruments launched prior to SORCE still operating in 2008."
2-2211	2	58	26	58	32	I have never gotten the terms for solar radiation straight, so perhaps this material is all in order. But it seems confusing that solar radiance and irradiance have the same units, and one is four times the other, and one varies with solar cycle but the other doesn't. Is there a steradian unit missing somewhere? In the last sentence, please specify what time period the average is for (does it span a full solar cycle?), how the revised estimate compares with earlier estimates, and what importance, if any, is there for this revision. [Dian Seidel, USA]	The 1360.8 Wm-2 value corresponds to the amount of incoming solar electromagnetic radiation per unit area that would be incident on a plane perpendicular to the sun's rays at a distance of 1 astronomical unit (AU), or approx. the mean distance between the Sun and the Earth. This quantity is often referred to as the solar "constant". The amount of solar radiation intercepted by the Earth is determined by Earth's cross-sectional area (pi x Re^2, where Re=radius of earth). This energy is distributed over the entire surface area of the Earth (4 x pi x Re^2) as it rotates. The average incoming solar irradiance is therefore 1360.8/4 Wm-2 or 340 Wm-2. Solar irradiance over a solar cycle varies by only 0.1%. This corresponds to only 0.3 Wm-2 in the average incoming solar irradiance. Therefore, the 340 Wm-2 is a rough approximation of the solar irradiance at any given time. The new value is already compared with earlier estimates (1365 Wm-2 mentioned in the text).
2-2212	2	58	32			http://www.bom.gov.au/climate/mjo/graphics/region.ts.dateline.gif shows that the average value of OLR since the beginning of 2008 is on an average significantly above 239 W/m2. [François GERVAIS, France]	Line 32 refers to a total solar irradiance of 340 Wm-2. It says nothing about OLR. Nevertheless, the 239 Wm-2 value discussed later in the text (lines 43-44) corresponds to a global mean OLR value over a 10-year period (2001-2010). The webpage listed is a mean OLR for 7.5S-7.5N and 170E-170W. The two quantities are not directly comparable because OLR

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							varies considerably with location.
2-2213	2	58	38	59	6	CERES data at TOA do not balance to better than 6 W m-2. Their imbalance at TOA was arbitrary and initially based on Trenberth et al 2009. The Kato et al paper has the downwelling flux at 347 W m-2 and Trenberth and Fasullo (2011) conclude that this is not viable. Kato et al do only a radiation balance, not a full energy balance. They end up with an extra 20 W m-2 at the surface to dispose of vs Trenberth et al 2009, and that is not viable in SH and LH. It strongly suggests a major bias in the modeling of downwelling radiation (not this is NOT an observed quantity). Such modeling is similar in many GCMs and there is a real possibility that all GCMs are wrong. [Note this could relate to things like two stream approximations (lack of 3D clouds) but mostly probably reflects the input variables of cloud, moisture andtemperature and aerosols. Using Cloudsat may give better depictions but problems are obvious.] The discussion here is very biased towrd CERES which is likely seriously astray. The assesment here should discuss this. [Kevin Trenberth, USA]	The current version of CERES EBAF uses a constraint based upon Argo in-situ ocean measurements (see Loeb et al 2012 Nature Geosciences reference for details) (Note: Loeb et al 2011 is now Loeb et al 2012). The Kato et al downward LW flux of 347 Wm-2 is based upon the latest observations from CERES, MODIS, CALIPSO and Cloudsat. The 1-sigma range given by Kato et al for downward LW is 340 - 354 Wm-2. The Trenberth et al 2009 value of 333 Wm-2 falls outside the lower bound by 7 Wm-2. Trenberth et al do not state what the confidence interval is in their estimate. They determine surface LW as a residual of all other terms in the global surface energy budget. As a result, there is no way to compare with actual surface radiation measurements. Studies that take into account the information contained in the direct surface observations support a higher downward LW radiation similar to Kato et al. (2011), exceeding 340 Wm-2. (Wild et al. 1995 J.Climate, Wild et al. 1998 Clim. Dyn., Wild et al. 2001 J. Climate). Wild et al. (2008) showed that climate models, with a multimodel mean longwave downward radiation of 338 Wm-2, are low by 6 Wm-2 on average compared to direct surface observations. On the other hand, the same studies also suggest that climate models tend to overestimate the shortwave radiation at the Earth surface, suggesting a best estimate for the absorbed surface radiation near 160 Wm-2, similar to Trenberth et al. (2009, 2011). The discrepancy in the surface net radiation between the best estimates inferred from direct observations and Trenberth et al. 2009 is therefore smaller than suggested by the reviewer, and within the uncertaintes of the non-radiative energy balance components (Trenberth et al. 2011 acknowledge that the latent heat flux can be as high as 85 Wm-2). We expanded the discusson in Section 2.5.1 to address these issues raised by the reviewer withhin the possiblities of the severe space constraints. The claim that the discussion is biased toward CERES is unfair, as the surface fluxes in the current standard CERES data products are based upon passive retrievals only, not Cloudsat and CALIPSO.
2-2214	2	58	39	58	50	It looks to me like the residual of 0.6 W/m2 is well within the uncertainties of the individual incoming/outgoing fluxes, not to mention the uncertainty of their differences. If my interretation is correct, then you should explicitly state that the satellite data provide no statistically significant evidence that there is an inbalance in Earth's TOA radiation budget. [JOHN OGREN, USA]	Actually, the 0.6 Wm-2 is based upon both satellite and Argo in-situ measurements. The paper by Loeb et al (2012) provides a detailed error analysis. According to that paper, there is an imbalance of 0.5 +/- 0.43 Wm-2 for 2001-2010 (the 0.43 Wm-2 uncertainty is at the 90% confidence level). To clarify this, we've modified line 42 on p. 2-58 to the following: "...uncertainty to be consistent with independent

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							estimates of the global heating rate based upon in-situ ocean observations (Loeb et al., 2012)."
2-2215	2	58	44	58	44	Replace "considering" with "with". [Robert Waterland, United States of America]	Editorial, but not acepted, this would slightly change the meaning of sentence.
2-2216	2	58	52	58	55	These two sentences say essentially the same thing. [Dian Seidel, USA]	Taken into account. Text has been shortened and revised as follows: "The components of the radiation budget at the surface are generally afflicted with larger uncertainties than their counterparts at the TOA, as they cannot be directly measured by passive satellite sensors."
2-2217	2	58				terms of interpretation of the conclusions of AR4. I think it should be made clear to what extent these conclusions could be [Klaas Folkert Boersma, Netherlands]	Reviewer comment erroneosly cut in 3 pieces. See response given in 2-2203
2-2218	2	58				compromised in view of the better knowledge we now have about solar irradiance. [Klaas Folkert Boersma, Netherlands]	Reviewer comment erroneosly cut in 3 pieces. See response given in 2-2203
2-2219	2	58				Section 2.5 needs major revisions. Figure 2.34 is totally unacceptable and the material assessed is woefully incomplete. The only full energy flow diagram published is Trenberth et al 2009 and modified slightly in Trenberth and Fasullo (2011); Trenberth, K. E. and J. T. Fasullo, 2011: Tracking Earth's energy: From El Niño to global warming. Surveys in Geophysics, Special Issue, doi: 10.1007/s10712-011-9150-2. (available from my web site.) The Loeb et al (2009) paper was rejected as I understand it. All the issues are addressed by Trenberth and Fasullo (2011). The change in ocean heat content from Trenberth, K. E., 2010: The ocean is warming, isn't it? Nature, 465, 304. and Lyman, J. M. et al. Nature 465, 334–337 (2010) is 0.63 W m-2 for the top 700 m alone and so adding in the region below 700 m makes 0.9 W m-2 for the imbalance much more plausible than 0.6 (p 58 line 44). GPCP has global rpecipitation as 76 W m-2 and the 88 in Fig 2.34 has no basis whatsoever and is deemed implausible by all the precip guys in GEWEX and GPCP. [Kevin Trenberth, USA]	The energy budget diagram (Fig. 2.34) provides the components of the TOA, surface and within-atmosphere energy budget that are routinely available at regional, zonal and global spatial scales and at daily, monthly and annual time scales in data products distributed to the user community by various organizations (e.g., GEWEX, NASA, NOAA, ESA, etc.). In addition, the information contained in the worldwide direct surface radiation observations (from BSRN and GEBA/WRDC) have been incorporated in the surface estimates (see also reply to 2-2213). Because there are no regional or zonal values at daily or even monthly time-scales, many of the numbers making up the global means in the earlier estimates presented in the IPCC reports cannot be independently verified with direct surface measurements nor can they be compared with other data products produced by GEWEX, NASA, NOAA, etc. Section 2.5.1 has been expanded within the possibilities of space limiatations, also in response to Review comment 2-2200. The "Loeb et al (2009)" paper was never rejected. It appeared in J. Climate. The Loeb et al. (2011) paper was published in Nature Geosciences in 2012. That paper uses the most recent Argo data to determine the planetary imbalance. The 0.9 Wm-2 value is based upon a climate model according to Trenberth et al (2009). As for the precipitation rate, Trenberth et al. 2011 acknowledge that the latent heat flux can be as high as 85 Wm-2, but see also response given under 2-2222.
2-2220	2	59	1	59	1	Should "Cloudsat…properties" be in this reference list? [Karen Rosenlof, United States of America]	Accepted-text revised (technical endnote error in reference list)
2-2221	2	59	1	59	2	..properties; L'Ecuyer..' → '..properties (L'Ecuyer..' [Peter Burt, UK]	Accepted-text revised

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2-2222	2	59	15		20	There should be a discussion of the observed value of 76 W m-2 from GPCP and the adjustments by Trenberth et al 2009 to give 80 (increase of 5% over oceans). GPCP v 3 is being produced and will come out in 2012. Note as chair of the GEWEX SSG, I am well informed about the GPCP views on this topic. [Kevin Trenberth, USA]	The latent heat flux is the energy equivalent of evaporation, which equals precipitation in the long term average global mean. The remote sensing methods widely used to estimate precipitation, especially over the vast oceans, have documented biases that imply that the amount of precipitation is underestimated. New global precipitation information from the CloudSat radar suggest that precipitation has been underestimated by approximately 10% over tropical ocean regions and by even larger fractions over mid-latitude oceans (Berg et al. 2010 J. Appl. Met and Climatol., 49, 535-543; Ellis et al. 2009 Geophys. Res.Lett. doi:10.1029/2008GL036728; Haynes et al. 2009 J.Geophys. Res. doi:10.1029/2008JD009973, Petty 1997: J. Geophys. Res. 102, 1757-1777). In addition, the total contribution from snowfall to global precipitation has so far been neglected in pervious global mean latent heat flux estimates (Stephens et al. 2012, Nature Geoscience). The above suggests that, while acknowledging the considerable uncertainties still inherent in these estimates, an increase in the latent heat flux on the order of 10% compared to estimates published in previous IPCC reports is plausible and consistent with the best estimates from the radiative components. Trenberth et al. 2011 also acknowledge that the latent heat flux can be as high as 85 Wm-2. The value of 85 Wm-2for the global mean latent heat flux has been adopted in the related Figure, since this value fits to radiation budget estimates constrained by direct surface observations, and is considered upper limit of current uncertainties in precipitation retrieval by Trenberth and Fasullo (2012) as well as towards the low end of the uncertainty range given by Stephens et al (in press), and is thus an acceptable value from diffent point of views..
2-2223	2	59	24	59	24	The reference to section 8.2.1 should be to 8.3.1 [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted-text revised
2-2224	2	59	24	59	29	The main point of this material is unclear. Why is this set of results from AR4 highlighted? [Dian Seidel, USA]	We highlight the AR4 results as background to the follow-up study by Andronoval et al (2009), which is new.
2-2225	2	59	25	59	45	There are lots of numbers in this paragraph. What is the overall assessment of the changes in the TOA radiation budget? [William Collins, United Kingdom of Great Britain & Northern Ireland]	As noted in the text, tropical net radiation increased by 2 Wm-2 between 1985 and 2005. The change in net radiation is associated with a 3 Wm-2 decrease in reflected solar radiation and a 1 Wm-2 increase in thermal radiation. In contrast, if we rely on observations of OLR inferred from instruments with limited spectral coverage (ISCCP-FD, HIRS), the inferred change in net radiation between 1985 and 2005 reaches ~6 Wm-2, or 3 times larger than what is obtained from ERBS/CERES, which are based upon broadband measurements. We also note that changes

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							to the operational channels used for HIRS cause unphysical jumps in the HIRS record.
2-2226	2	59	31	59	33	This trend can not be seen in Figure 2.35. [Uwe Stoeber, Germany]	The reference to Fig. 2.35 has been deleted.
2-2227	2	59	38	59	41	It seems something missing in the sentence. [Claudio Cassardo, Italy]	Accepted-text revised
2-2228	2	59	40	59	40	(HIRS) Lee et al. (2204;2007) → (HIRS) (Lee et al., 2004;2007) [Peter Burt, UK]	Accepted-text revised
2-2229	2	59	56	59	56	21th → 21st [Peter Burt, UK]	Accepted-text revised
2-2230	2	59				section 2.5.2. This section contains many errors that must be corrected. Firstly, as discussed in AR4 there is no clear evidence that the jump in the ERBS record that occurred at time of battery failure in 1992 (see Trenberth, K. E., 2002: Changes in tropical clouds and radiation: Are they real? Science, 296, (21 June 2002) 2095a.) is real. It is a jump at time of 3 month hiatis when the whole space craft cooled off and it was not operated the same way affecting heating of instruments etc), not a trend. There are many reasons, not the least of which is modeling, to suggest that it is spurious.lines 27-30 are incorrect. Fig 2.35 is not consistent with either the CERES record or the OHC record. This section should be based on Trenberth and Fasullo 2010, 2011. Trenberth, K. E., and J. T. Fasullo, 2010: Tracking Earth's energy. Science, 328, 316-317. Also Trenberth, K. E., 2009: An imperative for climate change planning: tracking Earth's global energy. Current Opinion in Environmental Sustainability, 1, 19-27, doi:10.1016/j.cosust.2009.06.001. Please see Trenberth and Fasullo (2011) for a full discussion of this issue. See also Meehl, G. A., J. M. Arblaster, J. T. Fasullo, A. Hu, and K. E. Trenberth, 2011: Model-based evidence of deep-ocean heat uptake during surface-temperature hiatus periods. Nature Climate Change, 1, 360-364, doi: 10.1038/NCLIMATE1229. for a full discussion of this issue from the OHC side. Since the publication of (Trenberth and Fasullo (2010) on the missing energy, the CERES data have been revised to make 20% of the problem go away, and the OHC data are better as well, but there is a major issue remaining in 2008-2009 wrt missing data because the OHC does not increase and the CERES data suggest 1 W m-2 more radiation. This was associated with La Nina and Loeb et al (2011) have a paper in press in the same issue as Trenberth and Fasullo 2011 that detail this. Fig 2.35 is unacceptable. It mixes up random and systematic errors. Systematic errors allow changes to detected, such as 2008-09 vs 2001-07. [Kevin Trenberth, USA]	As noted by the reviewer, concerns about the ERBS record were raised in both Trenberth et al (2002) and AR4. However, in the same issue of Science, Wielicki et al (2002) responded to the concerns about a calibration shift during the 4 month hiatus associated with a spacecraft battery system anomaly. Total channel offset changes after instrument operations were resumed were expected based upon the physics of active-cavity instruments and past experience. The validity of the changes was verified by observing deep space four times between 1984 and 1999. In addition, LW fluxes derived from AVHRR, HIRS and ERBS before and after the period in question were consistent within 0.5 Wm-2. Wielicki et al conclude that there is no evidence to support the claim that a change in ERBS calibration after the hiatus explains the observed decadal changes. Furthermore, the following related sentence addresses this issue in AR4: "However, careful inspection of the sensor calibration revealed no known issues that can explain the decadal shift in the fluxes despite corrections to the ERBS time series relating to diurnal aliasing and satellite altitude changes (Wielicki et al., 2002b; Wong et al., 2006)." Therefore, without further evidence to the contrary, there is no basis to claim the results summarized in lines 27-30 are incorrect. Fig. 2.35 is based upon the CERES record and the recent Argo OHC record. The Trenberth et al (2010) paper stitched together 3 different versions of CERES data products (CERES ERBE-Like, EBAF, and FLASHFLUX) that use different algorithms and calibration approaches, and considered only one OHC analysis (from Levitus), which was revised less than a year later. The Trenberth and Fasullo (2010) paper provides no discussion of the uncertainties in either their "stitched" CERES record nor the OHC data. Ignoring uncertainties in the latter is especially problematic, as there was a change in instrumentation from XBT to Argo right at the time when the alleged decline in ocean heating rate occurred. While Trenberth et al (2011) show the impact of using a consistent CERES record throughout the entire period, they gloss over uncertainties in the OHC data. For example, there is no recognition that a shift from

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							XBT to Argo increases uncertainty in the OHC record, nor is there any acknowledgement that the reference they cite (Lyman et al, 2010) clearly states that the decline in ocean heating rate after 2004 is not statistically robust. According to Lyman et al (2010): "These uncertainties are large enough that interannual variations, such as the 2003–2008 flattening, are statistically meaningless." The Trenberth papers choose to ignore these warnings and instead go on to argue there is "missing energy" in the system. The "major issue" mentioned by the reviewer about the 2008-2009 difference in ocean heating rate and CERES net flux is addressed by Loeb et al (2012): simply put, it falls within the uncertainty of the observations. The argument about random vs systematic errors does not hold up: Figs.1a-b in Loeb et al (2012) show the level of uncertainty in ocean heating rate over short timescales. If we compared heating rates for 2008-09 vs 2001-07, the uncertainty in ocean heating rate alone would exceed 1 Wm-2.
2-2231	2	60	2	60	2	Replace "correlated, since oceans have a much larger heat capacity compared to land and the atmosphere and therefore" with "correlated. Oceans cover most of the Earth's surface and have a much larger heat capacity than the land and the atmosphere and therefore". [Robert Waterland, United States of America]	We decided to leave the original formulation in, as it is more concise and contains the same information.
2-2232	2	60	5	60	5	delete 'from one another' (tautology) [Peter Burt, UK]	Accepted-text revised
2-2233	2	60	5	60	6	Cite the recent Loeb et al paper here? Observed changes in top-of-the-atmosphere radiation and upper-ocean heating consistent within uncertainty. Nature Geoscience (2012)doi:10.1038/ngeo1375. [Marcus Sarofim, USA]	The Loeb et al. (2012) paper is already cited in this paragraph (page 60 line 7 in the original draft). (On lines 60-5 to 60-6, we summarize the findings of Trenberth et al (2010), followed on line 60-7 by the recent study by Loeb et al mentioned by the reviewer.)
2-2234	2	60	5	60	7	I do not see that trend in Figure 2.35. [Uwe Stoeber, Germany]	The reference to Fig. 2.35 has been deleted.
2-2235	2	60	5	60	8	According to Lyman et al, Nature, 2010, all the different ocean heat content series are flattening out during the period after 2005 or so. A reference to Chapter 3 would be useful. [Peter Guttorp, USA]	A reference to chapter 3 has been added. We note that Lyman et al (2010) also clearly state that the 2003-2008 flattening is statistically meaningless.
2-2236	2	60	7	60	7	The estimate of Loeb is much higher than eg R. S. Knox, David H. Douglass 2010: Recent energy balance of Earth International Journal of Geosciences, 2010, vol. 1, no. 3. Regardless of that, Loeb concludes: "We combine satellite data with ocean measurements to depths of 1,800 m, and show that between January 2001 and December 2010, Earth has been steadily accumulating energy at a rate of 0. 50 +/ 0.43 Watts per meter squared (uncertainties at the 90% confidence level). We conclude that energy storage is continuing to increase in the sub-surface ocean." This value of 0.50 W/m2 is still lower than the GISS model expects. [Marcel Crok, The Netherlands]	Comparisons between CERES and several climate models (including GISS) are shown in the Loeb et al (2012) supplementary material.
2-2237	2	60	18	60	19	Replace "Changes in radiative fluxes at the surface can be traced further back in time than the satellite-based TOA fluxes, however not on a global scale but only at selected" with "Local changes in radiative fluxes at the surface can be traced further back in time than the satellite-based TOA fluxes, although only at selected". [Robert Waterland, United States of America]	Taken into account-text revised
2-2238	2	60	18	60	26	This paragraph could be eliminated or shortened. [Dian Seidel, USA]	Taken into account. We shortened the paragraph.
2-2239	2	60	28	60	28	I think you meant to delete “the Earth surface” when you introduced the abbreviation “SSR” but this raises the point that changing albedo of the Earth surface could change SSR, directly and via reflection off clouds; so	Accepted-text revised

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						surface albedo could be inserted after “such as”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	
2-2240	2	60	28	60	28	Replace "Various processes have the potential to alter SSR the Earth surface, such as changes" with "Various processes have the potential to alter SSR at the Earth surface: these include changes". [Robert Waterland, United States of America]	We preferred the original formulation
2-2241	2	60	28			"Various processes have the potential to alter SSR the Earth surface" : "at" is missing after "SSR" [Jean Poitou, France]	Accepted-text revised
2-2242	2	60	40	60	40	The numbers cited appear to be upper limits in Norris and Wild (2007) so say “dimming from 1971 until the mid-1980s of up to 3.1 W m-2 per decade and subsequent brightening of up to 1.4 W m-2 per decade” [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account . The values taken from Norris and Wild (2007) are based on the least square trends (their table 1), while also median slopes are given. We revidsed the text to include both trend estimates of the pan European timeseries.
2-2243	2	60	44	60	44	delete 'seasons' (tautology) [Peter Burt, UK]	Accepted-text revised
2-2244	2	60	51	60	51	According to Che et al. (2005), global radiation in China decreased steadily by about −9.1 W/m2 per decade from 1965 to 1988. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Che et al. (12005) mention a trend of 0.78 W/m2 per year during 1961 to 1990 and a trend of 9.1 W/m2 per decade from 1965 to 1988. We therefore adjusted the number given in the text accordingly.
2-2245	2	60				section 2.5.3.1 My understanding is that there are issues of spatial coverage and instrument calibration that are not adequately dealt with here. Instrument heating affects results and has likely amplified dimming and brightening. This is being documented and may not be published in time for AR5 but caution is called for. Why isn't Schwartz, R.D., 2005: Global dimming: clear sky atmospheric transmission from astronomical extinction measurements. J.Geophys. Res., 110, D14210, doi:10.1029/2005JD005882 included? This section does not do a balanced assessment. [Kevin Trenberth, USA]	Taken into account. There is indeed still considerable room for studies on thorough data quality control and homogeneization as pointed out in Wild (2012, BAMS). The sentence that already referred to this issue in the FOD (p.2- 60: "However, the quality of these historic measurements is variable and not always well established"), has therefore been expanded in the revised verson. Emerging studes do not support an amplification of the brightening due to measurement artifacts. The study the reviewer refers to (Wang et al., submitted), in contrast suggests that brightening might be underestimated in studies based on conventional pyranometer records compared to studies based on separate measurements of the diffuse and direct components. A more extended paragraph has been added, and also the Schwartz study is now referenced.
2-2246	2	61	18	61	19	Satellites don't infer anything, people infer things from satellite observations. [Dian Seidel, USA]	Accepted-text revised as follows:"Since satellites do not directly measure the surface fluxes, they have to be infered from measurable top-of-atmosphere signals"
2-2247	2	61	25	61	25	Change "Knowledge on" to "Knowledge of" [Dian Seidel, USA]	Accepted-text revised
2-2248	2	61	31	61	44	This section on surface solar radiation (SSR) trends claims consistency with diurnal temperature range (DTR) trends, but the section of chapter 2 that discusses DTR trends concludes that they are poorly understood due to diurnally varying biases in the data sets. If so, this section should not quote consistency with DTR trends as strong evidence for SSR trends. [Bruce Wielicki, USA]	Latest work with newly homogenized DTR records show over global land surfaces the very same change in the DTR trend in the 1980s as highlighted in the Wild et al. (2007, GRL) study cited here, with a decline before the 1980s and a levelling off thereafter (Peter Thorne personal communication). This trend reversal is consistently seen in dfferent datasets and in analyses performed by different groups, and

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							therefore does not put into questions the findings reported here. A strong trend reversial in DTR was also found over Europe in these newly homegeneized datasets, confirming earlier work cited here (Makowski et al. 2009). Therefore, also the quality controlled and homogeneized DTR data are in line with the changes in surface solar radiation reported in this section.
2-2249	2	61	34	61	36	Should mention the accuracy of DTR variations as mentioned in 2.2.1.3 ("medium-to-low confidence") - which will have a bearing on possible consistency with dimming etc... [Gareth S Jones, UK]	See response to 2-2248
2-2250	2	61	40	61	40	Makowski et al. (2009) don't examine the 1930s-1940s. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted-text revised. we removed the reference to Makowski et al. in this context.
2-2251	2	61	52	62	15	Your model does not allow you to know that much of the absorbed energy from solar radiation in the daytime is removed by convection and radiated at higher levels in the atmosphere. The temperature at the surface during the daytime and its variabilty is thus unknown. You are also not allowed to know that everything is different at night when there is no radfiation from the sun and the radiation from the earth declines, except the atmosphere can return part of the ordinary and latent heat it absorbed during the day [VINCENT GRAY, NEW ZEALAND]	It is not clear how the reviewer's comments relate to the material presented in the section under consideration.
2-2252	2	62	6	62	6	The correct citation for this statement is Philipona et al. (2004) [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. We kept Philipona et al. (2004), but removed Philipona et al. (2009) as we unserstand the suggestion of the reviewer.
2-2253	2	62	6	62	8	The surface radiative forcing fluxes from CFC11,CFC12 and CFC22 have been measured at 0.11,0.24 and 0.012 W/m2 with an FTS at 45 N . Since these gases were non existant in 1750, the presence of these fluxes in the present represent an independent proof that man has altered the radiation balance of the atmosphere and increased the greenhouse effect. W.F.J. Evans and E. Puckrin, Observation of the Atmospheric Thermal Emission Spectrum of Dichlorodifluoromethane (CCl2F2), Can. J. Applied Spec., 39, pp 85-90, (1994).W.F.J. Evans, and E. Puckrin, An Observation of the Atmospheric Thermal Emission Spectrum of Trichlorofluoromethane (CFC-11), Geophys. Res. Lett., 21, pp 2,381-2,384, (1995). W.F.J. Evans and E. Puckrin, A Measurement of the Greenhouse Radiation Associated with CCl4, Geophys. Res. Lett., 23, pp 1,769-1,772, (1996). E. Puckrin, W.F.J. Evans, Jiangnan Li and H Lavoie, Comparison of Clear-Sky Greenhouse Fluxes Simulated With Radiative Transfer Models, Can. J Remote Sensing, 30 pp 903-912, 2004. These values are consistant with the NOAA AnnualGreenhouse Gas Index on the GMDL website.The index is computed with IPCC formulae from annual measurements of GHG mixing ratios. [Wayne Evans, USA]	Taken into account. A sentence has been added "A contribution from anthropogenic chlorofluorocarbons (CFCs) to the downward thermal radiation has been documented in spectral atmospheric radiation measurements by Evans and Puckrin (1995)."
2-2254	2	62	9	62	9	This is an example where "surface temperature" actually does mean the temperature of the surface, not the air. While it is obvious here due to the context, there could be cause for confusion elsewhere. [George Kiladis, USA]	Noted
2-2255	2	62	9	62	10	This final sentence of the paragraph doesn't add much and is disconnected from the rest of the paragraph. It can be eliminated. [Dian Seidel, USA]	Acccepted, this sentence is a theoretical statement and does not provide observational evidence, and therefore does not fit well into the chapter and has been removed.
2-2256	2	62	18	62	43	In China, a decreasing trend of SW radiation was observed for the period from 1960s to early 1990s (Shi et al., 2008). On the other hand, the decreasing trend of cloud cover was observed for 1951-1994 (Kaiser, 1998) and the decreasing trend of sunshine duration was also observed for 1954-1998 (Kaiser, 2002). These	Noted. An interesting point but due to space constrain we cannot cover this in detail. Also, in this chapter emphasis is on studies documenting observed

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						inconsistent trends may be understood as the aerosol direct effect. The sunshine duration is not counted if the direct solar radiation is less than 120W/m2 perpendicular to the detector even under the clear sky condition. Absorbing aerosols such as black carbon in the sub-cloud layer give rise to the direct effect even under the overcast sky condition (Hayasaka et al., 2006). Therefore, aerosols direct effect can be larger than the effct of cloud cover decrease. (Refrences) Hayasaka et al., Geophys. Res. Lett. 33, L06802, doi:10.1029/2005GL025093, 2006. Kaiser, D. P., Geophys. Res. Lett., 25(19), 3599-3612, 1998. Kaiser, D. P., Geophys. Res. Lett., 29(21), 2042, doi:10.1029/2002GL016057, 2002. Shi, G.-Y. et al., J. Appl. Meteor. Climatology, 47, 1006-1016, 2008. [Tadahiro Hayasaka, Japan]	temporal changes in radiation and radlation related quantitites, rather than on process studies which are more dealt with in Chapters 7 and 8 .
2-2257	2	62	20	62	43	This is supposed to be an assessment report, so it would be very appropriate ti include in 2.5.3.3. a quantitative assessment of the contributions of aerosols, humidity and clouds to changes in SSR. [JOHN OGREN, USA]	Very little studies exist on the quantification of these contributions from an pure observational point of view. Modelling results and process studies are not covered in this chapter, but should be referred to in chapters 7 and 8.
2-2258	2	62	32	62	32	"Aerosols can directly attenuate SSR by scattering and absorbing solar radiation (direct effect)…" Please include Jacobson, M. Z., Studying the effects of aerosols on vertical photolysis rate coefficient and temperature profiles over an urban airshed, J. Geophys. Res., 103, 10,593-10,604, 1998 as it discusses this issue in depth and shows the effects of aerosols on surface solar and thermal-IR radiation changes in an urban region and how surface solar changes feed back to temperatures. [Mark Z. Jacobson, U.S.A.]	This is modelling study and should be refered to in chapter 7 or 8. See also response to 2-2257
2-2259	2	62	32	62	34	Terminology for the aerosol indirect effects should be aligned to chapter 7 and the glossary. [Olivier Boucher, France]	Accepted, we replaced "first and second indirect effects" by the more general expression "aerosol indirect effects"
2-2260	2	62	34	62	34	The citations given are inappropriate, as there are earlier works that define and explain these terms. But these terms were also defined and explained in AR4 (and earlier), so perhaps a citation isn't even needed here. [JOHN OGREN, USA]	Accepted- we are aware that a comprehensive literature exists on the aerosol direct and indirect effects which cannnot be covered here. We therefore removed the references and referred to Chaper 7 instead.
2-2261	2	62	41	62	43	That observed changes in gases contribute to the observed increases in downwelling thermal radiation seems somewhat out of [Klaas Folkert Boersma, Netherlands]	Accepted, we removed this paragraph, as it describes more an expectation rather than an observational evidence as requested in this chapter.
2-2262	2	62	41	62	48	The first part of this excerpt says the hydrological cycle drives radiation changes, and the second part says the opposite. [Dian Seidel, USA]	Section 2.5.3.4 has been removed as a considerable part contains theory, while the emphasis in this chapter is on the documentation of observed changes in the climate system. A remaining part of this section is now covered in Section 2.5.3.3
2-2263	2	62	49	62	49	Allen and Ingram (2002) estimate a precipitation increase of 3.4% per °C of warming. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account- we replaced "at around 2-3%/K" by "at around 3%/K"
2-2264	2	62		63		2.5.3.4 please see also Trenberth 2011. p 63 l 6 is not correct as Wentz et al (2007) does not verify in other data sets and is dependent on the particular period chosen. [Kevin Trenberth, USA]	This part of section 2.5.3.4 has been removed, see also response given in 2-2262
2-2265	2	62				place after discussing the effects of direct attenuation of SSR by clouds and aerosols so extensively beforehand. [Klaas Folkert Boersma, Netherlands]	Reviewer comment erroneosly cut in 2 pieces. See response given in 2-2261
2-2266	2	62				2.5.3.2 Similarly, this is not a balanced assessment. [Kevin Trenberth, USA]	The literature related to observed changes in thermal and net radiation is small. We are not awave of any other observation-based studies that could be included in this assessment.

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2-2267	2	62				2.5.3.3 This section perhaps gets to the crux of the matter as to whether there is any explanation for how warming occurs while dimming occurs. One possibility is that dimming is overestimated and regional, related to urban sites and air pollution is a factor locally. Until or unless there is a more complete assessment as to how this all works physically, declarations about the reality of dimming and brightening in spite of many reservations in some studies that are not adequately reflected here. [Kevin Trenberth, USA]	A number of studies show the spatiotemporal coincidence of dimming phases with lack of warming (from the 1950s to the 1980s in many regions, as well as enhanced warming in periods when dimming disapeared (1990s)), so there is no evidence for a conflict between solar dimming/brightening and warming (see e.g. Wild et al. 2007 GRL or Wild 2012 BAMS). Urbanisation effects are, as in observationsal records of many climate variables, an issue which needs careful assessment as stated on page 61 10-14 in the original FOD.
2-2268	2	63	5	63	7	This discussion of Wentz et al 2007 large satellite microwave precipitation trends is inconsistent with the previous section of chapter 2 on precipitation trends (much smaller and less certain). Needs to be resolved. [Bruce Wielicki, USA]	This part of section 2.5.3.4 has been removed, see also response given in 2-2262
2-2269	2	63	10	63	10	What is meant by "statistical" uncertainty here? Noisy data? Due to observational uncertainty? Due to geophysical variability? [Dian Seidel, USA]	This part of section 2.5.3.4 has been removed, see also response given in 2-2262
2-2270	2	63	15	63	18	The revisions accentuate the absurdity of the whole exercise. The "uncertainties" are actually ranges of the extent to which all of the figures vary with changed circumstances and the so-called "balance" is admittedly bogus. All of the figures involve calculations usiing non linear equations and skewed distribution curves, Even the supposedly constant solar radiation and TOA radiation received are constantly varying. [VINCENT GRAY, NEW ZEALAND]	We cannot follow the reviewer's argumentation here
2-2271	2	63	20	63	20	Replace "could be substantially expanded" with "have substantially increased". [Robert Waterland, United States of America]	Taken into account, we replace it by "have been substantially extended"
2-2272	2	63	26	63	27	Sentence does not make sense [Peter Burt, UK]	Taken into account, we revised the sentence.
2-2273	2	63	26	63	29	The conclusion on the surface solar radiation needs work. All of the discussion of this hinges on regional and not global trends. Given the link to regional aerosol changes, this is ok, but some clarification is needed to clarify whether the conclusions only apply to limited geographic regions (some land) or whether globally significant trends exist. This section overemphasized regional trends which may have much smaller impact on global trends that drive global climate change. [Bruce Wielicki, USA]	Taken into account: we revised the setence originally on page 63 line 26 as follows: "Since AR4 the evidence for widespread decadal changes in solar radiation incident on land surfaces has been substantiated " and added a sentence "Over some remote land areas and over the oceans, confidence is low due to the lack of direct observations, which hamper a true global assessment". There is evidence for a large scale signal over terrestrial surfaces from direct radiation measurements as well as more abundant related proxy observations, such as diurnal temperature range and sunshine duration data. Also, remote radiation sites pick up a coherent signal (e.g. Dutton et al. 2006) suggesting a non-purely local phenomenon. Satellite-derived surface fluxes show a coherent brightening signal from the 1980s to 2000 over oceans, where direct aerosol effects are less important, but indirect effects are more effective, which are easier to capture by satellites (Wild 2012 BAMS). Recent modelling studies (Booth et al. 2012 Nature) further suggest that aerosol can induce significant trends in sea surface temperatures, which further supports larger scale variation in surface solar radiation. But since this is the observational chapter, we stick here with pure observations, and do not refer to the evidence from modelling studies.

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2-2274	2	63	28	63	28	“hydrological quantities”, please be specific with respect to which variables this refers to (in Section 2.5 mainly precipitation is discussed). [Lena M. Tallaksen, Norway]	Taken into account. We replaced “hydrological quantities” with "aerosol characteristics".
2-2275	2	63	28	63	29	The indications for the net radiation are based only on indirect inferences, thus this sentence appears in conflict with the assertion (line 12, page 62) that "Little is known about decadal changes in the surface net radiation". [Claudio Cassardo, Italy]	Taken into account, we adjusted the sentence originally on page 62, line 12 as follows: "There is limited information on decadal changes in surface net radiation from direct observations, in large part because…"
2-2276	2	63	31	74	23	The subchapter 2.6 tells a lot about the circulation features themselves. However, there is nothing about the influence of the circulation mechanisms or indices on the climate parameters. Numerous studies were done to describe quantitatively how the variations in atmospheric circulation result in air temperature or precipitation fluctuations and/or trends. Notably, NAO for Europe, ENSO for South America, PNA for North America are very important. From combined studies, trying to identify relative role of different mechanisms on the air temperature variations, one could cite the papers: Popova V. (2007) Winter snow depth variability over northern Eurasia in relation to recent atmospheric circulation changes. International Journal of Climatology, Vol. 27, pp.1721-1733. Popova V., Shmakin A. (2010). Regional structure of surface air temperature variations in North Eurasia in the second half of 20th - beginning of 21st centuries. Izvestiya Atmospheric and Oceanic Physics, Vol. 46, No 2, pp. 15-29. The first one deals with relative roles of NAO and SCAND in snow depth variations in North Eurasia in 1950-2005, while the second one investigates NAO and SCAND impacts on the air temperature variations. In the latter paper, it is shown that together the two factors explain 100% of the winter warming trend since 1970. Moreover, these two studies revealed that relative importance of certain circulation modes can vary on decadal scale: for North Eurasia, SCAND was the most influential mechanism in 1950-1975, while since 1975 the leading role passed to NAO. [Andrey Shmakin, Russia]	Noted - Chapter 14 does that; a further reference is added in the third paragraph
2-2277	2	63	31	74	23	The subchapter 2.6 tells a lot about the circulation features themselves. However, there is nothing about the influence of the circulation mechanisms or indices on the climate parameters. Numerous studies were done to describe quantitatively how the variations in atmospheric circulation result in air temperature or precipitation fluctuations and/or trends. Notably, NAO for Europe, ENSO for South America, PNA for North America are very important. From combined studies, trying to identify relative role of different mechanisms on the air temperature variations, one could cite the papers: Popova V. (2007) Winter snow depth variability over northern Eurasia in relation to recent atmospheric circulation changes. International Journal of Climatology, Vol. 27, pp.1721-1733. Popova V., Shmakin A. (2010). Regional structure of surface air temperature variations in North Eurasia in the second half of 20th - beginning of 21st centuries. Izvestiya Atmospheric and Oceanic Physics, Vol. 46, No 2, pp. 15-29. The first one deals with relative roles of NAO and SCAND in snow depth variations in North Eurasia in 1950-2005, while the second one investigates NAO and SCAND impacts on the air temperature variations. In the latter paper, it is shown that together the two factors explain 100% of the winter warming trend since 1970. Moreover, these two studies revealed that relative importance of certain circulation modes can vary on decadal scale: for North Eurasia, SCAND was the most influential mechanism in 1950-1975, while since 1975 the leading role passed to NAO. [Andrey Shmakin, Russia]	Duplicate comment (2-2276)
2-2278	2	63	33	63	34	The word "changes" appears 5 times in 28 words. A new world record! [Dale Hurst, United States of America]	Noted but sentence omitted.
2-2279	2	63	33	63	35	Rewrite sentencre [Karen Rosenlof, United States of America]	Noted but sentence omitted.
2-2280	2	63	33	63	45	Atmospheric circulation also influences ocean circulation. [VINCENT GRAY, NEW ZEALAND]	Rejected - this is Chapter 6 material.
2-2281	2	63	33	86	33	All this material Is more effectively dealt with by traditional meteorology. The absurd IPCC static model of a constant climate is unconcerned with these details of climate reality [VINCENT GRAY, NEW ZEALAND]	Rejected - comment not clear.
2-2282	2	63	37	63	38	Consider skipping this material on geopotential height changes, which are not actually circulation changes. Also, consider using the word "wind" somewhere in this paragraph. [Dian Seidel, USA]	Noted - Sentece rephrased (but changes in GPH are circulation changes), material is shortened.
2-2283	2	63	42	63	43	Change "in the early 2000s" to "from the mid 1990s to the early 2000s". [Xiangdong Zhang, United States of America]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2284	2	63	43	63	45	Can you please provide references for this? [Norman Loeb, United States of America]	Noted - This is a preview of the section, now better clarified through restructuring.
2-2285	2	63	47	63	48	Please assess whether heavier use of reanalysis for trend studies is actually a good thing. The opinion of someone actually involved with creating reanalysis datasets would be valuable here. [Karen Rosenlof, United States of America]	The task is to assess the literature, a large part of which is based on reanalyses (see Box 2.3). To assess whether or not this development is healthy is not the task of IPCC.
2-2286	2	63	47	63	48	Saying "reananlyses have gained even more weight" is an interesting way of putting the situation. Perhaps this weight gain should be assessed here. Is it healthy? [Dian Seidel, USA]	The task is to assess the literature, a large part of which is based on reanalyses (see Box 2.3). To assess whether or not this development is healthy is not the task of IPCC.
2-2287	2	63	49	63	49	text missing after 'space-borne' (the data sets are not in themselves space-borne) [Peter Burt, UK]	Editorial
2-2288	2	63				2.5.4 I strongly disagree with this summary for all the reasons given above. It requires a more balanced assessment and consensus view of several authors without vested interests. [Kevin Trenberth, USA]	Noted, the summary has been changed.
2-2289	2	64	3	64	3	Replace "SLP" with Sea Level Pressure (SLP)". [Robert Waterland, United States of America]	Noted - The acronym SLP is now introduced earlier in the paragraph.
2-2290	2	64	9	64	9	observations → observation [Peter Burt, UK]	Noted but sentence omitted.
2-2291	2	64	9	64	10	It may be interesting to punctualize if these discrepancies are due only to the different methods of reconstruction techniques, or to the different choice of the (few) stations, or … [Claudio Cassardo, Italy]	Noted, but lack of space for detailed discussion: slightly reworded.
2-2292	2	64	12	64	12	What is the import of the DJF pressure drops in these regions? Also, need to define DJF [Dian Seidel, USA]	Noted - This is a summary of main AR4 findings. DJF is now spelled out.
2-2293	2	64	12			"AR4 concluded that DJF sea level pressure decreased between 1948 and 2005 in the Arctic, Antarctic and North Pacific." I think DJF deserves to be spelled out - elsewhere DJFM is quite widely used, but DJF is rare - there are enough acronyms to befuddle the reader without having quite unneccessary ones sprung. Also one has to ask if December-February are equally relevant in the Arctic and Antarctic. [Philip Lloyd, South Africa]	Noted - DJF is now spelled out - DJF was used by AR4 and here summarize AR4 findings hence cannot change to DJFM.
2-2294	2	64	16	64	16	Two recent studies have investigated these issues in more detail. After "1861-1992" recommend adding: ", while Power and Kociuba (2011a) found statistically significant declines over periods ranging from 1901-1999 through to 1901-2004". The (June-December) Southern Oscillation Index (SOI), a measure of MSLP pressure difference over the South Pacific between Tahiti and Darwin, exhibited a downward trend over the period 1876-2008 and over 1958-2008, but the declines are not statistically significant (Power and Kociuba 2011b)". References: Power, S.B., and G. Kociuba, 2011a: What caused the observed twentieth century weakening of the Walker circulation? J. Climate, 24, 6501–6514, doi: http://dx.doi.org/10.1175/2011JCLI. Power, S.B. and G. Kociuba, 2011b: Impact of global warming on the Southern Oscillation Index. Climate Dynamics, 37, 1745-1754, DOI: 10.1007/s00382-010-0951-7. [Scott Power, Australia]	Taken into account (references added).
2-2295	2	64	16	64	18	Can this 1861-1992 result be updated data from the past two decades? Similarly, can the 1950-1996 results be updated? [Dian Seidel, USA]	Taken into account - references added (see reply to 2-2294).
2-2296	2	64	22	64	23	most rceebnt decade → 2000s OR 2001 - 2011 [Peter Burt, UK]	Editorial
2-2297	2	64	23	64	23	The word "culminated" is probably not the best choice, since we are already past 2009/2010. [Dian Seidel, USA]	Editorial
2-2298	2	64	23		29	Rahimzadeh et al., 2010 surveyed the variabilty of wind speed in Iran. In genreal the number of artilce that have surveyed the wind speed variabilty is less than for climatic parameter like temeprature and precipitation.	Noted, but reference is too local.

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						therfore I suggest you , use it all. the identification of article is :Rahimzadeh, F.,A. M. Noorian, M. Pedram, M. C. Kruk, 2010, "Wind speed variability over Iran and its impact on wind power potential: a case study for Esfehan Province, Journal of Meteorological Applications. DOI: 10.1002/met.229. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	
2-2299	2	64	24	64	24	Add a citation "Zhang et al., 2008" before "Osborn, 2011". [Xiangdong Zhang, United States of America]	Not clear what is meant and why.
2-2300	2	64	27	64	27	Add a citation "Zhang et al., 2008" after "Panagiotopulos et al., 2005". [Xiangdong Zhang, United States of America]	Not clear what is meant and why.
2-2301	2	64	28			why is this period 1980 to 1999 included? [Kevin Trenberth, USA]	This period was used in the cited paper.
2-2302	2	64	30	64	35	The SLP diagrams in Fig 2.37 (also p166) are not very informative. It may be better to expand these diagrams to show SLP anomalies in each decade. These should show the variations much more clearly. In the lower diagrams, 100hPa may be too high in the atmosphere to optimally show the latitudinal expansion of height levels. 100hPa includes the cooling stratosphere in the extratropics, so 250hPa or 300hPa may well be better. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account - Figure is changed (100 hPa is no longer shown, SLP is expanded).
2-2303	2	64	32	64	32	Figure caption says that SLP is from November to March but the figures are labelled with November-April [Celeste Saulo, Argentina]	Editorial
2-2304	2	64	40	64	52	Specific Comments for Section 2.6.2: A scientific link between the climate change and variability on wind sources is missing. e.g. large- scale dynamics and orography should be considered. There are some key papers (Guo et al., 2011; Najac et al., 2011) need citation such as: Guo, H., M.Xu, Q.Hu, 2011, changes in near-surface wind speed in China:1969-2005. Int.J.Climatılogy, 31, 349-358.; J. Najac, C. Lac, L. Terray, 2011, Impact of climate change on surface winds in France using a statistical-dynamical downscaling method with mesoscale modelling, 31, 415-430. [SELAHATTIN INCECIK, TURKEY]	Noted - References do not add substantially, downscaling is outside the scope of observations Chapter.
2-2305	2	64	42	64	42	Replace "the microwave band" with "Microwave radiometers". [Robert Waterland, United States of America]	Rejected - there are also scatterometers, altimeters, SARs in the general microwave band
2-2306	2	64	42	64	52	This background information can easily be shortened or eliminated. [Dian Seidel, USA]	Accepted - the background info has been shortened.
2-2307	2	64	45	64	46	Growth in typical ship size IS responsible for a growth in mean anemometer height (Kent et al. 2007) [Elizabeth Kent, England]	Accepted - reworded.
2-2308	2	64	46	64	46	Anemometer heights are available for many ships in WMO Pub. 47 (Kent et al. 2007) and this information has been incorporated into ICOADS v2.5 so is now readily available. Thomas et al. 2008 (Thomas, B. R., E. C. Kent, V. R. Swail and D. I. Berry, 2008: Analysis of Monthly Mean Marine Winds Adjusted for Observation Method and Height, International Journal of Climatology, 28(6), 747-763, DOI: 10.1002/joc.1570.) analysed only winds of known height/method. [Elizabeth Kent, England]	Accepted - reworded.
2-2309	2	64	48	64	52	Thomas et al. (2008) concluded that up until the early 1980s the findings of Cardone et al. (1990) [that the transition from visual to anemometer and of increasing anemometer height caused a spurious increase in reported wind speed that was largely removed by accounting for these known changes] were correct. After this period they found that Beaufort-scale adjusted visual winds increased in strength more than height-adjusted winds. They concluded that either some anemometer winds were being reported flagged incorrectly as visual winds or that knowledge by the observer of the unadjusted anemometer wind speed was influencing their subsequent choice of estimated Beaufort interval. Berry and Kent (2011: Berry, D.I. and E. C. Kent, 2011: Air-Sea Fluxes from ICOADS: The Construction of a New Gridded Dataset with Uncertainty Estimates, International Journal of Climatology, 31(7), 987–1001, DOI: 10.1002/joc.2059.) applied an ad hoc adjustment to visual winds after 1985 to improve agreement between height adjusted anemometer winds and visual winds. Uncertainty still remains in the in situ wind record from ships and the buoy record has not yet been fully exploited. [Elizabeth Kent, England]	Accepted - reworded.
2-2310	2	64	48	64	52	I would not take issue with the statement that the long-term variability in winds is poorly-defined, but rather argue that the reasons for the marine in situ wind record being problematic are not as described in this	Accepted - offending paragraph excluded.

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						paragraph. In addition to the issues described above, the changing size and shape of vessels may result in changing biases due to air-flow distortion (Moat, B.I., Yelland, M.J., Pascal, R.W. and Molland, A.F. (2005) An overview of the airflow distortion at anemometer sites on ships. International Journal of Climatology, 25, (7), 997-1006. (doi:10.1002/joc.1177)) and systematic changes to instrument exposure cannot be ruled out. [Elizabeth Kent, England]	
2-2311	2	64	54	64	54	This repeats material on line 43 [Dian Seidel, USA]	Accepted - re-edited.
2-2312	2	64	55	64	55	scatterometers measure surface wind vectors and they have been measured since 1991 on ERS-1 so what is stated here is incorrect. See Quilfen et al (2001). Quilfen, Y., B. Chapron, D. Vandemark, 2001: The ERS Scatterometer Wind Measurement Accuracy: Evidence of Seasonal and Regional Biases. J. Atmos. Oceanic Technol., 18, 1684–1697. http://dx.doi.org/10.1175/1520-0426(2001)018<1684:TESWMA>2.0.CO;2 [Roger Saunders, United Kingdom]	Taken into account - paragraph rewritten
2-2313	2	64	56	64	56	Sometimes the use of several acronyms, or of less known acronyms, could make problematic to understand the text. Even if there is a summary table with all acronym meaning, I suggest in some cases, to indicate also their meaning in the text, especially for those less popular as, for instance, the Special Sensor Microwave Imager (SSM/I) here mentioned. [Claudio Cassardo, Italy]	Editorial - checked that all acronyms are properly defined
2-2314	2	64	57	65	1	This description makes it sound like CCMP & Blended Sea Winds datasets are solely based on SSM/I data. [Elizabeth Kent, England]	Accepted - rewritten
2-2315	2	65	1	65	6	This could be eliminated [Dian Seidel, USA]	Rejected - if the ocean surface wind topic is to be covered, this is the description of the essential recent progress.
2-2316	2	65	1		14	wind requires high frequency sampling: every 6 hours. [Kevin Trenberth, USA]	Noted
2-2317	2	65	5	65	5	newm = ? [Peter Burt, UK]	Editorial
2-2318	2	65	5	65	5	Replace "newm" with "new" [Mihai Dima, Romania]	Editorial, same as 2-2317
2-2319	2	65	5	65	5	newm [Elizabeth Kent, England]	Editorial, same as 2-2317
2-2320	2	65	5	65	5	newm --> new [Hai Lin, Canada]	Editorial, same as 2-2317
2-2321	2	65	5	65	5	replace "newm" with "new" [Celeste Saulo, Argentina]	Editorial, same as 2-2317
2-2322	2	65	5	65	5	Change “newm” to “new” [Roger Saunders, United Kingdom]	Editorial, same as 2-2317
2-2323	2	65	5	65	5	Typo, "new" (not "newm"). [Christian-D. Schoenwiese, Germany]	Editorial, same as 2-2317
2-2324	2	65	5	65	5	newm' should be just 'new'. [Zhaomin Wang, UK]	Editorial, same as 2-2317
2-2325	2	65	5			"m" should be dropped from "newm" [Michael Brewer, United States of America]	Editorial, same as 2-2317
2-2326	2	65	5			"newm" should be "new" [Jean Poitou, France]	Editorial, same as 2-2317
2-2327	2	65	8	65	14	Figure 2.38 really shows how varied the four products are. In the S Pacific, for instance, they four show a0, b+, c++ and d-. In the central Indian, it is a-, b0, c++ and d0. In N Atlantic, a+, b++, c-, d0. If they cannot even agree on the direction of the trend, then the conclusions need to be significantly more tentative than they are at present. [Philip Lloyd, South Africa]	Accepted. Figure 2.38 (now Fig 2.37) is redone, its description in the text is rewritten.
2-2328	2	65	9	65	11	This is not true. WASW is negative around Antarctica. [Uwe Stoeber, Germany]	Accepted. See 2-2327.
2-2329	2	65	9	65	12	The trend patterns in Fig 2.38 look quite different. "largely consistent" seems rather subjective to me. [Gareth S Jones, UK]	Accepted. See 2-2327.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2330	2	65	9	65	14	Frankly speaking, I am unable to see that "The trend pattern [of Blended Sea Winds and CCMP] is largely consistent with the corresponding trend in 20CR and with the WASWind product as well" as mentioned in the text. Visually I see some similarities between (a) and (b) (but in (a) the negative trends in the tropics appear stronger, in absolute values, as well as the positive trends in Antarctica). The trends of (c) appear to me quite different from (a) and (b), especially over Indian and Pacific equatorial areas, but also over polar regions, where the positive trends in (c) are much weaker. In (d), the box plot graphic style make hard to have a visual comparison with the others (why it has not been used the same graphical style?), but I can notice that, in the southern extratropical oceans, the positive trends are not prevailing as in the other datasets, and also in the polar regions (excluded near Alaska and Bering strait) there are few (and weaker) positive trends. Thus, if (d) is an improvement with respect to the others, this represents a substantial improvement. [Claudio Cassardo, Italy]	Accepted. See 2-2327.
2-2331	2	65	10	65	10	What does "largely consistent" really mean? In looking at the figure, it seems that the WAS winds show negative trends at high southern latitudes while others are positive. The equatorial patterns in all four look quite a bit different as well. [Karen Rosenlof, United States of America]	Accepted. See 2-2327.
2-2332	2	65	12	65	14	This statement does not seem to be supported by the WASWind results in Fig. 2.38. [Dian Seidel, USA]	Accepted. See 2-2327.
2-2333	2	65	13			From figure 2.38; I would argue that WASWind doesn't show an increase in wind, south of 40°S; the general trend of WASWind south of 40°S is more a decrease. Ships probably avoid high wind regions and have more leeway to do that than in other part of the globe? Therefore the decrease? [Francois DANIS, France]	Accepted. See 2-2327.
2-2334	2	65	16	65	16	Is there a specific reason why wind speed is only measured over the ocean with satellites? [Birgit Hassler, USA]	Rejected - ICOADS, WASWind, NOCS are based on the in situ data.
2-2335	2	65	18	65	18	If winds over land in reanalyses do not match observations, why would we assume they are reliable over ocean? Consider removing trends based on reanalyses here and in Fig. 2.38 [Dian Seidel, USA]	Noted - Disagreement between reanalyses and in-situ winds does not mean that reanalysis are wrong (we are not making that statement). We now show another reanalysis, include the land area, and also show trends from land stations.
2-2336	2	65	23		29	The intervals of the trends here are not stated. Needs work. [Kevin Trenberth, USA]	Rejected - time periods of trends are all stated.
2-2337	2	65	29	65	29	Antarctic strengthening of winds in Turner et al. (2005) was only for coastal stations. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - sentence changed.
2-2338	2	65	32	65	32	m/s → m s-1 [Peter Burt, UK]	Editorial
2-2339	2	65	34	65	34	A further twist is added by Troccoli, A., K. Muller, P. Coppin, R. Davy, C. Russell, and A. L. Hirsch, 2012: Long-Term Wind Speed Trends over Australia. J. Climate, 25, 170–183. doi: http://dx.doi.org/10.1175/2011JCLI4198.1 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reference added.
2-2340	2	65	43	54	44	It appears that upper levels winds are still getting little attention in this report. [Karen Rosenlof, United States of America]	Noted - There is not much to be reviewed.
2-2341	2	65	43	65	43	got → received [Peter Burt, UK]	Editorial
2-2342	2	65	44	65	44	It's not quite accurate to characterize radiosonde and pilot balloon observations as "global", given station distributions. [Dian Seidel, USA]	Taken into account - reworded.
2-2343	2	65	46	65	46	It's not necessarily true that wind inhomogeneities are "far less common" than temperature inhomogeneities. The former have not been studied nearly as extensively as the latter. [Dian Seidel, USA]	Taken into account - reworded.
2-2344	2	65	47	65	47	Atmospheric Motion Winds from satellite imagery have been derived since FGGE (1979) giving a good global coverage away from the poles (and more recently a polar winds dataset has also been derived). I suggest a sentence on this measurement technique is added as it is an important input to reanalyses at least. The following reference may be useful Menzel (2001) Menzel, W. P., 2001: Cloud Tracking with Satellite Imagery: From the Pioneering Work of Ted Fujita to the Present. Bull. Amer. Meteor. Soc., 82, 33–47.	Taken into account - reference added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						doi: http://dx.doi.org/10.1175/1520-0477(2001)082<0033:CTWSIF>2.3.CO; [Roger Saunders, United Kingdom]	
2-2345	2	65	49	66	5	To avoid confusion, in the first paragraph "In contrast to the wind stilling at the surface, no or much weaker trends were found for lower tropospheric winds from balloon data or reanalyses" I suggest to add "at global scale", in order to differentiate this finding from that of Jiang et al. (2010), valid only over China. In line 2, again I suggest to add "global": " systematic global trend analyses". [Claudio Cassardo, Italy]	Editorial
2-2346	2	65	51	65	51	Change "stilling" to "slowing". [Karen Rosenlof, United States of America]	Editorial
2-2347	2	65	53			Shouldn't there be a vertical length scale associated with the shear unit? [Drew Shindell, USA]	Taken into account - reworded.
2-2348	2	65		65		Section 2.6.2: Is it possible to carry out a little analysis comparing trends in SLP (Fig. 2.37) and surface winds, to see if they are consistent? [Alice Grimm, Brazil]	Rejected - Fig. 2.37 does not show SLP trends.
2-2349	2	65				2.6.3 This is not an assessment. [Kevin Trenberth, USA]	Taken into account - the paragraph is rewritten.
2-2350	2	65				2.6.4 This is cavalier wrt temporal sampling; esp over Antarctica. The records are quite incomplete. See also Chap 14. [Kevin Trenberth, USA]	Comment unclear and unspecific (no line numbers). "Global" replaced with "large scale"
2-2351	2	66	7		31	I assume that these results are mostlycold-point tropopauses. Calrifying this and the 'other' technique would be helpful. [Larry Thomason, United States of America]	Noted - cold-point tropopause is added.
2-2352	2	66	9	66	9	change "integral" to "integrated" [Karen Rosenlof, United States of America]	Editorial
2-2353	2	66	9	66	9	Should "integral" be changed to "integrated"? [Dian Seidel, USA]	Editorial
2-2354	2	66	20			"Angell (2006) found similar trends in NNR" It took me some time to discover that NNR was not an acronym but a data set. This needs to be clarified - perhaps "Angell (2006) found similar trends in the NNR dataset" [Philip Lloyd, South Africa]	Taken into account - reworded.
2-2355	2	66	23	66	31	A newer study of tropopause trends is Wang et al. 2012, 'How well do we know recent climate trends at the tropical tropopause?', in review at J.G.R. [Melissa Free, USA]	Taken into account - reference added.
2-2356	2	66	24	66	25	This sentence is only partially accurate. (Ie, temperature trends do not explain all the subtleties of water vapor trends). Better to say that to first order temperatures control water vapor input (and Brewer 1949 is what I'd use as a reference). You can use the tape recorder in water vapor as an example that temperatures (as demonstrated by large oscillations in the annual cycle) control the annual cycle of water vapor. [Karen Rosenlof, United States of America]	Taken into account - reworded.
2-2357	2	66	27	66	28	"Trends in tropopause temperature are generally considered uncertain (Fueglistaler et al., 2009). However, Wilcox et al. (2011), using a new definition of the tropopause," I think the important thing about the Fueglistaler review is that it demonstrates that 'the tropopause' is a figment of the imagination - it is a deep layer which is a transition between troposphere and stratosphere. If this is accepted, then some 'new definition' of the tropopause does not really assist, and the search for trends at The Troposphere, however defined, cannot really add to our understanding. I suggest Wilcox et al does not deserve mention. [Philip Lloyd, South Africa]	The paragraph is reworded.
2-2358	2	66	27	66	28	Why are trends in tropopause temperature considered uncertain? And note, for water vapor, it's cold point temperature that matters, not tropopause temperature. [Karen Rosenlof, United States of America]	The paragraph is reworded, "cold point tropopause temperaturews" is specified, and a reference (Wang et al. 2012) is added to demonstrate the uncertainties.
2-2359	2	66	27	66	31	How is the reader to reconcile the general statement of uncertainty in trends with the specific results of Wilcox et al.? [Dian Seidel, USA]	Taken into account - Wilcox et al. is removed.
2-2360	2	66	28	66	29	Note what the "new definition" is (a blend of thermal and dynamical previous definitions). It would also be useful to note how this new definition compares to other estimates. Is a trend based on reanalysis really comsidered accurate? [Karen Rosenlof, United States of America]	Taken into account - Wilcox et al. reference is removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2361	2	66	30	66	31	Some clarification needed: All northern hemisphere? Subtropoics at all or some specified altitudes? [Christian-D. Schoenwiese, Germany]	Noted, but sentence is omitted
2-2362	2	66	31	66	31	What is the "eastern emisphere"? Also at page 68, line 10. [Claudio Cassardo, Italy]	Noted, but sentence is omitted
2-2363	2	66	31	66	31	Subtropical jets. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted, but sentence is omitted
2-2364	2	66	31	66	31	Last sentence needs a rewrite. [Karen Rosenlof, United States of America]	Noted, but sentence is omitted
2-2365	2	66	31			It might be worth noting that these temperature trends are apparently inconsitent with the stratospheric water vapor trends discussed earlier in the chapter. [Drew Shindell, USA]	Noted, but sentence is omitted
2-2366	2	66	35	68	25	The tropical belt widening (and the corresponding polar jet stream poleward movement) evidenced in these session are assessed as a trend concerning a multidecadal period, so they are annual. Provided that I am not an expert of global circulation changes, I am wondering if such phenomena are larger in the colder season, as it is evidenced by the number of cyclones in the successive paragraph. [Claudio Cassardo, Italy]	The widening is an annual signal.
2-2367	2	66	43	67	53	In this section ERA-INTERIM extends back to 1979, so the light blue line in Fig 2.39 can be extended back 10 more years. [Philip JONES, UK]	Taken into account - updated
2-2368	2	66	45	66	45	insert 'the' after 'to' [Peter Burt, UK]	Editorial
2-2369	2	66	46	66	46	replace hyphens with commas [Peter Burt, UK]	Editorial
2-2370	2	66	53			Figure 2.39. Once again: difficult to distinguish different line plots: perhaps combine dashed/solid/color to improve? thicker legend lines to easier see colors. [Bruce Wielicki, USA]	Accepted - line style is changed
2-2371	2	66	56	66	56	Oort and Yienger (1996) used 30°N not 40°N in their definition of the northern Hadley circulation. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - rephrased.
2-2372	2	66				2.6.5 The paper by Trenberth et al 2011 on reanalyses is relevant here to questions of data quality. [Kevin Trenberth, USA]	Taken into account - reference added.
2-2373	2	67	1		53	The changes in the Hadley and Walker circulations are huge with ENSO and the only proper way to do this is to remove and separate out ENSO effects. This has not been done properly. Datasets are generally inadequate, see Trenberth et al 2011 for reanalyses. Careful wording is required in dealing with this issue. An assessment should make those comments. [Kevin Trenberth, USA]	Taken into account - A sentence is added.
2-2374	2	67	5	67	5	Work of Vecchi et al. has been augmented taking later data into account. Recommend replacing sentence with: Previous studies indicate a weakening of the Pacific Walker circulation based on changes of MSLP gradients across the Pacific from e.g. 1861-1992 (Vecchi et al. 2006) and from 1901-2004 (Power and Kociuba (2011a). Reference: Power, S.B., and G. Kociuba, 2011a: What caused the observed twentieth century weakening of the Walker circulation? J. Climate, 24, 6501–6514, doi: http://dx.doi.org/10.1175/2011JCLI. [Scott Power, Australia]	Taken into account - rephrased and reference added.
2-2375	2	67	9	67	9	More recent research has been conducted on this topic. Suggest adding after "strongest" on line 9: " Power and Kociuba (2011b) identified negative trends in the SOI for the periods 1876-2008, 1900-2008 and 1958-2008, but none of these trends are statistically significant". Reference: Power, S.B. and G. Kociuba, 2011b: Impact of global warming on the Southern Oscillation Index. Climate Dynamics, 1745-1754, DOI: 10.1007/s00382-010-0951-7. [Scott Power, Australia]	Reference added.
2-2376	2	67	9	67		Note: this paragraph needs to be consistent with and avoid duplication of material given on page 64, para 2. [Scott Power, Australia]	Taken into account - paragraph restructured.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2377	2	67	10	67	10	find → found [Peter Burt, UK]	Editorial
2-2378	2	67	13	67	13	find → found [Peter Burt, UK]	Editorial
2-2379	2	67	19	67	23	Note that the Davis paper shows using multiple metrics that statistical significance in regards to tropical widening based on reanalysis products may be an issue. What is the actual confidence in the conclusion that there has been tropical widening? [Karen Rosenlof, United States of America]	Noted - The widening of the tropical belt alone is assessed as "medium confidence", but taken together with the poleward shift of the jet and of the storm tracks (consistent results based on different data and approaches), we conclude with high confidence that there is a likely "poleward motion of circulation features" (also concluded in SREX for the storm tracks).
2-2380	2	67	19	67	31	The data sets shown in Figure 2.40 are so divergent that I can see no grounds for suggesting that "the width of the tropical belt has increased at least since 1979." Greater caution is probably needed. [Philip Lloyd, South Africa]	Rejected - "Suggest" is cautious enough given the fact that trends in widely different measures all agree in sign and many are significant.
2-2381	2	67	21	65	21	"the AR4": remove "the". [Hai Lin, Canada]	Editorial
2-2382	2	67	22	67	22	had → have [Peter Burt, UK]	Editorial
2-2383	2	67	32	67	39	This was my small contribution to the chapter, so I'm not completely objective here. However, I think the material in lines 32-36 and the material in lines 36-39 are imbalanced. The first section was an attempt to distill a lot of results into two brief assessment sentences. The second section (added after I submitted the contribution) reports very specific results from a single recent study, and provides quantitative results for tropical expansion that were not given for the other studies. I'd suggest combining the Wilcox et al. results with those cited in lines 34-36. The sentence (lines 36-37) about the 100 hPa geopotential height changes, shown in Fig. 2.37, bottom, is not (to my knowledge) based on peer-reviewed literature and should probably be deleted. [Dian Seidel, USA]	Taken into account - Wilcox is no longer cited and Fig. 2.37 is restricted to SLP.
2-2384	2	67	33	67	33	insert 'the' after 'of' [Peter Burt, UK]	Editorial
2-2385	2	67	38	67	38	Tropics → tropics [Peter Burt, UK]	Noted but sentence omitted.
2-2386	2	67	45			"cloudiness" should be "high cloud" [Kevin Trenberth, USA]	Editorial
2-2387	2	67	50	67	51	"The qualitative consistency of these observed changes in independent datasets suggests a widening of the tropical belt" As noted in the previous comment, Figure 2.40 shows so large a spread that even a suggestion does not seem warranted. [Philip Lloyd, South Africa]	Rejected - "Suggest" is cautious enough given the fact that trends in widely different measures all agree in sign and many are significant.
2-2388	2	68	1			the term "eddy driven" appears without justification or introduction. It is geostrophic to a large degree. [Kevin Trenberth, USA]	Taken into account - The term is dropped.
2-2389	2	68	7		25	Disagree: there are several studies, especially those of Chang (see Chang 2007 and previous works). Reanalyses suffer from not adequately tracking storm tracks and jets, especially prior to 1979. The conclusions are far from clear. [Kevin Trenberth, USA]	Sentence is omitted, Chang 2007 is cited further down.
2-2390	2	68	16	68	16	on → in [Peter Burt, UK]	Editorial
2-2391	2	68	17	68	17	Figure 2.40 does not seem to show poleward movement in the jet streams even though it does in the other parameters. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - rephrased.
2-2392	2	68	17			There is no visible trend in Figure 2.40. [Uwe Stoeber, Germany]	Taken into account - rephrased.
2-2393	2	68	18	68	18	Clarify whether "speed trends" refers to trends in the speed of jet stream winds or the speed of jet location migration. Also, if "eddy-driven jets have become more common", does that suggest that the concept of a single mid-latitude jet, that can migrate, is not correct? Is there some potential inconsistency (or appearance	Noted - sentence rephrased.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						thereof) in this section? [Dian Seidel, USA]	
2-2394	2	68	20	68	20	delete comma after 'Atlantic' [Peter Burt, UK]	Editorial
2-2395	2	68	21	68	21	– → , [Peter Burt, UK]	Editorial
2-2396	2	68	25	68	25	NH → Northern Hemisphere [Peter Burt, UK]	Editorial
2-2397	2	68	26	68	26	I don't think the "jet core has been contracting", but rather the polar vortex has been contracting (shrinking in area), in conjunction with poleward jet migration. [Dian Seidel, USA]	Taken into account -"Contracting" replaced with "migrating"
2-2398	2	68	30	68	30	delete comma after 'Atlantic' [Peter Burt, UK]	Editorial
2-2399	2	68	30	68	30	Ocean → Oceans [Peter Burt, UK]	Editorial
2-2400	2	68	35	68	35	NH → Northern Hemisphere [Peter Burt, UK]	Editorial
2-2401	2	68	35	68	35	..was found, however,..' → '...was found. However, ...' [Peter Burt, UK]	Editorial
2-2402	2	68	35	68	35	I suggest to add a couple of representative citations about previous work used in AR4. For example, after "was found", add "(e.g., Zhang et al., 2004; Yin, 2005). [Xiangdong Zhang, United States of America]	Rejcted - We start from AR4, not from the literature assessed therein.
2-2403	2	68	35	68	43	North Atlantic storminess has been considered in this paper. Cornes, R.C. and Jones, P.D., 2011: Am examination of storm activity in the northeast Atlantic region over the 1851-2003 period using the EMULATE gridded MSLP data series. J. Geophys. Res. 116, D16110, doi:10.1029/2011JD016007. [Philip JONES, UK]	Taken into account - reference added.
2-2404	2	68	35	68	43	A. Lehmann, K.Getzlaff and J. Harlaß, 2011: Detailed assessment of climate variability in the Baltic Sea area for the period 1958 to 2009. 46,185-196. They document a seasonal shift of strong wind events from autumn to winter and early spring over the Baltic sea. [Valentina Pavan, Italy]	Rejected - study area is too regional for this Chapter.
2-2405	2	68	35	68	43	G. Leckerbusch, D.Renggli and U.Ulbrich, 2008: Development and application of an objective storm severity measure for the Northeast Atlantic region. Meteorologische Zeitschrift, 17, 575-587. They show an increase in the number of sever storm over Northeast Altantic region, due to both an increase in wind intensity and to a broadening of the affected areas linked with longer broader cyclone paths. [Valentina Pavan, Italy]	Taken into account - reference added.
2-2406	2	68	35	68	43	The following reference is clearly relevant here: Bender, F. A-M., V. Ramanathan and G. Tselioudis (2011) Changes in extratropical storm track cloudiness 1983-2008: Observational support for a poleward shift, Clim. Dynam., DOI: 10.1007/s00382-011-1065-6. See also Chapter 7, page 16, lines 16-26. [Henning Rodhe, Sweden]	Taken into account - reference added.
2-2407	2	68	36			„uncertainties are significant“ the term significant should be replaced by, e.g., considerable. [Douglas Maraun, Germany]	Taken into account - reworded.
2-2408	2	68	37	68	37	Since the differences between two reanalyses were just highlighted, it's probably best to specify which reanalysis Schneidereit et al. used. [Dian Seidel, USA]	Taken into account - reworded.
2-2409	2	68	40	68	43	The following recent studies that are based on pressure triangles and also relate storminess to NAO are good references here (both are cited later in section 2.7.4): (1) Wang, X. L. L., et al. 2011: Trends and low-frequency variability of storminess over western Europe, 1890-2007. Climate Dynamics, 37 (No. 11), 2355-2371. DOI: 10.1007/s00382-011-1107-0. (2) Wang, X. L. L., et al. 2009b: Trends and Variability of Storminess in the Northeast Atlantic Region, 1874-2007. Climate Dynamics, 33(7), 1179-1195. DOI: 10.1007/s00382-008-0504-5. [Xiaolan Wang, Canada]	Taken into account - reference added.
2-2410	2	68	45	68	45	What 1900-2004 data were used in this analysis? [Dian Seidel, USA]	Accepted (station data)
2-2411	2	68	49	68	49	add "." after "NNR data". [Hai Lin, Canada]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2412	2	68	49		53	NNR and ERA-40 data are not adequate for this, see Trenberth, K. E., D. P. Stepaniak and L. Smith, 2005: Interannual variability of patterns of atmospheric mass distribution. J. Climate, 18, 2812-2825. [Kevin Trenberth, USA]	Taken into account - reworded.
2-2413	2	68	50	69	50	Replace "century plus" with a range of years. [Dian Seidel, USA]	Editorial
2-2414	2	68		68		Section 2.6.6.1: Is it possible to establish some connection with the data on Fig. 2.37? [Alice Grimm, Brazil]	Noted - Fig. 2.37 is changed, only SLP is shown, hence the jet changes cannot be linked.
2-2415	2	69	1	69	27	Blocking is also discussed in Sections 9.5.2.2 and 14.2.11. Consolidation and/or cross referencing needed [George Kiladis, USA]	Noted - cross-referencing is added.
2-2416	2	69	2	69	2	Replace "can be associated to" with "are often associated with". [Robert Waterland, United States of America]	Editorial
2-2417	2	69	2	69	2	Persistent blocking of what? Don't assume every reader will understand this phrase. [Robert Waterland, United States of America]	Taken into account - reworded.
2-2418	2	69	3			"blocking is held responsible" is not appropriate wording as blocking was part of the whole phenomenon not a cause. [Kevin Trenberth, USA]	Taken into account - reworded.
2-2419	2	69	4	69	6	Replace "are used to classify the weather on a given day. Alternatively, feature-based methods have been developed (Croci-Maspoli et al., 2007a). These methods require daily SLP fields or upper-level fields from reanalyses." with "are commonly used to classify the weather on a given day. Feature-based methods are also used (Croci-Maspoli et al., 2007a). All these methods require daily SLP fields and upper-level fields from reanalyses.". [Robert Waterland, United States of America]	Editorial
2-2420	2	69	8	69	19	The following reference also finds a decrease in blocking events annually over the Atlantic Region. Kreienkamp, F., Spekat, A. and Enke, W. 2010. Stationarity of Atmospheric Waves and Blocking over Europe – Based on a Reanalysis Dataset and Two Climate Scenarios. Theory of Applied Climatology 102: 205-212. [Anthony Lupo, USA]	Reference added. But note that Kreienkamp et al. conclude that "Hardly any evidence for a trend behaviour could be found between 1951 and 2007, apart from a very weak decrease during that time."
2-2421	2	69	8	69	27	The following reference found a global increases in blocking overall from 1970 to 2008. (I will submit this spring to a refereed journal, but the following reference is the best I can do for now - maybe this can hold the place until the second review.). In the Northern Hemisphere, this includes increases in Pacific and Continental region blocking. Lupo, A.R., Clark, J.V, Hendin, A., Kelly, A., Mihalka, K., Perrin, B., Puricelli, K., Kelley, A., 2008: The global increase in blocking occurrences. The 20th Conference on Global Climate Change. 88th Annual Meeting of the American Meteorological Society. 13 – 18 January, 2008, New Orleans, LA [Anthony Lupo, USA]	Rejected - Cannot consider papers conference presentations.
2-2422	2	69	8	69	27	Scaife et al. (2010) show that models continue to underestimate the occurrence of blocking at least in the Northern Hemisphere. Scaife, A. A., T. Woollings, J. Knight, G. Martin, T. Hinton, 2010: Atmospheric Blocking and Mean Biases in Climate Models. J. Climate, 23, 6143–6152. doi: http://dx.doi.org/10.1175/2010JCLI3728.1 [Anthony Lupo, USA]	Rejected - Models relevant to observations Chapter.
2-2423	2	69	10	69	10	30-year → 30 year [Peter Burt, UK]	Editorial
2-2424	2	69	10	69	17	Werner et al. and Philipp et al. basically agree. This should be clarified, references should not just be listed without being put into context. [Douglas Maraun, Germany]	Taken into account - reworded.
2-2425	2	69	11	69	12	Consider replacing "cylconic" and "anticyclonic" with "high" and "low pressure systems", and link these to storminess, to be more broadly understood. [Dian Seidel, USA]	Prefer to use authors' terminology, but an assessment sentence is added that summarizes the results in simpler form.
2-2426	2	69	12	69	14	I think “Trnka et al. (2009) also found an increase by more than 80% of the frequency of drought-conducive weather types in central Europe from the 1940s to 2005 during April to June” more accurately expresses Trnka et al.’s results. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account - reworded.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2427	2	69	16	69	17	Replace "in winter, more frequent blocking over Great Britain in spring, and a retreated Azores high in summer." with "in winter; more frequent blocking over Great Britain in spring; and a retreat of the Azores high in summer.". [Robert Waterland, United States of America]	Changed.
2-2428	2	69	17	69	19	Isn't the distinction between changes in the weather type frequency and within type variability somewhat arbitrary and artificial? When defining weather types, one collapses the whole parameter space onto a small number of points. If one would increase (decrease) the number of weather types, some of the within type variability would change into between type variability (and vice versa). So isn't the statement just that low frequency variability in mean temperature is caused by strong variability in weather patterns, low frequency variability in temperature extremes by smaller variability in weather patterns? [Douglas Maraun, Germany]	Taken into account - the sentence is reworded and statement on within type variability is omitted.
2-2429	2	69	18	69	18	Suggest to define "low-frequency variability". [Hai Lin, Canada]	Noted, but sentence omitted.
2-2430	2	69	19	69	19	Insert “often” before “governed”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted but sentence omitted.
2-2431	2	69	24	69	24	is → are [Peter Burt, UK]	Editorial
2-2432	2	69	24	69	24	add "." after "Section 2.6.9". [Hai Lin, Canada]	Editorial
2-2433	2	69	26	69	26	SH → Southern Hemisphere [Peter Burt, UK]	Editorial
2-2434	2	69	26	69	27	Weidenmann et al. (2002) also show a decrease in blocking numbers in the SH from 1970 to 1999, and then Lupo et al. (2008 - above), show an increase in SH blocking from 2000 - 2008. This includes an increase in Pacific and Indian Ocean region blocking. Weidenmann, J.M., A.R. Lupo, I.I. Mokhov, and E. Tikhonova, 2002: The Climatology of Blocking Anticyclones for the Northern and Southern Hemisphere: Block Intensity as a Diagnostic. Journal of Climate, 15, 3459-3473 [Anthony Lupo, USA]	Rejected - Paper is 10 years old and not an update since AR4.
2-2435	2	69	27			1948 to 1999 an odd period? The whole section 2.6.6 is not an assessment [Kevin Trenberth, USA]	Taken into account - the paragraph is completely rewritten.
2-2436	2	69	31	69	32	Replace "The stratosphere is coupled with the troposphere through fluxes of radiation, momentum, and mass. The most relevant characteristics of stratospheric circulation for climate and for trace gas distribution are the" with "The stratosphere is coupled to the troposphere through fluxes of radiation, momentum, and mass. The most important characteristicsof stratospheric circulation for climate, and for trace gas distribution, are the". [Robert Waterland, United States of America]	Editorial
2-2437	2	69	31	69	35	Either remove this paragraph (as unnecessary background information) or make it clearer to the non-specialist by defining and explaining QBO and BDC. [Dian Seidel, USA]	Accepted - paragraph rephrased.
2-2438	2	69	33	69	33	delete comma after 'Oscillation' [Peter Burt, UK]	Editorial
2-2439	2	69	33	69	33	BDC should be briefly characterized. [Christian-D. Schoenwiese, Germany]	Accepted - paragraph rephrased.
2-2440	2	69	33			also sudden stratospheric warmings [Kevin Trenberth, USA]	Accepted - paragraph rephrased.
2-2441	2	69	37	69	37	Explain why one would choose the 100 hPa level. Figure 2.37 is very difficult to read: perhaps it should be split up into individual charts for each time period. [Robert Waterland, United States of America]	Taken into account - Fig. 2.37 is changed (100 hPa omitted) and hence the sentence omitted
2-2442	2	69	37	69	38	As suggested above, remove Fig. 2.37 as not derived from citable literature. [Dian Seidel, USA]	Taken into account - Fig. 2.37 is changed (100 hPa omitted) and hence the sentence omitted
2-2443	2	69	38	69	38	where and what were the circulation changes? [Peter Burt, UK]	Taken into account - Fig. 2.37 is changed (100 hPa omitted) and hence the sentence omitted
2-2444	2	69	41			what period? [Kevin Trenberth, USA]	Unclear comment

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2445	2	69	42	69	42	vortex → Vortex [Peter Burt, UK]	Editorial
2-2446	2	69	44	69	44	insert 'the' after 'in' [Peter Burt, UK]	Editorial
2-2447	2	69	44	69	44	report --> reported [Hai Lin, Canada]	Editorial
2-2448	2	69	46	69	46	polar vortex → Polar Vortex [Peter Burt, UK]	Editorial
2-2449	2	69	47	69	47	"…high" --> "… are high" [Hai Lin, Canada]	Editorial
2-2450	2	69	47	69	48	Replace "Langematz and Kunze (2008) find a strong dependence of stratospheric GPH trends over the Arctic on the time period," with "Langematz and Kunze (2008) find that trends in stratospheric GPH trends over the Arctic depnd strongly on the time period used,". [Robert Waterland, United States of America]	Editorial
2-2451	2	69	51	69	57	Note: BDC changes can also be inferred from temperature observations. See chapter 4 of the ozone assessment discussion on BDC. It can also be estimated from reanalysis output via calcuation of the TEM circulation. [Karen Rosenlof, United States of America]	Taken into account - reworded.
2-2452	2	70	1	70	6	The material in Chapter 10 on stratospheric temperature change indicators of circulation changes (e.g., Fu et al.) seems relevant here. [Melissa Free, USA]	Noted - cross-referencing added.
2-2453	2	70	4	70	4	The findings of Rosenlof and Reid (2008) were jutifiably questioned by Lanzante (2009), whose work should be mentioned. Reference: Lanzante, J R, 2009: Comment on “Trends in the temperature and water vapor content of the tropical lower stratosphere: Sea surface connection” by Karen H. Rosenlof and George C. Reid. Journal of Geophysical Research, 114, D12104, doi:10.1029/2008JD01054 [Dian Seidel, USA]	Noted - reference added.
2-2454	2	70	4	70	6	"found no statistically significant trend in the age of air in the 24-35 km layer over the Northern mid-latitudes from" [Dale Hurst, United States of America]	Taken into account - rephrased.
2-2455	2	70	6	70	6	Add the following citation alongside Bonisch et al. (2009): Ray, E. A., et al. (2010), Evidence for changes in stratospheric transport and mixing over the past three decades based on multiple data sets and tropical leaky pipe analysis, J. Geophys. Res., 115, D21304, doi:10.1029/2010JD014206. [Dale Hurst, United States of America]	Noted - reference added.
2-2456	2	70	11	71	30	This box includes a very complicated table. Is it really necessary? I think this could be significantly simplified by just showing and discussing box 2.4 figure 2 [Karen Rosenlof, United States of America]	Rejected - this table is meant as an internal coordinating reference for traditional indices of climate variability used in the report.
2-2457	2	70	14			Modes of climate variability do not “impact” climate; they are statistical characteristics of the climate. Thus “represent statistical properties” is preferable to “impact”. [David Sauchyn, Canada]	Rejected - when climate variability is approximated by a superposition of a few modes, describing local climate variability in terms of contributions, or "impacts", of individual modes is a widely accepted useful approach.
2-2458	2	70	16	70	17	Unclear sentence: "relationships between regional climate variations at places far removed from 17 each other." [Gillles Molinié, France]	Rejected - unclear what the reviewer meant. 17 was the line number.
2-2459	2	70	16			„far removed“: is this correct English? [Douglas Maraun, Germany]	Editorial
2-2460	2	70	17	70	19	It is unclear from what the anomalies have to be substracted. It should be tell in regard of what the anomalies are computed: a global average or a mode average? I don't understand why the addition part is inside paranthesis while not the substracted part. [Gillles Molinié, France]	Editorial. Climate anomalies at pairs of stations exhibiting largest anti-correlation would be subtracted (e.g. Lisbon minus Reykjavik) to get an index (NAO).
2-2461	2	70	17	70	19	Consider re-writing this sentence to begin with the word "Index", then offer an explanation. Note, too, that lines 31-32 describe a very different way of defining an index of climate variability, so the overall explanation should probably be more general. [Dian Seidel, USA]	Editorial: suggestion followed.
2-2462	2	70	19	70	20	By regressing and "from other places" are unclear for me. Is it question of structure functions or	Editorial: reference to Box 14.1 added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						autocorrelations? [Gillles Molinié, France]	
2-2463	2	70	19	70	20	Besides regression, climate patterns also are often derived by PCA and expressed at EOFs. [David Sauchyn, Canada]	Noted. This is discussed further in the text.
2-2464	2	70	26	70	26	using 'etc' is poor style. Omit. [Peter Burt, UK]	Editorial: etc excluded
2-2465	2	70	28	70	28	"of climate system" -> "of the climate system" ? [Gillles Molinié, France]	Editorial: sentence excluded
2-2466	2	70	28	70	32	Is this paragraph really userfull? [Gillles Molinié, France]	Editorial: paragraph has been shortened and included into the first paragraph of the Box.
2-2467	2	70	31	70	31	insert comma after 'Indeed' [Peter Burt, UK]	Editorial
2-2468	2	70	34	70	34	insert 'the' after 'and' [Peter Burt, UK]	Editorial
2-2469	2	70	40	70	40	Box 2.4 is extremely useful, especially since continual reference to these modes is made throughout the entire report. It is very important that the definition of the modes be consistent throughout all chapters. Therefore the utility of Box 2.4 would be greatly enhanced if the "phase" of a mode was defined explicitly here, and care was taken to assure that these conventions were used throughout the report. This would be particularly helpful for those modes defined by EOFs. For example, an exact definition of the Wallace and Gutzler PNA is given in Table 1 and the positive phase convention was plotted in Fig. 2. I haven't checked them all but it appears that positive phases of all the modes have been plotted in Fig. 2 and that makes sense. This convention should be noted for future references in other chapters. [George Kiladis, USA]	Accepted. Explicit phase definition for PC-based indices is included. The consistency with Chapter 14 is checked.
2-2470	2	70	40	71	30	As the last paragraph of Box 2.4 seems to suggest, explaining "Patterns and Indices of Climate Variability" is problematic. I'm not sure this Box, or the associated figure, help clarify this issue. It's easy to imagine this box being used as part of an argument that seeks to undermine the credibility of long-term climate trends, especially since the Chapter reports a lot of trends over relatively short (<50 yr) periods,. I wish I could suggest a more compelling way to handle this section, but I don't have a clear idea. However, if the current review does not help improve this part of the chapter, and its relation to the rest of the chapter, later reviews could raise issues. [Dian Seidel, USA]	Rejected. This box is meant to provide common definitions for traditional climate variability indices throughout the report and thus to avoid the cases when the same index in different chapters is computed in different ways. The fact that there are many different indices for the same climate phenomena and that they might have different trends is a reality that has to be taken into account if reliable inferences about long-term climate change are to be obtained.
2-2471	2	70	45			why linear trends? There is no expectation of linear trends. [Kevin Trenberth, USA]	Rejected. Assuming that reviewer was referring to line 50, linear trends were used as a parsimonious way to account for secular change in timeseries.
2-2472	2	70		74		This section feels quite weak relative to the other material in the chapter. It is a rather cursory review and I'm not sure what it is trying to tell me. Given the variability in variability I wonder if it needs some references to the paleo-chapter which in theory could provide longer records. (I know there has been quite a lot done on paleo-enso so that perhaps could be used to provide context). [Simon Tett, United Kingdom]	Rejected. Sec 2.6.8. simply identified those of Box 2.4 climate indices that manifest significant linear trends towards present and assessed available peer-reviewed literature regarding their behavior. Research in paleoclimate is outside of the scope of Chapter 2.
2-2473	2	71	9	71	28	Consider adding the summer NAO index here as in: Folland, C.K., Knight, J., Linderholm, H.W., Fereday, D., Ineson, S. and J.W. Hurrell, 2009: The summer North Atlantic Oscillation: past, present and future. J. Clim. 22,1082–1103, DOI: 10.1175/2008JCLI2459.1. The data are available up to date from the Hadley Centre, e.g. via David Parker. Also consider adding the Parker et al AMO index as in: Parker, D.E., Folland C.K., A.A. Scaife, A. Colman, J. Knight, D. Fereday, P. Baines and D. Smith, 2007: Decadal to interdecadal climate variability and predictability and the background of climate change. J. Geophys. Res. (Atmos), 112,.D18115 doi 10.1029/2007JD008411. You already include the IPO index from this paper. The data are also available up to date from the Hadley Centre. If data need to be permanently available externally with url, this can be arranged. Note that the Folland et al (2009) Summer NAO paper is already in Chapter 2 references, but seemingly not yet used. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected: SNAO is already described in Table 1 of Box 2.4. Presenting a separate line for it (or a pattern) in Box 2.4 figures is not warranted, b/c these would be quite similar to winter NAO illustrations. PC-based AMO definition as introduced by Parker et al. 2007 (3rd PC of the global LF SST or NMAT field, responsible for ~2% of the total variance) is expected to be less robust than the AMO definitions that were already given in Box 2.4 Table 1 and thus does not need to be added unless specifically used in this definition by other chapters of the assessment report.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2474	2	71	14	71	17	Specify the dates of the pressure data underlying Box 2.4 Figure 2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial: has been specified in the caption of Figure 2.
2-2475	2	71	21	71	22	The indices do not define climate phenomenon, they are sometimes measures of the phenomenon. [Gareth S Jones, UK]	Accepted: sentence removed.
2-2476	2	71	23	71	23	of → in [Peter Burt, UK]	Editorial
2-2477	2	71	23	71	23	insert 'the' after 2nd 'of' [Peter Burt, UK]	Editorial
2-2478	2	71	27	71	27	insert comma after 'Thus' [Peter Burt, UK]	Editorial
2-2479	2	71	33	74	7	This attempt to quantify trends in indices of climate variability does not seem well-integrated with the rest of the chapter, in which climate trends are reported without much reference to these modes. If the modes are trending, and if physical climate parameters are also trending, then it seems reasonable to discuss the links between the modes and the physical parameters. As in the last comment, I don't have a helpful suggestion for a better approach, but I feel this approach opens the chapter to potential criticism. [Dian Seidel, USA]	Accepted. Re-edited for a better integration with the rest of the chapter.
2-2480	2	71	36	71	38	Other external forcing factors as well as anthropogenic influence changes in index values. [Gareth S Jones, UK]	Accepted - sentence removed.
2-2481	2	71	43	71	43	Did you define the acronym "SAM" previously? [Robert Waterland, United States of America]	Editorial
2-2482	2	71	47	74	7	The indices suggested above should be included in the Table 2.14 trends and in the discussion of Table 2.4. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected. See 2 -2473.
2-2483	2	71	48	71	48	What isthe noral state (value?) of the NAO? [Peter Burt, UK]	Rejected. The normal state of the NAO is 0, since NAO is defined through MSLP anomalies.
2-2484	2	71	48	71	48	quantify 'very low' value [Peter Burt, UK]	Rejected: obvious from the NAO panel in Box 2.4 Figure 1.
2-2485	2	71	50	71	50	Is "PC" an acronym for Principle Component? [Robert Waterland, United States of America]	Editorial
2-2486	2	71	55	71	56	AAO trends are said to be significant with 1% level of significance. That number should then be bold in Table 2.14. [Birgit Hassler, USA]	Accepted. Corrections made.
2-2487	2	71	58	72	3	The claim here that the AMO, defined as a residual of detrended SST "has significant…hemispheric climate impacts" has been strongly disputed in the peer reviewed literature. Studies by Trenberth [Trenberth, K. E., and D. J. Shea (2006), Atlantic hurricanes and natural variability in 2005, Geophys. Res. Lett., 33, L12704, doi:10.1029/2006GL026894] and Mann and Emanuel [2005] argue that efforts to impute temperature impacts based on simple linear detrending of SSTs suffer from a misidentification of forced variability (which is not linear in time) as internal variability. The original definition of the AMO e.g. Delworth and Mann (2000) [Delworth, T.L., Mann, M.E., Observed and Simulated Multidecadal Variability in the Northern Hemisphere, Climate Dynamics, 16, 661-676, 2000; see also references therein] use a more nuanced approach to defining the AMO. They find a heterogoenous spatial pattern of warming and cooling that largely cancels in the hemispheric mean. Modeling work by Knight et al [2005] supports this view, finding a very weak projection of the true AMO signal (no more than 0.1C) on to hemispheric mean temperature [Knight, J.R., Allan, R.J., Folland, C.K., Vellinga, M., Mann, M.E., A Signature of Persistent Natural Thermohaline Circulation Cycles in Observed Climate, Geophysical Research Letters, 32, L20708, doi: 10.1029/2005GL02423, 2005].This paragraph must be revised to reflect the disputed nature of the assertion made here. [Michael Mann, USA]	Taken into account. Offending sentence is rewritten. For the discussion of physical meaning of AMO and other climate modes readers are refered to Ch.14.
2-2488	2	71	58	72	3	Please include references (model and/or observational studies) for the statement that the AMO "has significant regional and hemispheric climate impacts". References could include the following (1) Zhang and Delworth,GEOPHYSICAL RESEARCH LETTERS, VOL. 33, L17712, doi:10.1029/2006GL026267, 2006. (2) Latif and Keenlyside, Deep Sea Research Part II: Topical Studies in Oceanography, 2011, Volume: 58, Issue: 17-18, 1880-1894. [Chris Roberts, Uk]	Taken into account, rewritten. See also 2-2487.

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2-2489	2	71	58		59	"significant regional" aspects are purely a consequence of the detrending. See Trenberth and Shea 2006 [Kevin Trenberth, USA]	Taken into account, rewritten, importance of detrending procedure is acknowledged. See also 2-2487.
2-2490	2	71	59	71	59	insert 'the' after 'called' [Peter Burt, UK]	Editorial
2-2491	2	72	1	72	1	superscript 'th' [Peter Burt, UK]	Editorial
2-2492	2	72	1	72	2	"Warm AMO phases occurred during the late 19th century". It should be noted that the presence of a warm phase in the 19th century and the magnitude of warm vs cold AMO anomalies depends on the method used to remove the influence of greenhouse warming from Atlantic SSTs (for example, see Trenberth and Shea, GEOPHYSICAL RESEARCH LETTERS, VOL. 33, L12704, doi:10.1029/2006GL026894, 2006). [Chris Roberts, Uk]	Taken into account, rewritten, importance of detrending procedure is acknowledged. See also 2-2487.
2-2493	2	72	3	72	3	AMO trend is stated to be significant for the time period 1979-present, but it is not given in bold in Table 2.14. [Birgit Hassler, USA]	Accepted. Corrections made.
2-2494	2	72	5	72	6	The PDO has been positive not negative since the phase shift in 1976/77. [David Sauchyn, Canada]	Rejected: (mean) values are positive, but the trend is negative
2-2495	2	72	5			“phase shifts” is more precise than “changes” [David Sauchyn, Canada]	Rejected: "changes" here are used in a sense of "trend", which is not the same as "phase shift".
2-2496	2	72	6	72	7	"This change,….is…" --> "These changes,…are…" [Hai Lin, Canada]	Editorial
2-2497	2	72	7	72	7	Should "reverting" be changed to "reversing"? [Dian Seidel, USA]	Editorial
2-2498	2	72	7	72	7	Need to introduce the concept of teleconnection. Replace "reverting" with "a reversal of previous". [Robert Waterland, United States of America]	Accepted. The reference is added to Box 2.4 (became Box 2.5 in SOD), where the concept of teleconnection is introduced.
2-2499	2	72	9	72	9	"seems to have sped up" seem to be too subjective a statement for this report. [Dian Seidel, USA]	Rejected. See 2-803.
2-2500	2	72	9	72	12	What does "s.d." stand for? [Gareth S Jones, UK]	Editorial: standard deviation.
2-2501	2	72	10	72	10	insert 'the' after 'at' [Peter Burt, UK]	Editorial
2-2502	2	72	11	72	11	suggest "with the reduced effective sample size due to autocorrelation" [Hai Lin, Canada]	Editorial
2-2503	2	72	14			this result depends on the data source/analysis. [Kevin Trenberth, USA]	Rejected: unclear what the reviewer means, specifically, here (aside from the fact that, to some extent, everything always "depends on the data source/analysis")
2-2504	2	72	17	72	30	Reference to Figure 2.5 would be useful here. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference added.
2-2505	2	72	17		30	Not an assessment. For instance Deser et al shows how some analyses are wrong. [Kevin Trenberth, USA]	Rejected: unclear what the reviewer means, specifically, here.
2-2506	2	72	23	72	23	Bunge and Clarke (2009) do not explicitly consider east-west gradients of SST across the Pacific. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence re-written.
2-2507	2	72	28	72	28	What is meant by "formal veracity"? [Dian Seidel, USA]	Editorial: meant to mean "formally correct"
2-2508	2	72	29	72	29	"…suggests…" [Hai Lin, Canada]	Editorial
2-2509	2	72	33			Instead of "present", please give the ending year of the trend periods in the table headings. It's nice that these periods match those used in some other sections of the chapter [Dian Seidel, USA]	Rejected. Ending years are given in the caption.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2510	2	72	36	72	38	"Trend values that are different from zero in the two-sided Student's t tests with 5% and 1% significance level are underlined and shown in bold, respectively" Either bolding was missing from the Table reproduction or there were no data meeting the 1% criterion - in which latter case the comment should be omitted. [Philip Lloyd, South Africa]	Accepted. Corrections made.
2-2511	2	72				Table 2.14: In the Table caption it is stated that trend values which are different from zero are either underlined or printed in bold. In the whole table, however, there is no bold number. [Birgit Hassler, USA]	Accepted. Corrections made.
2-2512	2	73	1			In Table please don't use AAO, rather please use SAM. [Kevin Trenberth, USA]	Rejected. A justification for this suggestion is unclear.
2-2513	2	73	3	73	25	I was surprised to see no reference to Wolter's Multivariate ENSO index included in the paragraph. [Michael Brewer, United States of America]	Rejected. Only univariate indices were used here.
2-2514	2	73	3	73	25	This seems like too much detail on ENSO. [Dian Seidel, USA]	Accepted. The discussion has been shortened.
2-2515	2	73	5	73	6	Significant trend of the Indian Ocean Basin Mode reported here should be formatted as bold in Table 2.14. [Birgit Hassler, USA]	Accepted. Corrections made.
2-2516	2	73	10	73	10	Consider adding a reference (perhaps Rasmusson, E. M., and J. M. Wallace, 1983: Meteorological aspects of El Niño / Southern Oscillation. Science, 222, 1195-1202.) for the "canonical" El Nino. [Dian Seidel, USA]	Accepted. A reference for "canonical" El Nino events is added.
2-2517	2	73	10		25	The evidence from earlier studies, such as Trenberth et al 2002: Trenberth, K. E., J. M. Caron, D. P. Stepaniak, and S. Worley 2002: The evolution of ENSO and global atmospheric surface temperatures J. Geophys. Res., 107, D8, 10.1029/2000JD000298. is that much of these structures relate to different phases of events and its evolution. This may relate also to how strongly locked in ENSO is to the annual cycle. There are other considerations of evolution and modes of ENSO when the 3D structure is considered such as in Trenberth, K. E., and L. Smith, 2009: Variations in the three dimensional structure of the atmospheric circulation with different flavors of El Niño. J. Climate, 22, No. 11, 2978-2991, doi: 10.1175/2008JCLI2691.1. This section also needs to be reconciled with Chapter 14. [Kevin Trenberth, USA]	Noted: further published details of ENSO events' evolution. Accepted: checked consistency with Chapter 14.
2-2518	2	73	12	73	12	Add the word "events" after "El Niño". [Robert Waterland, United States of America]	Editorial: does not seem necessary.
2-2519	2	73	27	74	7	In AR4 we could not justify including the PSA as it was not well defined. As noted here "it depends on the index". However see Chapter 14 also, Fig 14.10 for example. [Kevin Trenberth, USA]	Noted
2-2520	2	73	33	73	33	"its indices" [George Kiladis, USA]	Editorial
2-2521	2	73	33	73	33	"…it indices…" --> "…its indices…" [Hai Lin, Canada]	Editorial
2-2522	2	73				It is not that meaningful to present the trends in AMO in Table 2.14. AMO is defined for describing multi-decadal oscillations in the Atlantic SST. Though AMO index is obtained by averaging SST over the North Atlantic basin (0-70N), it is often used after the linear trend is removed in the averaged SST. In this table, it would be fine if averaged SST over 0-70N of the Atlantic basin is used. [Zhaomin Wang, UK]	Rejected: linear trend is subtracted for 1870-2010 period, the table reports trend slopes for shorter periods.
2-2523	2	74	3	74	3	Replace "the the weakening" with "new weakening" [Mihai Dima, Romania]	Editorial
2-2524	2	74	3	74	3	"the" appears twice [Celeste Saulo, Argentina]	Editorial
2-2525	2	74	3	74	3	the' is redundant. [Zhaomin Wang, UK]	Editorial
2-2526	2	74	6	74	6	"…its consequences…" --> "…its consqeuence…" [Hai Lin, Canada]	Editorial
2-2527	2	74	11	74	23	To a large extent, it seems that the new and improved data sets used for looking at changes in large scale circulation are output from reanalyses. I'm not sure how good these are for trends in the troposphere (where there is a lot of data input), but there are some pretty severe problems in the stratosphere related to change in amount of input data, shifts in streams that models are run over. I think there should be some sort of statement regarding confidence in trends estimated from reanalyses. [Karen Rosenlof, United States of America]	Rejected - Reanalyses are discussed in a Box. Care is taken with any statement concerning trends from reanalysis data.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2528	2	74	15	74	15	circulation → Circulation [Peter Burt, UK]	Editorial
2-2529	2	74	19	74	23	It is interesting to study changes in indices of climate variability. It would even be more exciting to discuss the new literature about changes in the correlation between different modes (e.g. between ENSO and NAO) or changing regimes. [Wanner Heinz, Switzerland]	Noted. This is partly covered in Ch. 14.
2-2530	2	74	23	74	23	Replace "have been" with "were". [Robert Waterland, United States of America]	Editorial
2-2531	2	74	25	76	34	Chapter 2 needs to provide clear definition of extremes for other chapters to follow. We should discuss at the 3rd LA meeting on this issue. Can we think about three kind of "extreme indices". One is something absolute, such as annual maximum of daily maximum tempertaure that engineers can use to derive design values. One is relative to its long-term climatology (such as percentiles indices). And one is also relative but thresholds have some meaning for impacts such as 25C? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: Cross-chapter meeting held at LA3 to discuss and agree proposed list of indices to include. For various reasons however it was impossible to analyse exactly the same indices across the chapters focussing on these types of extremes but we have attempted to find as much overlap as possible.
2-2532	2	74	25	76	35	If extremes in the hydrological cycle are to be inlcuded in chapter 2.7 there is a need to inlcude flood and hydrological drougth or low flow indices in Box 5.2. [Hege Hisdal, Norway]	REJECTED: In comparison to the SREX report we have very little space to cover all aspects of the hydrological cycle. We have therefore chosen to limit our assessment to "climate extremes" in the Box on indices which we feel we can cover in the space provided to the required level of detail.
2-2533	2	74	25	76	35	The introduction to "extreme events" described here is conceptually incomplete. Time scales of metrics associated with extreme "events" are on the order of hours to days." The conceptualiztion for drought is more vague. An explicit discussion - on the order of a box - discussing the potential spatial and temporal variation of extremes would be valuable. This topic is highlighted more strongly in AR5 in part as a result of SREX. Some of that interest has stemmed from the notion that the public is more affected by extremes, but the truth is that ecosystems and economies are more affected by trends in tails of distributions than by trends in the mean, per se. An example is trends in the distribution of mean annual streamflow - its an annual scale event, but it is still something for which the distribution can change with dramatic effects on ecology and economy. There are other metrics with longer time footprints and with a range of spatial footprints (which may be changing scale!) that should be conceptually addressed even if not extant in the literature. [Charles Luce, United States of America]	REJECTED: The spatial and temporal variation of extremes is covered quite rigorously by SREX and therefore we feel that the addition of a box would be unessessary repetition. Also unlike SREX we have much more limited space for our assessment of extremes and therefore it is impossible to cover all aspects to the level of detail we would like given space constraints. As an example SREX Chapter 3 had over 70 pages for this purpose, we only have 6 pages. Our purpose is also to assess the peer-reviewed literature and for that reason if something is "not extant in the literature" then it is not something that belongs within this assessment.
2-2534	2	74	25	76	35	Referencing back to figure 1.9 in the introduction to AR5 WG1 would be useful somewhere in this introduction. [Charles Luce, United States of America]	TAKEN INTO ACCOUNT: We update the conceptual probability density functions that have appeared in previous IPCC reports using real observations and these PDFs are now included in FAQ2.2. We now make reference to this FAQ in the introduction.
2-2535	2	74	25	76	35	Fits to the mean of observed extremes is one method to examine trends in extremes. Several examples are included in this section. For example, one could take the annual peak streamflow and see if there is a trend in that value. To a hydrologist, this is an examination of trends in the size of the "mean annual flood." Other metrics may be of more interest, e.g. the trend in the 2-year flood, or the 10-year flood. Detecting changes in those can best be done using a tool called quantile regression, but this has only been applied in a few hydrologic studies, and only one related to trends over time. This section should acknowledge the general statistical framing that has been done to examine the problem of extremes and the examples of ongoing changes in the analysis of data to better frame results in the context of Figure 1.9. [Charles Luce, United States of America]	TAKEN INTO ACCOUNT: The revised text in Section 2.6 we feel now better reflects the statistical advances that are being made within the climate literature in recent years particularly through the use of extreme value theory.
2-2536	2	74	25	86	31	Further to my comments above, may I suggest a box somewhere in Section 2.7 which explains the 'temperature scaling' hypothesis whereby extreme precipitation is suggested to increase by the moisture holding capacity of the atmosphere governed by the Clausius-Clapeyron equation. Can describe the	TAKEN INTO ACCOUNT: Some discussion of temperature scaling hypothesis is now included. However the majority of the discussion takes place in

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						assumptions underpining this hypothesis (e.g. constant relative humidity, constant vertical velocities) and arguements in favour and against this hypothesis (e.g. decrease in relative humidity over much of the land, the fact that observational evidence is showing different scaling rates depending on the length of the storm). The Clausius-Clapeyron/thermodynamic scaling idea shows up repeatedly in various chapters of this report, and it would be good to clearly articulate what is meant by the idea. [Seth Westra, Australia]	Ch. 7 and we thus reference the appropriate section of this chapter in the text. We do not feel that it is the remit of Chapter 2 to include a box on this subject.
2-2537	2	74	27	75	3	This background information on extreme events can probably be shortened. [Dian Seidel, USA]	ACCEPTED. This section of the chapter has been re-written and shortened.
2-2538	2	74	29	74	30	Does SREX actually stand for anything? [Gareth S Jones, UK]	ACCEPTED: Yes the Special Report on Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation. We now spell out the acronym.
2-2539	2	74	47		57	There seems to be an implicit assumption that events have something at least vaguely gaussian in there distribution. Is this a generally accepted fact or is it possible that they can naturally have a non-symetric distribution? [Larry Thomason, United States of America]	TAKEN INTO ACCOUNT: We do not believe that there is an implicit assumption that these events are Gaussian. In fact most of the references use methods that do not assume an underlying statistical distribution. The introduction to this section has however been revised and we hope that this implication is not present in the revised text.
2-2540	2	74	51	74	55	Replace "2012) with many regional studies indicating that the changes observed in the frequency of extremes can be explained or inferred by shifts in the overall probability distribution of the climate variable (Ballester et al., 2010; Griffiths et al., 2005; Simolo et al., 2011). However, note that these studies refer to counts of threshold exceedance (frequency, duration) which closely" with "2012). Numerous regional studies indicate that the changes observed in the frequency of extremes can be explained or inferred by shifts in the overall probability distribution of the climate variable (Ballester et al., 2010; Griffiths et al., 2005; Simolo et al., 2011). However, it should be noted that these studies refer to counts of threshold exceedance - frequency, duration - which closely". [Robert Waterland, United States of America]	EDITORIAL:
2-2541	2	74		82		As mentioned below, it would be valuable to explain variation in extreme temp and precip events (section 2.7), where coastal and mountain areas are generally separated from latitudinal averages, which are dominated by continental conditions. For example, there is one sentence on line 47 that states no statistically significant changes in extreme precip events in coastal areas in N America. More could be said on results for these geographic areas (mountains, coastal) globally. [Beverly Law, USA]	TAKEN INTO ACCOUNT: This level of regional detail is more appropriately covered by Ch 14
2-2542	2	74				Section 2.7 Changes in Extreme Events focuses on climate variables and little is said about hydrological variables like runoff. For instance Box 2.5 Extremes Indices does not include any high or low flow indices (or similar flood or drought indices). For instance, the commonly used annual 7-day maxima (high flow) and minima (low flow) indices could be added. [Lena M. Tallaksen, Norway]	TAKEN INTO ACCOUNT: An assessment of stream flow and run-off indices are included in Section 2.5.2.
2-2543	2	75	1	75	3	Why assess the conclusions of AR4? I thought AR5 should assess the progress after AR4! I do not see the benefit of assessing AR4 again. [Uwe Stoeber, Germany]	REJECTED: We feel that it is beneficial to the reader to state up front what the AR4 conclusions were so that it is clear when we make the conclusions in AR5 whether our assessment has changed or not.
2-2544	2	75	8	76	35	This box is useful and consistent with the material from the recent chapter 3 of the IPCC SREX report, which is also referred to at the beginning of this box (although an indication of the chapter would be useful). Nonetheless, I saw that a few sentences were taken verbatim from Box 3.1 of that chapter (e.g. page 75/line 33-page 76/line1). It would be good that the text in such instances be slightly more paraphrased or else explicitly quoted. [Sonia Seneviratne, Switzerland]	ACCEPTED: This Box has been updated and verbatim quotes from SREX have been removed.
2-2545	2	75	12	75	13	"do not represent indices that might be related to extreme evrnts, e.g., NINO3" This sentence is imprecise and doesn't clarify the difference between, say, NINO3 and TX90. [Douglas Maraun, Germany]	ACCEPTED: This sentence has been rewritten to clarify this.
2-2546	2	75	13	75	13	"related to extreme events e.g., NINO3" should have the comma moved to after events: "related to extreme	EDITORIAL:

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						events, e.g. NINO3" [Richard Heim, U.S.A.]	
2-2547	2	75	13	75	13	Does NINO3 really need to be mentioned in this box? [Dian Seidel, USA]	REJECTED: We feel it is important that the reader is not confused by the term "index" to mean a climate index such as NINO3. However it is clear from other comments that we failed to do this in the FOD and we have rewritten this sentence accordingly to clarify this.
2-2548	2	75	13	75	20	Please mention and make reference to the extreme index development effort of the Expert Team on Climate Change Detection, Monitoring and Indices (ETCCDMI) working under the joint WMO Commission for Climatology (CCl)/World Climate Research Programme (WCRP) Climate Variability and Predictability (CLIVAR) project. References : - Peterson, T. C., and Coauthors, 2001: Report on the Activities of the Working Group on Climate Change Detection and Related Rapporteurs 1998-2001. WMO, Rep. WCDMP-47, WMO-TD 1071, Genevor, Switzerland, 143 pp. - Peterson, T.C., 2005: Climate change indices. WMO Bulletin, 54(2), 83-86. [Tsz-cheung Lee, Hong Kong]	REJECTED: This group is now known as the WMO CCl/CLIVAR/JCOMM Expert Team on Climate Change Detection and Indices (ETCCDI) and we have included many more recent references to this group throughout the rest of the text e.g. Zhang et al. 2011which discusses the history and development of the Expert Team and the indices, providing a much more up to date summary of the work of this group.
2-2549	2	75	13			How is NINO3 an “extreme event”? This sentence is unclear. [David Sauchyn, Canada]	ACCEPTED: This sentence has been rewritten to clarify that we are referring to 'extreme' phases of the ENSO phenomenon not the NINO3 index itself.
2-2550	2	75	16	75	16	"Other definitions relate to" should have a comma after to: "Other definitions relate to," [Richard Heim, U.S.A.]	EDITORIAL:
2-2551	2	75	18	75	20	"Box 2.5, Table 1 lists some of the common definitions for indices that are widely used in the scientific literature and for which near-global datasets exist." Most of those listed in Box 2.5 are based on threshold-excess concepts, but block maxima concepts (e.g. the annual maximum precipitation event) are also commonly analysed since simple statistical theories exist to model non-stationary extremes (e.g. Coles, 2001, "An Introduction to the Statistical Modelling of Extreme Values"). [Seth Westra, Australia]	NOTED: While this is true, nearly all studies using block maxima only focus on local or regional scale analysis.
2-2552	2	75	20	82	11	"Extreme indices are more generally defined for (daily) temperature and precipitation characteristics, and are rarely applied to other weather and climate variables, such as wind speed, humidity, or physical impacts and phenomena." True enough, but recently there have recently been a large number of papers published which deal with sub-daily precipitation which has not been covered in this chapter. Sub-daily precipitation is an extremely important variable from a human impacts perspective (e.g. Berne et al, 2004), and also many of the extreme precipitation phenomena such as convection occurs only over a small portion of the day. Furthermore, all the references below show that trends in sub-daily precipitation are not necessarily consistent with trends in daily precipitation. Therefore a paragraph or two on this should be included in this chapter, potentially in Section 2.7.2. Some research on this includes the 'temperature scaling papers' (papers which look at empirical relationships between extreme precipitation and atmospheric temperature, and include Lendernk and Van Meijgaard (2008), Haerter and Berg (2009), Hardwick-Jones et al (2010) and Utsumi et al (2011)), and the 'trend' papers indlucing Westra and Sisson (2011) and Jakob et al (2011) for Australia, Lenderink et al (2011) for Hong Kong and the Netherlands, and a number of additional papers cited in section 2.1 of Willems et al (2012). References given below. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: We agree that sub-daily precipitation should be discussed more within section 2.6 although we note that there are still very limited studies on anything other than small regional scales. However we do now make an assessment based of the available literature.
2-2553	2	75	20	82	11	Berne, A., G. Delrieu, J.-D. Creutin, and C. Obled (2004), Temporal and spatial resolution of rainfall measurements required for urban hydrology, Journal of Hydrology, 299, 166-179. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2554	2	75	20	82	11	Haerter, J. O., and P. Berg (2009), Unexpected rise in extreme precipitation caused by a shift in rain type?, Nature Geoscience, 2, 372-373. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2555	2	75	20	82	11	Hardwick-Jones, R., S. Westra, and A. Sharma (2010), Observed relationships between extreme sub-daily precipitation, surface temperature and relative humidity, Geophysical Research Letters, 37(L22805). [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2556	2	75	20	82	11	Jakob, D., D. J. Karoly, and A. Seed (2011), Non-stationarity in daily and sub-daily intense rainfall - Part 2: Regional assessment for sites in south-east Australia, National Hazards and Earth Systems Science, 11, 2273-2284. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.

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2-2557	2	75	20	82	11	Lenderink, G., and E. van Meijgaard (2008), Increase in hourly precipitation extremes beyond expectations from temperature changes, Nature Geoscience, 1, 511-514. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2558	2	75	20	82	11	Lenderink, G., H. Y. Mok, T. C. Lee, and G. J. Van Oldenborgh (2011), Scaling and trends of hourly precipitation extremes in two different climate zones - Hong Kong and the Netherlands, Hydrological Earth Systems Science, 8, 4701-4719. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2559	2	75	20	82	11	Utsumi, N., S. Seto, S. Kanae, E. E. Maeda, and T. Oki (2011), Does higher surface temperature intensify extreme precipitation?, Geophysical Research Letters, 38(L16708). [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2560	2	75	20	82	11	Westra, S., and S. A. Sisson (2011), Detection of non-stationarity in precipitation extremes using a max-stable process model, Journal of Hydrology, 406, 119-128. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2561	2	75	20	82	11	Willems, P., K. Arnbjerg-Nielsen, J. Olsson, and V. T. V. Nguyen (2012), Climate change impact assessment on urban rainfall extremes and urban drainage: Methods and shortcomings, Atmospheric Research, 103, 106-118 [Seth Westra, Australia]	TAKEN INTO ACCOUNT: See above comment.
2-2562	2	75	21	75	21	Box 2.5 text needs balancing with one or two key references on temperature and rainfall indices. Suggest the key IPCC 2007 reference be added here, Alexander et al, 2006, as in your reference list at line 24, p88 [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: More recent references are now included which update the Alexander et al. 2006 study.
2-2563	2	75	25	75	25	Could add an earlier reference to indices of temperature/humidity extremes: Gaffen, D.J., and R.J. Ross, 1998: Increased summertime heat stress in the United States. Nature, 396, 529-530 [Dian Seidel, USA]	REJECTED: Our intention here is to update the findings of AR4 and SREX and therefore we only include references prior to 2006 if absolutely necessary.
2-2564	2	75	25	75	27	Indices based on simulated soil moisture should in my opinion be mentionned here, like soil moisture percentiles (Sheffield et al., 2007,2008, 2009) and Standardized Soil Wetness Index (SSWI, Vidal et al., 2010). [Jean-Philippe Vidal, France]	TAKEN INTO ACCOUNT: Our intention is to cover "climate indices" in this box that are widely used in the climate community as stated in the introductory paragraph. For this reason we mostly only consider indices that are solely based on temperature and precipitation.
2-2565	2	75	25	75	27	References for the comments on this paragraph: - Sheffield, J., K. M. Andreadis, E. F. Wood, D. P. Lettenmaier, 2009: Global and Continental Drought in the Second Half of the Twentieth Century: Severity–Area–Duration Analysis and Temporal Variability of Large-Scale Events. J. Climate, 22, 1962–1981. doi: http://dx.doi.org/10.1175/2008JCLI2722.1 - Vidal, J.-P., Martin, E., Franchistéguy, L., Habets, F., Soubeyroux, J.-M., Blanchard, M., and Baillon, M., 2010: Multilevel and multiscale drought reanalysis over France with the Safran-Isba-Modcou hydrometeorological suite, Hydrol. Earth Syst. Sci., 14, 459-478, doi:10.5194/hess-14-459-2010 [Jean-Philippe Vidal, France]	TAKEN INTO ACCOUNT: These references have now been assessed by the chapter authors in Section 2.6.2.2
2-2566	2	75	26	75	27	You are using the SPI and SPEI from Vincente-Serrano et al 2010a. This dataset uses CRUTS 3.1 which you've taken out of the Figures in the precipitation section. You took it out because there isn't yet a paper submitted (hope to rectify this), but used a paper that has used it! [Philip JONES, UK]	NOTED:
2-2567	2	75	30	75	30	in Bos 2.5, Table 1, for the Dryness Index, PDSI and SPI are defined but SPEI isn't. SPEI needs to be defined (I assume Standardized Precipitation Evapotranspiration Index). [Richard Heim, U.S.A.]	ACCEPTED: SPEI has now been defined
2-2568	2	75	30	75	30	Box 2.5 gives definitions for extreme indices for some climate variables. It will be useful to include similar indices for high and low flows in a river. I don't have access to the SREX report - perhaps such indices have been deifned therein. For high flows, those with exceedance proability of 95 % can be considered. For low flow, value at the other end of the PDF, say Q95, may be considered. [Sharad K Jain, India]	TAKEN INTO ACCOUNT: The focus of this box is primarily on the types of indices that are widely used within the climate community as stated in the opening paragraph. An assessment of stream flow and run-off indices are included in Section 2.5.2.
2-2569	2	75	30	75	30	One should perhaps not consider PDSI/SPI/SPEI as extreme indices in a way one don't consider annual mean temeprature as an extreme temperature index since they all include normal years. [Xuebin Zhang, Canada]	REJECTED: These are measures of drought and fall under the category of "climate indices" which are commonly used within the climate community as we

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							define it in this box.
2-2570	2	75	30			There is inadequate discussion of "heavy precipitation". Most papers use daily data yet most of the time it does not precipitate and to do precipitation properly requires hourly data. The result of using daily is a very flawed and fuzzy picture. This sort of comment should be made. [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT: Most of the discussion is around daily precipitation because that is what most of the literature contains. We agree that some discussion should be made of sub-daily precipitation but to date there is practically no literature on a large enough spatial scale for us to make an apprropriate assessment. More discussion on sub-daily precipitation is included in Ch 7 and Ch 14.
2-2571	2	75	34	75	36	How are derived indices “easier to obtain” than daily temperature and precipitation data, when most climate indices are derived from temperature and precipitation data? [David Sauchyn, Canada]	TAKEN INTO ACCOUNT: Most countries are willing to exchange indices but not daily data. Therefore they calculate the indices themselves based on daily data and exchange those. This is why it is easier to obtain indices. This has been made clearer in the text.
2-2572	2	75	37	76		Should the first `extreme' be in inverted commas in `extreme "extremes"' to be consistent with "moderate" used earlier in previous sentence? [Gareth S Jones, UK]	EDITORIAL
2-2573	2	76	1	76	3	"better investigated" is a bit weak. For most applications it is the only way to robustly determine very extreme extremes, but the advantages are much more than this. EVT characterises the whole tail. Where it sits releative to the body of the distribution and its shape ("heavy" or "light" tailed). Because it looks at all the data in the tail (magnitude and probability) it has much better signal to noise characteristics than other commonly used methods. It also can be applied reletively easily to a non-stationary changing climate. HadEX is an excelent dataset but it is severely limited because it looks at very specific extreme indicied (eg days over a threshold). Significant progress for AR6 will only be achieved if we begin to form observational datasets that characterise the whole tail. EVT would seem an obvious tool. [Simon Brown, UK]	NOTED:
2-2574	2	76	1			I suggest “rare extremes” rather than “extreme extremes” [David Sauchyn, Canada]	ACCEPTED: This sentence now begins "Rarer extremes.."
2-2575	2	76	2	76	3	"...and a growing body of literature is exploring its use with in the climate sciences". Should cite [Westra and Sisson, 2011, 'Detection of non-stationarity in precipitation extremes using a max-stable process model', Journal of Hydrology, 406, 119-128pp] as to my knowledge this is the only reference currently available which uses a full spatial extreme value model based on max-stable processes to evaluate changes in hydrological extremes. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: This reference has now been assessed by the chapter authors.
2-2576	2	76	3	76	3	Two excelent examples of EV application to future climate extremes, which should be referenced so that others can follow, are; Hanel, M., T. A. Buishand, and C. A. T. Ferro (2009), A nonstationary index flood model for precipitation extremes in transient regional climate model simulations, J. Geophys. Res., 114, D15107, doi:10.1029/2009JD011712. and Analysis of precipitation extremes in an ensemble of transient regional climate model simulations for the Rhine basin, M Hanel T A Buishand Clim Dyn (2011) 36:1135–1153 DOI 10.1007/s00382-010-0822-2 [Simon Brown, UK]	TAKEN INTO ACCOUNT: These references have now been assessed by the chapter authors.
2-2577	2	76	3			More references could be added, e.g., Sillmann,J.; M. Kallache, M.; Croci-Maspoli and R.W. Katz (2011): Extreme cold winter temperatures in Europe under the influence of North Atlantic atmospheric blocking. Journal of Climate, 24, 5899-5913; D. Maraun, T.J. Osborn and H.W. Rust: The influence of synoptic airflow on UK daily precipitation extremes. Part I: observed spatio-temporal relations, Clim. Dynam. 36(1-2), 261-275 [Douglas Maraun, Germany]	TAKEN INTO ACCOUNT: These references have now been assessed by the chapter authors.
2-2578	2	76	8	76	8	superscript 'th' (x2) [Peter Burt, UK]	EDITORIAL:
2-2579	2	76	9	76	9	5-day → 5 day [Peter Burt, UK]	EDITORIAL:

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2-2580	2	76	11	76	11	exdceedance → exceedence [Peter Burt, UK]	EDITORIAL:
2-2581	2	76	17	76	17	replace hyphen with full stop and capitalise 'An' [Peter Burt, UK]	EDITORIAL:
2-2582	2	76	22	76	23	The sentence, "In addition to the complication of defining an index, the way in which indices are calculated to create global averages for example also adds an additional complication to their calculation.", would read better if parantheses were added where indicated: "In addition to the complication of defining an index, the way in which indices are calculated (to create global averages for example) also adds an additional complication to their calculation." [Richard Heim, U.S.A.]	EDITORIAL:
2-2583	2	76	33	76	33	Fig. 1 (Box 2.5) what is the meaning of the areas without colours?: No-data? Significance? [Celeste Saulo, Argentina]	TAKEN INTO ACCOUNT: This figure and figure caption have been updated and areas with no data are now shown in grey.
2-2584	2	76	38	76	38	I am not sure 2.7.1 Temperature is a good title here, would Tempertaure extremes" be better? [Xuebin Zhang, Canada]	ACCEPTED: What is now Section 2.6.1 has been renamed
2-2585	2	76	38	82	11	Booth et al. (2012) have made a detailed analysis of changing extremes indices in western North America. Booth, E. L. J., Byrne, J. M. and Johnson, D. L. (2011), Climatic changes in western North America, 1950–2005. International Journal of Climatology. doi: 10.1002/joc.3401, in press in online “early view”. [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: The reference has been assessed by the chapter authors
2-2586	2	76	38			In this section also see Mahlstein et al 2011 ERL 6, doi:10.1088/1748-9326/6/3/034009 [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT: This reference is more appropriate for Chapter 10.
2-2587	2	76	40	79	2	This section is interesting but seems far too long compared with the terse text earlier. If cuts are made, I'd suggest they come section 2.7 [Philip JONES, UK]	ACCEPTED: This section has been reduced accordingly.
2-2588	2	76	45	76	45	Add after "based on more recently available evidence": "and using the revised AR5 uncertainty guidance". [Sonia Seneviratne, Switzerland]	ACCEPTED: amended
2-2589	2	76	49	76	49	Replace "exists" with "supports the conclusion". [Robert Waterland, United States of America]	ACCEPTED: amended
2-2590	2	76	49	76	57	A tabe would be easier to digest, or a graph as opposed to a listing of values within the paragraph. [Karen Rosenlof, United States of America]	ACCEPTED: A table has been added
2-2591	2	76	56	77	1	Here is another example of possible reporting of too many signficant figures in the trend and confidence interval values.. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: Values have been updated.
2-2592	2	76	56	77	2	The list of trend values would be less confusing if it would be explained upfront, which of the three values shown is based on which data set. Giving this information at the end of the phrase might be a bit late. [Birgit Hassler, USA]	ACCEPTED:A table has been added
2-2593	2	77	4	77	4	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2594	2	77	5	77	5	replace hyphen with : [Peter Burt, UK]	EDITORIAL:
2-2595	2	77	7	77	7	insert comma after second) [Peter Burt, UK]	EDITORIAL:
2-2596	2	77	14	77	20	From this caption for Fig. 2.41, it is not clear if the extremes are based on annual data or seasonal (or monthly, …). [Dian Seidel, USA]	ACCEPTED: Figure and caption has been updated accordingly
2-2597	2	77	22	77	23	States that increase in Tmin faster than Tmax in Duke and HadEX datasets. This does not make it clear whether this is not seen in HadGHCND, or whether this was not assessed for HadGHCND. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: The reference to the Duke dataset has now been removed as this dataset does not have data past 2005 (the HadEX dataset results have also been removed for this reason). We have updated the sentence and section accordingly.
2-2598	2	77	22	77	23	The phrase 'would have lead to a reduction' could be improved upon. As currently written, it does not indicate	TAKEN INTO ACCOUNT: DTR results are covered in

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						whether that actually occurred or not. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	Section 2.4.1.3
2-2599	2	77	22	77	24	This mention of DTR reduction does not seem to be consistent with the discussion of DTR trends earlier in the chapter. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: Discussion of DTR trends is now consistent with Section 2.4.1.3. However in this section we are referring to the fact that extreme minimum temperatures are increasing faster than extreme maximum temperatures and this is not the same metric as DTR.
2-2600	2	77	27	77	27	There is a comprehensive study on trends in extreme temperature indices for South America, which should be included here: Vincent, L.A., T.C. Peterson, V.R. Barros, M.B. Marino, M. Rusticucci, G. Carrasco, E. Ramirez, L.M. Alves, T. Ambrizzi, M.A. Berlato, A.M. Grimm, J.A. Marengo, L. Molion, D.F. Moncunill, E. Rebello, Y.M.T. Anunciação, J. Quintana, J.L. Santos, J. Baez, G. Coronel, J. Garcia, I. Trebejo, M. Bidegain, M.R. Haylock, D. Karoly, 2005: Observed trends in indices of daily temperature extremes in South America 1960-2000. Journal of Climate, 18, 5011-5023. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: This section has now been reduced significantly. Regional trends in extremes are also covered in Ch. 14. In addition there is a much more recent comprehensive study by Skansi et al., 2012 and this is referenced here instead.
2-2601	2	77	30	77	30	Replace "close to a doubling (or halving) of the occurrence of warm and cold nights" with "close to a doubling of the occurrence of warm and a halving of the occurrence of cold nights". [Robert Waterland, United States of America]	EDITORIAL:
2-2602	2	77	30	77	32	The first sentence of the paragraph ends with "..although this is not the case across all regions." So the reader expects to now get a list of exceptions, but instead is given more examples supporting the main point. Consider rephrasing so the paragraph is smoother. [Dian Seidel, USA]	ACCEPTED: This section has been re-written.
2-2603	2	77	35	77	36	There is a more comprehensive study on trends in extreme temperature indices for South America, which should be included here: Vincent, L.A., T.C. Peterson, V.R. Barros, M.B. Marino, M. Rusticucci, G. Carrasco, E. Ramirez, L.M. Alves, T. Ambrizzi, M.A. Berlato, A.M. Grimm, J.A. Marengo, L. Molion, D.F. Moncunill, E. Rebello, Y.M.T. Anunciação, J. Quintana, J.L. Santos, J. Baez, G. Coronel, J. Garcia, I. Trebejo, M. Bidegain, M.R. Haylock, D. Karoly, 2005: Observed trends in indices of daily temperature extremes in South America 1960-2000. Journal of Climate, 18, 5011-5023. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: This section has now been reduced significantly. Regional trends in extremes are also covered in Ch. 14. In addition there is a much more recent comprehensive study by Skansi et al., 2012 and this is referenced here instead.
2-2604	2	77	39	77	47	The cooling areas may be related to the moistening areas in Figure 2.18 (though that is specific humidity and the relevant variable is relative humidity) and Figure 2.42. [David Parker, United Kingdom of Great Britain & Northern Ireland]	NOTED
2-2605	2	77	42	77	42	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2606	2	77	51	77	52	I suggest “Record high daily maximum temperatures averaged across the USA now outnumber record low daily minimum temperatures by a ratio of 2:1” [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: amended
2-2607	2	77	51	77	53	This claim about record maximum/minimum temperatures is highly dependent on the selection of "recent" and "preceding" decades. Meehl's study began in 1950. If one begins with the decade of the 1930s, or the 1900s, the trend toward more record maximum temperatures disappears. US max/min temperture records go back to 1872 for many stations. [Richard Keen, USA]	TAKEN INTO ACCOUNT: Records dating back prior to 1950 are less robust and therefore we have made our conclusions based on this. Also it is not just in the US but this result has been observed across other regions (e.g. we also indicate a similar result for Australia). However, we have amended the sentence accordingly.
2-2608	2	77	54	77	54	large coherent trends: no doubts about the warming, but in the figures, this appears is true for cold nights and days, and for warm days and nights until 1985, while in the last 20 years the HadGHCND behavior diverges from that of the other datasets. The comment is on lines 4-9, but could be anticipated here. [Claudio Cassardo, Italy]	TAKEN INTO ACCOUNT: HadGHCND is now compared with datasets that are available over the same period so this divergence is less apparent, although it still exists. As noted we discuss the reasons in the text but feel that where it is currently discussed is the appropriate place for it.

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2-2609	2	77	55			The paper Wergen & Krug (2010), Record-breaking temperatures reveal a warming climate, Europhys. Lett 92(3) 30008, should be cited in this context. [Douglas Maraun, Germany]	TAKEN INTO ACCOUNT: This paper has been assessed by the authors.
2-2610	2	77	56	78	2	I suggest to organize the trends in a table, which will be more readible. [Claudio Cassardo, Italy]	REJECTED: Since there are 12 values in total we do not feel that a Table is warranted.
2-2611	2	78	7	78	8	This final sentence of the paragraph seems out of place with what preceeds it. [Dian Seidel, USA]	NOTED
2-2612	2	78	18	78	28	I would like, modestly, show to your attention also our study on summer 2003, in which it was investigated at local scale (and quantified) the effect of the soil moisture depletion as an exhacerbating factor of the heat wave (in addition to the anticyclonic conditions and their associated downward motions): C. Cassardo, L. Mercalli and D. Cat Berro (2007) "Characteristics of the Summer 2003 Heat Wave in Piedmont, Italy, and its Effects on Water Resources" Journal Of The Korean Meteorological Society, 43, 3, pp. 195-221 - available here (http://ccassardo.webuda.com/paperi/2007_characteristics_of_the_summer_heat_wave_in_piedmont_italy.pdf). [Claudio Cassardo, Italy]	TAKEN INTO ACCOUNT: This paper has been assessed by the authors.
2-2613	2	78	18			Please define what is “heat wave” in this Chapter ? [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: Warm spells are defined in Box 2.4 Table 1 and some description of heat waves in given in FAQ2.2. We have referred the reader to these sections to make our definition clearer.
2-2614	2	78	19	78	19	delete comma after 'advection' [Peter Burt, UK]	EDITORIAL:
2-2615	2	78	22	78	26	The argumentation would be clearer here if the text would highlight that the dry soil moisture conditions are either induced by precipitation deficits or evapotranspiration excesses, or the combination of both (from a simple consideration of the surface water balance). The higher evaporative demand and early vegetation onset are just two factors that can induce higher evapotranspiration. In addition, low cloudiness may not be the single factor inducing higher evaporative demand: wind speed, and the temperature and dryness of advected air are also important factors. [Sonia Seneviratne, Switzerland]	ACCEPTED:
2-2616	2	78	22	78	28	An additional feedback from soil moisture on hot extremes is related to the persistence of soil moisture anomalies, which can affect the persistence of hot spells as highlighted in Lorenz et al. (2010, GRL). Ref: Lorenz, R., et al. 2010, Geophysical Res. Letters, 37, L09703, doi:10.1029/2010GL042764 [Sonia Seneviratne, Switzerland]	ACCEPTED:
2-2617	2	78	22	78	28	On the role of advected air and possible non-local feedbacks, see also Vautard et al. (2007, GRL) and Haarsma et al. (2009, GRL). Ref: Haarsma, R.J., et al., Geophys. Res. Lett., VOL. 36, L04705, doi:10.1029/2008GL036617. [Sonia Seneviratne, Switzerland]	TAKEN INTO ACCOUNT: These papers have been assessed by the authors.
2-2618	2	78	23	78	23	Probably, Seneviratne et al. (2006, Nature) should also be cited here, since it was the first study to highlight that feedbacks between soil moisture and summer temperature extremes would be dependent on the climate regime (i.e. strongest in transitional climate regions and thus subject to shifts with climate change). Additionally, Seneviratne et al. (2010, Earth-Science Review) provides an extensive review on this topic. Refs: Seneviratne, S.I. et al., 2006, Nature, 443, 205-209; Seneviratne, S.I,, et al. 2010, Earth-Science Reviews, 99, 125-161. [Sonia Seneviratne, Switzerland]	TAKEN INTO ACCOUNT: These papers have been assessed by the authors.
2-2619	2	78	24	78	24	Delete “Vautard et al., 2010” which relates to trends in winds, not to heat-drought interaction. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference removed
2-2620	2	78	25	78	25	I don’t find the link involving reduced cloudiness and higher evaporative demand in Black and Sutton, 2007 whose mention of evaporation and cloudiness relates to the Mediterranean; and the link is only implicit in Fischer et al., 2007a. "Low-cloudiness" would be better expressed as "reduced cloudiness" to avoid confusion with low-level cloudiness such as stratus. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: This section has been rewritten
2-2621	2	78	26	78	28	The main value of the article by Hirschi et al. (2011, Nature Geoscience) is that it documents the suggested feedback with observations, while previous studies were generally based on modeling experiments. However, the present sentence goes further than the cited article regarding the role of these feedbacks for heat wave	ACCEPTED: Text amended

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						trends. It namely states "This suggests that part of the observed trend in the hot extremes could have resulted from trends in moisture availability". Hence the present sentence should be modified to include "partly" before "enhanced the duration of extreme summer heat waves". [Sonia Seneviratne, Switzerland]	
2-2622	2	78	33	78	33	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2623	2	78	34	78	34	1985 appears to be incorrect. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: This should be 1895. Text amended accordingly.
2-2624	2	78	34			I think it should be 1885 instead of 1985. [Uwe Stoeber, Germany]	ACCEPTED: This should be 1895. Text amended accordingly.
2-2625	2	78	34			the 1930s remain the dominant decade in the 1985 to 2005 time series' doesn't make any sense to me… [Larry Thomason, United States of America]	ACCEPTED: This should be 1895. Text amended accordingly.
2-2626	2	78	42	78	42	delete comma after 'Thailand' [Peter Burt, UK]	EDITORIAL:
2-2627	2	78	42	78	43	Please note that the study by Lee et al. (2011) did not explicitly mention "heat waves". The study results showed that, from 1885 to 2008, there was a significant increase in the warm spell duration index but decrease in the cold spell duration index in Hong Kong. [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: In this context we are combining the definitions of "heat waves" and "warm spells".
2-2628	2	78	48	78	48	insert 'that' after 'conclusions' [Peter Burt, UK]	EDITORIAL:
2-2629	2	78	54			Why "although"? The arguments rather support than oppose each other. [Uwe Stoeber, Germany]	REJECTED: The sentence uses the uncertainly language of IPCC which distingishes between medium confidence where a likelihood statement cannot be applied and high confidence where a likelihood statement is applied. Therefore the "although' is justified.
2-2630	2	79	2	79	2	In the sentence there are some words repeated (that this is). [Claudio Cassardo, Italy]	ACCEPTED: text amended
2-2631	2	79	2	79	2	The phrase "high confidence that this is likely" doesn't make much sense to me. [Robert Waterland, United States of America]	ACCEPTED: "that this is" removed
2-2632	2	79	4	79	4	When I saw "hydrological cycle", I was expecting to see precipitation, evaporation etc. I was not thinking about extreme rainfall nor drought. Also, this section is way too long when compared with other section. It would be better if this section is split into two, one covers extreme precipitation, and other droughts. [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: This section has been rewritten and shortened and separate sections on precipitation extremes and floods and droughts have been included.
2-2633	2	79	4	82	11	Normally the extremes associated with the hydrological cycle would be floods and droughts (including hydrological droughts). There is little information on these runoff extremes in the chapter. Again the references in comment no. 2 could be used to refer to recent studies of observed changes in hydrological extreme indices. Wilson et al. (2010) include trend detection of rain and snow floods (daily maximum) and also shows that snowmelt floods occur earlier in the snow dominated Nordic river basins. This reference should be one of several (more to be found in the SREX-report) adding to the study by Petrow and Merz (2009) that only considers Germany. Stahl et al. (2010) show that low flows in Europe have increased in winter low flow regimes and decreased in most summer low flow regimes. [Hege Hisdal, Norway]	TAKEN INTO ACCOUNT:This section has been rewritten and shortened and separate sections on precipitation extremes and floods and droughts have been included. Run-off and stream flow are covered in 2.5.2.
2-2634	2	79	8	79	8	Rare is not defined here. Perhaps provide an indicative return period or equivalent. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: Text amended
2-2635	2	79	9	79	10	insert commas after 'view' and 'analyses' [Peter Burt, UK]	EDITORIAL:
2-2636	2	79	10	79	10	insert comma after 'however' [Peter Burt, UK]	EDITORIAL:
2-2637	2	79	16	79	16	Hydrological → hydrological [Peter Burt, UK]	EDITORIAL:

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2-2638	2	79	16	79	16	Consider this revised definition of the hydrologic cycle (noting spelling and lower case): "The hydrologic cycle describes the continuous circulation of water between Earth's atmosphere and both surface and subsurface bodies of water." [Forrest Mims, USA]	EDITORIAL:
2-2639	2	79	16	79	20	This is introductory material and would be better at the start of the section (line 6) [Peter Burt, UK]	EDITORIAL:
2-2640	2	79	23	79	26	Does "wetter climate" refer to the globe? And what is wetter? The atmosphere (water vapor)? The link between a "wetter" global climate and the precipitation trends is unclear. This section is subject to misinterpretation. [Dian Seidel, USA]	ACCEPTED: some additional discussion of the temperature scaling of precipitation is discussed with reference to global and regional changes.
2-2641	2	79	28	80	19	Where is the summary on Australian extremes? In addition to the Westra and Sisson (2011) and Jakob et al (2011) papers listed above which focus more on shorter-duration rainfall, there are numerous papers on daily rainfall trends around the continent which I am sure lead author Lisa Alexander would be aware of and therefore I won't list here. These references should be included. [Seth Westra, Australia]	TAKEN INTO ACCOUNT: This section has been shortened with much of the regional detail removed since this is within the purview of Ch 14. These references and others are however considered in other parts of section 2.6.
2-2642	2	79	28		34	Do not agree that SREX did a good job at all. The need is for hourly data. [Kevin Trenberth, USA]	REJECTED: SREX did a good job assessing the available literature. While we agree that there is a need for the analysis of hourly data, we would highlight that there is very little literature to date on this subject (on the continental/global scales which is within the scope of this chapter). Some literature is available on local/regional scales but this is not within the remit of this chapter but that of Ch 14. However we do agree that we need to make some mention of the fact that hourly data are required and we have amended this section accordingly.
2-2643	2	79	31	79	31	Pryor et al., 2009 reference for precipitation should be Internat. J. Climatology Vol 29. pp 31-45 doi 10.1002/joc.1696. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: The reference has been amended.
2-2644	2	79	33	79	33	insert comma after 'decades' [Peter Burt, UK]	EDITORIAL:
2-2645	2	79	34	79	34	insert comma after 'however' [Peter Burt, UK]	EDITORIAL:
2-2646	2	79	36	79	36	Does "heavy precipitation increased" mean there are more heavy precip events, or there is more precip in heavy precip events? [Dian Seidel, USA]	TAKEN INTO ACCOUNT: This section has been rewritten.
2-2647	2	79	36	79	37	Identifying "North America: and then "Canada, the U.S., and Mexico" in the same sentence is redundant. [David Sauchyn, Canada]	ACCEPTED: Reference is now only made to North America
2-2648	2	79	37	79	37	delete comma after U.S. [Peter Burt, UK]	EDITORIAL:
2-2649	2	79	37	79	37	insert comma after Mexico [Peter Burt, UK]	EDITORIAL:
2-2650	2	79	38	79	38	Instead of "reduction in the return period", consider simpler language like "more frequent" [Dian Seidel, USA]	REJECTED: It is hard to convey the concept of a percentage reduction in return period by just using the term "more frequent". This would not allow the specifics of the study and is therefore not changed.
2-2651	2	79	41	79	41	superscript 'th' (x2) [Peter Burt, UK]	EDITORIAL:
2-2652	2	79	42	79	42	insert 'of the USA' after 'regions' [Peter Burt, UK]	EDITORIAL:
2-2653	2	79	42	79	42	century → Century [Peter Burt, UK]	EDITORIAL:
2-2654	2	79	42	79	42	"The central plains" should have plains capitalized: "The central Plains". [Richard Heim, U.S.A.]	EDITORIAL:

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2655	2	79	46	79	46	superscript 'th' [Peter Burt, UK]	EDITORIAL:
2-2656	2	79	49	79	52	There is a more comprehensive study of extreme precipitation trends in South America, which should be included here: Haylock, M. R., T. C. Peterson, L. M. Alves, T. Ambrizzi, Y. M. T. Anunciação, J. Baez, V. R. Barros, M. A. Berlato, M. Bidegain, G. Coronel, V. Corradi, V. J. Garcia, A. M. Grimm, D. Karoly, J. A. Marengo, M. B. Marino, D. F. Moncunill, D. Nechet, J. Quintana, E. Rebello, M. Rusticucci, J. L. Santos, I. Trebejo, and L. A. Vincent, 2006: Trends in total and extreme South American rainfall 1960-2000 and links with sea surface temperature. Journal of Climate, 19, 1490-1512. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: More recent studies covering much of South America have also now been included.
2-2657	2	79	54	80	29	Somewhere, studies should be cited that aim to explain changes on different time scales. E.g., Scaife et al. J Climate (2008, already cited elsewhere in Chapter 2) find that much of the trends in precipitation extremes can be explained by changes in the NAO. Similarly, for the UK Maraun et al, Clim Dynam, 2011 (reference given above) find that changes in extreme precipitation on interannual and decadal scales can partly be explained by changes in the large scale atmospheric circulation. [Douglas Maraun, Germany]	TAKEN INTO ACCOUNT: We do include references now to studies which show that sea surface temperature patterns and large scale circulation can influence regional changes in extremes.
2-2658	2	79	55	79	55	Does this "trend in summer precipitation" relate to extreme precipitation? If not, does this reference belong here? [Dian Seidel, USA]	TAKEN INTO ACCOUNT: It has been clarified that this refers to precipitation extremes.
2-2659	2	79		82		There have been quite a few significant works left out, most of them observational works. They are listed below. In particular, Lau and Wu (2007) discussed the changes in extreme precipitation over low latitudes; Fujibe et al. (2005) showed long term record over 106 years; Sun et al. (2007) reviewed model results; and Liu et al. (2009) found a quantitative link between the change in observed extreme precipitation and the increase in global temperature. Fujibe, F., N. Yamazaki, M. Katsuyama, and K. Kobayashi (2005), The increasing trend of intense precipitation in Japan based on four-hourly data for a hundreds years, SOLA, 1, 41–44. Goswami, B. N., V. Venugopal, D. Sengupta, M. S. Madhusoodanan, and Prince K. Xavier (2006), Increasing trend of extreme rain events over India in a warming environment, Science, 314, 1442–1445. Groisman, P. Y., R. W. Knight, D. R. Easterling, T. R. Karl, G. C. Hegerl, and V. A. N. Razuvaev (2005), Trends in intense precipitation in the climate record, J. Clim., 18, 1326–1350. Karl, T. R., and R. W. Knight (1998), Secular trends of precipitation amount, frequency, and intensity in the United States, Bull. Am. Meteorol. Soc., 79, 231–242. Klein Tank, A. M. G., and G. P. Können (2003), Trends in indices of daily temperature and precipitation extremes in Europe, 1946–99, J. Clim., 16, 3665–3680. Lau, K. M., and H. T. Wu (2007), Detecting trends in tropical rainfall characteristics, 1979–2003, Int. J. Climatol., 27, 979–988. Lenderink, G., and E. V. Meijgaard (2008), Increase in hourly precipitation extremes beyond expectations from temperature changes, Nature Geoscience, 1, 511–514. Liu, B., M. Xu, M. Henderson, and Y. Qi (2005), Observed trends of precipitation amount, frequency, and intensity in China, 1960–2000, J. Geophys. Res., 110, D08103, doi:10.1029/2004JD004864. Liu, Shaw Chen , Congbin Fu, Chein-Jung Shiu, Jen-Ping Chen, and Futing Wu. Temperature dependence of global precipitation extremes, GRL, VOL. 36, L17702, doi:10.1029/2009GL040218, 2009. Manton, M. J. et al. (2001), Trends in extreme daily rainfall and temperature in Southeast Asia and the South Pacific: 1961–1998, Int. J. Climatol., 21, 269–284. Min, S.-K., X. Zhang, F. W. Zwiers and G. C. Hegerl (2011), Human contribution to more-intense precipitation extremes, Nature, Vol. 470, 378-381. Semenov, V. A., and L. Bengtsson (2002), Secular trends in daily precipitation characteristics: Greenhouse gas simulation with a coupled AOGCM, Clim. Dyn., 19, 123–140. Sun, Y., S. Solomon, A. Dai, and R. W. Portmann (2007), How often will it rain? J. Clim., 20, 4801–4818. Trenberth, K. E., A. Dai, R. M. Rasmussen, and D. B. Parsons (2003), The changing character of precipitation, Bull. Am. Meteorol. Soc., 84, 1205–1217. [Shaw Liu, China]	TAKEN INTO ACCOUNT: The purpose of this assessment is to update the assessments of AR4 and SREX. Some of the literature referenced here does not update those assessments. However, we agree that some of the other works could be included and we have amended the text accordingly.
2-2660	2	79		82		Section 2.7.2 Hydrological Cycle: Little is said about runoff extremes, apart from a short paragraph on flood. [Lena M. Tallaksen, Norway]	TAKEN INTO ACCOUNT: Run-off and stream flow are assessed in Section 2.5.2

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2661	2	79		82		In the paper by Stahl et al. (2010) trends in annual minima (low flow) 7-day are presented, whereas in Stahl et al. (2012) both trends in annual maxima (7-day high flow) and minima (7-day low flow) indices are discussed. In both these papers the indices are derived for the European dataset of more than 400 near-natural streamflow records. [Lena M. Tallaksen, Norway]	TAKEN INTO ACCOUNT: This section has been amended and reference to stream flow and runoff are now included in Section 2.5.2.
2-2662	2	79		82		The distribution of trends in observed high flow (Stahl et al., 2012) was found to be similar to the annual runoff (i.e. negative trends in south-eastern Europe and positive elsewhere) with somewhat more positive trends. Differences were found in the Alps and Scandinavia, where high flow decreased in some areas despite an increased annual runoff. Hence, high flow appears to have increased in rain-dominated hydrological regimes, whereas an inconsistent or decreasing signal was found in snow-dominated regimes, which typically have a late spring maximum runoff generated by snowmelt. [Lena M. Tallaksen, Norway]	TAKEN INTO ACCOUNT: This section has been amended and reference to stream flow and runoff are now included in Section 2.5.2.
2-2663	2	79		82		In Stahl et al. (2010) low flow values were derived for the summer half year (May to November) to exclude low flow periods caused by snow and ice. Low flow was found to have increased in most winter low-flow regimes and decreased in most summer low-flow regimes. For both 7-day and 30-day low flows, the proportion of negative (more extreme low flows) trends substantially exceeded the positive (less extreme low flows) trends. [Lena M. Tallaksen, Norway]	TAKEN INTO ACCOUNT: This section has been amended and reference to stream flow and runoff are now included in Section 2.5.2.
2-2664	2	79				section 2.7.2: Very relevant here is the framing of the questions and how they are approached. Please see Trenberth 2011 for considerable material on this. In particular, how data are processed wrt removal of means, whether anomalies are expressed as percentages (as appropriate if related to Clausius Clapeyron) and normalization methods (mm/day or %), whether also normalized by amount of warming and whether local or global warming, whether separated into seasons since different expectations occur in summer vs winter. SREX did a poor job on assessing these aspects. [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT: This section has been revised and although we have had to reduce this section quite considerably we discuss how sub-daily precipitation extremes and the framing of questions is relevant. There is also more information on this in Chapter 7.
2-2665	2	80	3	80	3	insert 'the' after 'in' [Peter Burt, UK]	EDITORIAL:
2-2666	2	80	3	80	3	superscript 'th' (x3) [Peter Burt, UK]	EDITORIAL:
2-2667	2	80	11	80	11	ten → 10 [Peter Burt, UK]	EDITORIAL:
2-2668	2	80	11	80	19	Please consider including the results of the following studies on the detected regional changes in extreme precipitation frequency and intensity in China. References: - Su, B., M. Gemmer, and T. Jiang, 2008: Spatial and temporal variation of extreme precipitation over the Yangtze River Basin. Quaternary International, 186(1), 22-31. - Qian, W., J. Fu, W. Zhang, and X. Lin, 2007: Changes in Mean Climate and Extreme Climate in China During the Last 40 Years. Advances in Earth Science, 22(7), 673-684. - Ren, Z. X., and D. Y. Yang, 2007: Study On Trends Of Extreme Climate Change in the Arid Region of Northwest China in Resent 40 Years. Journal of Arid Land Resources and Environment, 21(4), 10-13 - Sun, F., S. Yang, and G. Ren, 2007: Decade Variations of Precipitation Event Frequency, Intensity and Duration in the Northeast China. Journal of Applied Meteorological Science, 18(5): 610-618. - Zhang, Q., C. Y. Xu, S. Becker, Z. X. Zhang, Y. D. Chen, and M. Coulibaly, 2009: Trends and abrupt changes of precipitation maxima in the Pearl River basin, China. Atmos. Sci. Let., 10(2), 132-144. - You, L., X. Dai, and Y. Zhang, 2010: Extreme Precipitation Events in Inner Mongolia in 1961-2008. Adv. Clim. Change Res., 6(6), 411-416. - Wang, Y., and L. Zhou, 2005: Observed trends in extreme precipitation events in China during 1961- 2001 and the associated changes in large-scale circulation. Geophys. Res. Lett., 32, L09707, doi:10.1029/2005GL022574. - Zhang, D. Q., G. L. Feng, and J. G. Hu, 2008: Trend of extreme precipitation events over China in last 40 years. Chinese Phys. B, 17(2), 736-742. [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: These papers have been assessed by the chapter authors. Much of the regional information is contained in Ch 14.
2-2669	2	80	11	80	29	Please consider including the results of the study on the analysis of long period of hourly precipitation data of Hong Kong from 1885 to 2008. The study showed that the frequency of occurrence and intensity of heavy rain events in Hong Kong exhibited a long term increasing trend from 1885 to 2008. Reference : - Wong, M. C , H. Y. Mok, and T. C. Lee, 2010: Observed changes in extreme weather indices in Hong Kong.	TAKEN INTO ACCOUNT: This paper has been assessed by the authors. We acknowledge however that our assessment mostly focusses on daily extremes. Much more regional detail is contained

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Int. J. Climatol.. Published online in October 2010, doi:10.1002/joc.2238, 12 pp. [Tsz-cheung Lee, Hong Kong]	within Chapter 14.
2-2670	2	80	14	80	14	The sentence, "scales in the Asia-Pacific region and during monsoon seasons over Indian subcontinent (Krishnamurthy et", should have "the" added before Indian: "scales in the Asia-Pacific region and during monsoon seasons over the Indian subcontinent (Krishnamurthy et". [Richard Heim, U.S.A.]	EDITORIAL:
2-2671	2	80	15	80	15	A reference, Shiu et al. (2009, J. Climate, 5635-5649), can be added here. [Chia Chou, Taiwan, ROC]	TAKEN INTO ACCOUNT: This paper has been assessed by the chapter authors.
2-2672	2	80	18		19	what data were used? African data are few. [Kevin Trenberth, USA]	NOTED: We do not understand the comment given that we have made reference to several studies which have used data from Africa. Each study has obtained data from their own sources, workshops etc. It is not that African data are few but rather that data are hard to obtain so there are few analyses. We discuss in Box 2.5 the difficulty in obtaining data.
2-2673	2	80	21	80	21	Above → The above [Peter Burt, UK]	EDITORIAL:
2-2674	2	80	21	80	21	Similarly, the sentence "Above studies generally use indices" should begin with "the": "The above studies generally use indices". [Richard Heim, U.S.A.]	EDITORIAL:
2-2675	2	80	21			Above studies generally use indices which reflect „moderate“ extremes: this is not the case for Toreti et al, 2010. They use the POT approach. [Douglas Maraun, Germany]	TAKEN INTO ACCOUNT: We only state that the above studies "generally" have used moderate extremes and this is true of the large majority of the studies that we have cited.
2-2676	2	80	24	80	25	This repeats material on page 78 lines 38-39 [Dian Seidel, USA]	ACCEPTED: Repeated material removed.
2-2677	2	80	25	80	25	delete comma after 2010 [Peter Burt, UK]	EDITORIAL:
2-2678	2	80	31	80	43	The reliance on the SREX for information on severe thunderstorms is unfortunate. It seems to have missed many important references. [Harold Brooks, USA]	NOTED:
2-2679	2	80	31	80	43	Inhomogeneities in the US tornado records are discussed in Verbout, S. M., H. E. Brooks, L. M. Leslie, and D. M. Schultz, 2006: Evolution of the US tornado database: 1954-2003. Wea. Forecasting, 21, 86-93. and Doswell, C. A. III, H. E. Brooks, and N. Dotzek, 2009: On the implementation of the Enhanced Fujita Scale in the USA. Atmos. Res., 93, 554-563,doi:10.1016/j.atmosres.2008.11.003. Given the misuse of the tornado records in climate change discussions, it would be nice to have this said in this location. [Harold Brooks, USA]	ACCEPTED: These references have been included
2-2680	2	80	31	80	43	Hailpad studies from Italy and France appear to show a slight shift in the distribution of hail to larger sizes (caveat-almost none of the hail is larger than 2 cm and the result may not hold at larger sizes). Berthet, C., J. Dessens, J.L. Sanchez, 2010: Regional and yearly variations of hail frequency and intensity in France, Atmos. Res., 100, 4, 391-400,doi:10.1016/j.atmosres.2010.10.008. and Eccel, E., Cau, P., Riemann-Campe, K. and Biasioli, F. 2011: Quantitative hail monitoring in an alpine area: 35-year climatology and links with atmospheric variables. Int. J. of Clim.. doi: 10.1002/joc.2291 [Harold Brooks, USA]	TAKEN INTO ACCOUNT: These references have been included and the text amended accordingly (We believe that you are referring to the Berthet et al. 2011 paper and the Eccel et al. 2012 paper).
2-2681	2	80	31	80	43	Xie, B., Q. Zhang, and Y. Wang, 2010: Observed Characteristics of Hail Size in Four Regions in China during 1980–2005, J. Clim. 23, 4973-4982. doi: 10.1175/2010JCLI3600.1 looks at hail size changes in different regions in China and finds little consistency since 1980, but carry out model simulations using observed atmospheric changes that show why the regions can have different results [Harold Brooks, USA]	TAKEN INTO ACCOUNT: This reference has been included and the text revised.
2-2682	2	80	31	80	43	Berthet et al. showed a correlation between the fraction of precipitation falling as hail and the average summer temperature in the region of the ANELFA hailpad network in France (Berthet, C., J. Dessens, J.L. Sanchez, 2010: Regional and yearly variations of hail frequency and intensity in France, Atmos. Res., 100, 4, 391-400,doi:10.1016/j.atmosres.2010.10.008.) [Harold Brooks, USA]	TAKEN INTO ACCOUNT: This reference has been included and the text revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2683	2	80	33	80	34	Doswell et al. (2005) do not discuss station series inhomogeneities. They talk about inhomogeneities in the records of reports, but virtually none of the data come from station series [Harold Brooks, USA]	ACCEPTED: This reference has been replaced with other more appropriate references
2-2684	2	80	36			Why is there high uncertainty associated with assessing the environmental conditions that are favourable for severe thunderstorms or hailstorms". [David Sauchyn, Canada]	ACCEPTED: This was unclear from the current sentence and has been revised accordingly.
2-2685	2	80	45	80	46	Would a trend in the incidence of flooding be expected at a global scale given the variety of local conditions (rainfall intensity, snow cover, rate of snowmelt, soil and topographic factors, etc.) that determine flooding? [David Sauchyn, Canada]	TAKEN INTO ACCOUNT: The discussion in this section has been revised.
2-2686	2	80	45	80	55	Flood damage is introduced, but not much discussion is given on flood types e.g. pluvial versus fluvial. The following paragraph on drought addresses the differences in drought definitions in better detail. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: Given the limited space and the fact that flood events will be more robustly assessed by WGII we keep our assessment limited to discussion of observed trends in floods and remove any reference to flood damage.
2-2687	2	80	45	80	55	This is vague about the period(s) of study for the reported changes. [Dian Seidel, USA]	ACCEPTED: This section has been revised.
2-2688	2	80	45		55	Flood damage is affected by infrastructure and value of property. Whether heavy rains produce floods also depends on mitigation, drainage systems, levees, etc. Floods are not a good indicator of climate change. This should be reflected in l5-6 p 82 also. [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT: We agree that flood damage is outside the scope of WGI and therefore we have removed this sentence. However floods can be an indicator of heavy precipitation changes and in that sense should be used as an indicator for possible changes in observed climate (without making any statements about climate change)
2-2689	2	80	46	80	46	insert comma either side of 'however' [Peter Burt, UK]	EDITORIAL:
2-2690	2	80	52	80	54	It is emphasized that there is currently no clear and widespread evidence for observed changes in flooding except for the earlier spring flood in snow-dominated regions. For Europe two references are given: Benito et al., 2005 and Petrow and Merz, 2009. Neither of these provide any evidence for trend patterns in Europe. The first, is a 2 page introduction to a special issue on paleoflood hydrology and the reference is likely included by a mistake, whereas the second only addresses trends in flood in Germany. Thus, other studies/references are needed to confirm this statement (see the SREX report). [Lena M. Tallaksen, Norway]	ACCEPTED: The Petrow and Merz, 2009 study was the reference we intended to mention here but we pointed to the wrong Benito et al. 2005 reference regarding floods in Spain. We have therefore amended the text and updated the references accordingly.
2-2691	2	80	54	80	54	In this context see also reference S. Troemel and C.-D. Schoenwiese, 2007: Probability change of extreme precipitation observed from 1901 to 2000 in Germany. Theor. Appl. Climatol., 87, 29-39. In this paper a Gumbel PDF with time-dependent location and scale parameter is adopted to 132 observed German precipitation time series 1901-2000 and it is shown that the probability of exceeding the 95th percentile has increased considerably, especially in winter and western/southern parts of this region. [Christian-D. Schoenwiese, Germany]	TAKEN INTO ACCOUNT: This paper has been assessed by the authors.
2-2692	2	81	1	81	7	This paragraph is very good. Good approach to discuss drought based on the three different drought types: meteorological, agricultural, and hydrological drought. [Richard Heim, U.S.A.]	NOTED
2-2693	2	81	1	82	11	The discussion on drought in this section is strongly based on the work of only a few authors, largely working with the PDSI. The PDSI can be conceived of (as the author did) as a hydrologic model; thus the examination is of a trend on an index that has a non-linear relationship to actual metrics of concern, e.g. streamflow or soil moisture. Actual observations of trends in quartiles of streamflow showed very strong trends over thelast half decade in the Pacific Northwest, a result that should probably be discussed, if for no other reason than the analytical approach is directly at this problem. (See Luce and Holden, 2009, doi:10.1029/2009GL039407 as an example for hydrological drought). [Charles Luce, United States of America]	TAKEN INTO ACCOUNT: Given the limited space in this section we try to summarise the conclusions from SREX and any subsequent analyses. SREX thoroughly assessed different aspects of drought from meteororological to soil-moisture to hydrological drought. We discuss the problems with PDSI and the various drought metrics in order to highlight the various conceptual ways of thinking of drought. Therefore we disagree that our focus is solely on PDSI. Variables such as streamflow are assessed in Section 2.5.2 and regional changes are covered in Chapter 14.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2694	2	81	2	81	3	The WMO recently adopted the Standardized Precipitation Index as the recommended method for calculating Meteorological Drought. A reference to that might be appropriate. [Michael Brewer, United States of America]	NOTED
2-2695	2	81	2	81	6	The SREX is incorrectly cited here. As highlighted in the SREX (see Box 3.3 of SREX chapter 3), agricultural (or soil moisture) drought is NOT to be equated with a precipitation shortage. It is by definition a soil moisture shortage, which is either induced by precipitation shortage, evapotranspiration excess, or both. Hence, "a precipitation shortage during the growing season" should be replaced with "a soil moisture shortage during the growing season". Similarly, hydrological drought, i.e. runoff shortage, can be either induced by precipitation shortage, evapotranspiration excess, or both (in addition to human water use). Hence, "precipitation shortage" should be removed in the parenthesis following "hydrological drought". [Sonia Seneviratne, Switzerland]	ACCEPTED: text amended accordingly
2-2696	2	81	5	81	5	; → , [Peter Burt, UK]	EDITORIAL:
2-2697	2	81	9		47	The write up here flounders and should be reorganized somewhat. Yes Wells et al introduced a self-calibrating PDSI but a version has been used by Dai 2011b. [Kevin Trenberth, USA]	ACCEPTED: This paragraph has been rewritten.
2-2698	2	81	10			What is PDSI? [Uwe Stoeber, Germany]	TAKEN INTO ACCOUNT: The Palmer Drought Severity Index as spelt out in Box 2.4.
2-2699	2	81	14	81	15	How much analysis has there been subsequent to SREX; give references. [David Sauchyn, Canada]	TAKEN INTO ACCOUNT: There are several papers published on drought that were not included in the SREX assessment. These have been included in the revised text.
2-2700	2	81	17	81	18	Reference to Nicholls and Larssen (2011). 4AR cited Nicholls previous work where he claimed that temperatures exacerbate drought. I have since corrected him and I am pleased that he now appreciates that droughts lead to higher temperatures, not that temperatures cause higher evaporation (as now presented in Nicholls and Lassen (2011). The reference to such findings in 2011 ignores the long history of both Hydrology and Boundary layer Meteorology. Lockart et al, GRL, (2009) corrected the original claims. It would be more appropriate to cite this, but also it would be appropriate to acknowledge the error in the 4AR and confirm the long-established science of hydrology and boundary layer meteorology whith this regard. [Stewart Franks, Australia]	TAKEN INTO ACCOUNT: This was the incorrect reference to use here. The sentence refers to land-atmosphere feedbacks generally not specifically to temperatures exacerbating droughts. We do not feel that we have therefore ignored the science of hydrology and boundary layer meteorology nor that there was an error in AR4 in this regard.
2-2701	2	81	17	81	25	This paragraph is important to include since the PDSI my not be comparable across climate zones, or even applicable in some climate zones. The self-calibrating PDSI is a good replacement for the traditional PDSI. This is a very good paragraph to include, important in the discussion of drought. [Richard Heim, U.S.A.]	NOTED
2-2702	2	81	22	81	22	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2703	2	81	27	81	34	The wording of "increases" and "decreases" in dryness is confunsing in this paragraph. A decrease in dryness could easily be seen as an environment that becomes wetter, however, I think in line 30 where it is mentioned for North and Central America it is meant to describe conditions that become drier. [Birgit Hassler, USA]	ACCEPTED: This section has been rewritten
2-2704	2	81	27	81	34	There is an unmatched paranthesis in this paragraph. It begins in line 29: "continents (see Table 3.2". I suspect it should be closed after "(2012)": "continents (see Table 3.2, P128 of Seneviratne et al. (2012))." [Richard Heim, U.S.A.]	EDITORIAL:
2-2705	2	81	27	81	34	This is another case where divergent trends could be attributable to decadal scale variability in the hydroclimate. [David Sauchyn, Canada]	TAKEN INTO ACCOUNT: We do not rule decadal variability in or out as this is not within the remit of Chapter 2 but rather Chapter 10.
2-2706	2	81	27	81	34	The PDSI should be better introduced and trends in PDSI should be viewed with caution as emphasized in Chapter 10 (10-30, line 24). [Lena M. Tallaksen, Norway]	ACCEPTED: Agreement of the wording between Ch 2 and Ch 10 has been discussed
2-2707	2	81	32	81	32	In the sentence, "2006; Dai, 2011a; Dai, 2011b; Kunkel et al., 2008; Sheffield and Wood, 2008) while in Africa while there", there should be a comma before "while": "2006; Dai, 2011a; Dai, 2011b; Kunkel et al., 2008; Sheffield and Wood, 2008), while in Africa while there" [Richard Heim, U.S.A.]	EDITORIAL:

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2708	2	81	36	81	41	The paragraph stresses disparity in results from two studies, then presents Fig 2.42a showing results from just one of the two. Why? [Dian Seidel, USA]	ACCEPTED: Given the disparity amongst results this figure has been updated.
2-2709	2	81	36	81	41	It does not seem balanced to only provide the analysis from Dai (2011b) in Fig. 2.42a after highlighting the issues with assessing changes in drought and the fact that Sheffield and Wood (2008) found results opposite to those of Dai (2011a; 2011b). If the figure from Dai (2011b) is kept, a corresponding figure from Sheffield and Wood (2008) should be added, or else a clear argumentation provided for this choice. [Sonia Seneviratne, Switzerland]	ACCEPTED: Given the disparity amongst results this figure has been updated.
2-2710	2	81	36	81	41	The complexity of drought should be quantified better. Some of the complex features of Canadian drought were recently illustrated by, for example, Bonsal et al., 2011: Characterizing the surface features of the 1995-2005 Canadian Prairie drought in relation to previous severe Twenthieth Century events. Atmos.-Ocean, 49, 320338. Such information could be added elsewhere instead. [Ronald Stewart, Canada]	ACCEPTED: The drought discussion has been revised.
2-2711	2	81	36	81	41	It should be mentionned here that drought is a phenomenon affecting different levels of the hydrological cycle, from precipitation, soil moisture, to river flows, corresponding to the three types of droughts defined by Wilhite and Glantz (1985) and Keyantash and Dracup (2002). Assessments of trends in droughts may thus be performed on these different levels and give different results. Vidal et al. (2010) proposed standardized indices applicable throughout the hydrological cycle --namely SPI, Standardized Soil Wetness Index (SSWI) and Standardized Flow Index (SFI) -- that can be used to assess jointly such trends. [Jean-Philippe Vidal, France]	TAKEN INTO ACCOUNT: The drought discussion has been revised. We do discuss the complexity of drought and how different measures can lead to different results.
2-2712	2	81	36	81	41	Standardized indices like the SPI can be applied at different time scales (typically from 3 to 12 months) in order to represent short or longer droughts. Given the multi-frequency characteristics of the signal, trends obtained with a given time scale may not be transfered to another time scale. This has been shown by Vidal et al. (2010) who used 3 different time scales to characterize the same drought events over a 50-yr period. It is therefore essential to specify the time scale used in any cited study on drought trends based on standardized indices, and this should be the case here. [Jean-Philippe Vidal, France]	ACCEPTED: The drought discussion has been revised.
2-2713	2	81	36	81	41	Droughts develop in both time and space, therefore the spatial dimension is a critical variable to take into account in any drought trend study. This should be clearly mentionned in this paragraph. Examples are given by Sheffield et al. (2009) and Vidal et al. (2010). [Jean-Philippe Vidal, France]	ACCEPTED: The drought discussion has been revised.
2-2714	2	81	36	81	41	References for the comments on this paragraph: - Keyantash, J. & Dracup, J. A., 2002: The quantification of drought: An evaluation of drought indices Bulletin of the American Meteorological Society, 83, 1167-1180 - Sheffield, J., K. M. Andreadis, E. F. Wood, D. P. Lettenmaier, 2009: Global and Continental Drought in the Second Half of the Twentieth Century: Severity–Area–Duration Analysis and Temporal Variability of Large-Scale Events. J. Climate, 22, 1962–1981. doi: http://dx.doi.org/10.1175/2008JCLI2722.1 - Vidal, J.-P., Martin, E., Franchistéguy, L., Habets, F., Soubeyroux, J.-M., Blanchard, M., and Baillon, M., 2010: Multilevel and multiscale drought reanalysis over France with the Safran-Isba-Modcou hydrometeorological suite, Hydrol. Earth Syst. Sci., 14, 459-478, doi:10.5194/hess-14-459-2010 - Wilhite, D. A. & Glantz, M. H., 1985: Understanding the drought phenomenon: The role of definitions. Water International, 10, 111-120 [Jean-Philippe Vidal, France]	TAKEN INTO ACCOUNT:
2-2715	2	81	43	81	47	Conclusions from Fig. 2.42 b written in this paragraph do not mention that the strength of this index is also very high in South America [Celeste Saulo, Argentina]	ACCEPTED: This section has been revised.
2-2716	2	81	44	81	45	The combination of the words "precipitation intensity" and "dry spell length" is confusing for the description of the multiplicative measure described here. When does precipitatin happen in a dry spell? Consider explaining that measure in more detail, or rephrase the description of it. [Birgit Hassler, USA]	ACCEPTED: This section has been revised.
2-2717	2	81	45	81	45	Insert “positive” before trends. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: text amended
2-2718	2	81	46	81	46	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2719	2	81	52	81	53	Figure 2.42, Figure caption: Same as comment No. 52. [Birgit Hassler, USA]	NOTED

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2-2720	2	81	56	81	56	In the sentence, "In summary, analyses continue to support the AR4 and SREX conclusions that it is likely that there has been", "has" should be "have": "In summary, analyses continue to support the AR4 and SREX conclusions that it is likely that there have been". [Richard Heim, U.S.A.]	EDITORIAL: amended
2-2721	2	81	56	81	56	Replace "has" with "have". [Robert Waterland, United States of America]	EDITORIAL: amended
2-2722	2	81	56	81	58	Like I said before, the most complete analysis I have seen is one of Demetris Koutsoyiannis for the EGU 2011 conference. Unfortunately there is no peer reviewed publication yet. The analysis is available at http://itia.ntua.gr/getfile/1124/2/documents/2011EGU_DailyRainMaxima_Pres.pdf They analysed over 3000 time series with at least 100 years of data. Especially in the alleged anthropgenic era (since 1970) there is no trend at all. This is really the most global picture we have right now in my opinion. [Marcel Crok, The Netherlands]	REJECTED: Our summary reflects the assessment of the peer-reviewed literature from multiple independent analyses. The analysis to which you refer contains mostly data in North America, Europe and Australia and contains practically no data for Africa, South America or the tropics and only looks at trends in annual maxima rainfall. Therefore we feel this hardly represents "the most global picture we have right now" of all aspects of extreme rainfall. It is also the remit of Chapter 2 to only comment on observed changes and not make assertions about the "anthropogenic era" in respect to forcings of any observed change. The assessment provided here is made on all of the available literature including numerous regional studies and assessments of many characteristics of the extreme rainfall distribution. We reflect both our confidence and our uncertainty in our summary.
2-2723	2	81	56	81	58	In order for this comparison to be relevant, one must assume that the regions are equally well observed (i.e., a balanced design). Is this the case? [Peter Guttorp, USA]	TAKEN INTO ACCOUNT: It is not necessarily the case that every region is as equally well observed as every other but it is clear from our assessment prior to this the level of observationsal studies across regions. We theerefore base our summary here with that caveat in mind.
2-2724	2	81	56	82	11	I think that the estimation of drought trends suffer of the same problem of precipitation trends: the intrinsic variability of the datum is very large, thus it is needed a very long series (presently unavailable) for getting a sufficiently long signal and thus give significance to the trend. [Claudio Cassardo, Italy]	NOTED
2-2725	2	81	58	81	58	Similar change to "regions than there has been statistically significant decreases". "has" should be "have": "regions than there have been statistically significant decreases". [Richard Heim, U.S.A.]	EDITORIAL: amended
2-2726	2	82	13	83	15	A very good summary. Some of this material is already covered in Sections 10.6.1.5 and 11.4.2.5.3 and Box 14.3. Consider merging this information into one place to save space. There should at least be cross referencing between these sections. [George Kiladis, USA]	ACCEPTED: Text has been reduced in Ch 2 and the summary for tropical storms has now been split between Ch 2 and Ch 14.
2-2727	2	82	13	83	15	Please consider extending the assessment to other tropical cyclone basins, espeically the western North Pacific. As the characteristics of tropical cyclones vary significantly from one basin to the other, the approach of Section 2.7.3 which disproportionally inclined to the research results in Atlantic is not recommended. More emphasis should be given to the findings of the studies on the tropical cyclone trends / changes in other basins, in particular the western North Pacific which is the most active tropical cyclone basin. [Tsz-cheung Lee, Hong Kong]	ACCEPTED: A much more comprehensive assessment of regional changes is now covered between Ch2 and Ch14. While the text in Ch 2 has been shortened, it does now include a better assessment of changes in other ocean basins and a revised figure that includes landfalling typhoons in China has been included to take more account of this discrepency.
2-2728	2	v	13	83	15	Please include the findings of the ESCAP/WMO Typhoon Committee's first and second assessment reports on climate change and tropical cyclone activity in the Typhoon Committee region in this section. In 2009, the ESCAP/WMO Typhoon Committee formed an expert team to assess the impacts of climate change on frequency and intensity in the Western North Pacific and the South China Sea based mainly on relevant peer-review publications. The first assessment report was published in 2010. On the observational aspect, the	TAKEN INTO ACCOUNT: Main results are included.

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						assessment revealed that there are strong inter-annual and inter-decadal variations in the tropical cyclone activity over the western North Pacific (WNP). Analysis of available tropical cyclone data from 5 different datasets since 1950s indicates that most of the tropical cyclone datasets depict a decrease in the annual number of tropical cyclones and typhoons in the western North Pacific in the past few decades and some statistically significant trend. For tropical cyclone intensity, differences in tropical cyclone databases for this basin do not allow a convincing detection of a long term trend in this basin. The second report is also nearly completed and will be published in 2012. Reference : - Lee, T.C., W.J. Lee, T. Nakazawa, J.C. Weyman, and M. Ying, 2010: Assessment report on impacts of climate change on tropical cyclone frequency and intensity in the Typhoon Committee region, ESCAP/WMO Typhoon Committee, TC/TD-No. 0001. - Lee, T.C., 2012 : A review on the long term variations of tropical cyclone activity in the Typhoon Committee Region, to be published in the Tropical Cyclone Research and Review. [Tsz-cheung Lee, Hong Kong]	
2-2729	2	82	13	83	15	Please consider include the following references on the connections of the inter-annual and inter-decadal variations in the frequency of the tropical cyclones in the western North Pacific and South China Sea with the ENSO and Pacific decadal oscillation, and East Indian Ocean sea surface temperature anomaly. References : - Goh, A. Z.-C., and J.C.L. Chan, 2010: Interannual and interdecadal variations of tropical cyclone activity in the South China Sea. Int. J. Climatol. 30, 827-843. - Zhan, R.F., Y.Q. Wang, X.T. Lei, 2011: Contributions of ENSO and East Indian Ocean SSTA to the Interannual Variability of Northwest Pacific Tropical Cyclone Frequency. J. Climate, 24, 509–521. doi: 10.1175/2010JCLI3808.1. [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: Related references are cited.
2-2730	2	82	13	83	15	Please consider including the findings of the following studies on the possible influence of climate change on the shift of tropical cyclone track and formation location over the western North Pacific. References : - Wang, R. F., L.G. Wu and C. Wang, 2011: Typhoon Track Changes Associated with Global Warming, J. Climate, doi: 10.1175/JCLI-D-11-00074.1. - Tu, Jien-Yi, Chia Chou, Pao-Shin Chu, 2009: The Abrupt Shift of Typhoon Activity in the Vicinity of Taiwan and Its Association with Western North Pacific–East Asian Climate Change. J. Climate, 22, 3617–3628. doi: 10.1175/2009JCLI2411.1. - Wu, L.G. and B. Wang, 2004 : Assessing impacts of global warming on tropical cyclone tracks, Journal of Climate, 17, p1686-1698. [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: Wang et al (2011)'s result on Typhoon tarcks is cited.
2-2731	2	82	13	83	15	If it is possible, the tropical storm evolution figures over the various regions for the recent several decades should be provided as a global map similar to AR4. [Zong-Ci Zhao, China]	ACCEPTED: A revised figure has been included to reflect changes across more ocean basins
2-2732	2	82	15	83	15	A nice summary. There is some duplication with material covered in Sections 10.6.1.5, 11.4.2.5.3, and Box 14.3. Consider merging this information into fewer place to save space. There should at least be adequate cross referencing between these sections. [George Kiladis, USA]	TAKEN INTO ACCOUNT: Text has been reduced and has now been split between Ch 2 and Ch 14.
2-2733	2	82	19	82	21	This sentence convolutes two large issues in a way that is likely to mislead readers. In the first place, there is little evidence that climate change over the 20th century is best characterized by a linear trend; indeed, the weight of evidence suggests that the cooling in the northern hemisphere from mid century into the 1970s was likely a result of an increase in sulfate aerosol loading. There is an active controversy about whether the observed multidecadal variability over the century was principally radiatively forced, on the one hand, or a response to nonlinearly varying radiative forcing, on the other. Recent work by Isaac Held, among others, shows that the global surface temperature over the 20th century can be reproduced by a remarkably simple model of a system with finite heat capacity responding to changing radiative forcing; there is no need to invoke natural multi-decadal variability. But here the language seems to assume that any departure from a linear trend represents natural variability. The sentence should be re-worded to make it clear that the detection of long-term trends is not equivalent to finding an anthropogenic fingerprint, at least not during the 20th century. [Kerry Emanuel, United States of America]	ACCEPTED: We agree that the reference to "detection" is outside the remit of Ch 2. However this is a direct quote from AR4. This sentence should rightly only refer to the "assessment" of long-term trends and that in this sense that this is differentiated from a detectable anthropogenic signature and outside the scope of Ch 2.
2-2734	2	82	21	82	21	Superflouous apostrophe after "cyclones". [Kerry Emanuel, United States of America]	EDITORIAL: this closes a quote and quotation marks have now been used instead.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2735	2	82	21	82	23	The authors should be more specific about what has to be revised."somewhat revised" is much harder than necessary. After all, the principle conclusions do not change, just the confidence level is lower than in AR4. [Uwe Stoeber, Germany]	ACCEPTED: We have added "with respect to the confidence levels associated with observed trends".
2-2736	2	82	25	82	38	Changes in tropical cyclone tracks, such as in Tu et al. 2009 (J. Climate, 3617-3628) and changes in the seasonal cycle (Tu et al. 2011, Environ. Res. Lett., 6, doi:10.1088/1748-9326/6/034013) should be discussed, besides changes in number. [Chia Chou, Taiwan, ROC]	TAKEN INTO ACCOUNT: However, changes in tropical cyclone tracks and variability are covered in Box 14.3 and not in Ch 2.
2-2737	2	82	25	82	38	It is best to put observed trends in the context of expected levels of natural variability if at all possible. So there is a trend…how unusual is it compared with natural variability [Thomas Knutson, U.S.A.]	REJECTED: Discussions of whether observed trends are outside of those expected from natural varaiability is outside of the scope of Ch 2. This discussion sits within Ch 10.
2-2738	2	82	25	82	38	(ii) Chapter 2, hurricanes, page 82, lines 40-47. Time series of hurricanes over the North Atlantic are shown and discussed. This is great - interesting and relevant. Yet only half a sentence is given to the longest historical record of tropical cyclone activity in the southern hemisphere (Callaghan and Power Climate Dynamics 2011) in the same section. This record dates back to 1872, making it one of the longest historical records of tropical cyclone/hurricane activity in the world. Yet it is only assigned half a sentence and no plot. The robustness of this record was discussed by Callaghan and Power (2011) who concluded that the record is robust and confidence in the record is justified. Robustness was established by showing that the record exhibits a Poisson distribution (consistent with shorter satellite records of TC frequency), that variance in the first part of the record is the same as variance in the second part of the record, that there is an ENSO imprint on the variability consistent with shorter satellite based records, and that the trend cooincides with a trend in the SOI over the same period and is therefore consistent. In the unlikely event that tropical cyclones have been missed these would have occurred in the early part of the record and so their inclusion would increase the magnitude of the downward trend. Recommend that the fact that it is longest such record in SH and that there is a (declining) trend which is significant at 90% level be stated, and that the general statement re "robustness" (line 37) be modified to account for this robust result. [Scott Power, Australia]	TAKEN INTO ACCOUNT: This section has had to be reduced and much of the assessment of Tropical Cyclones is now contained within Ch 14. We do however include a figure of land-falling TCs in the East Australian region along with land-falling TCs in other ocean basins.
2-2739	2	82	26	82	26	40-year [Peter Burt, UK]	EDITORIAL:
2-2740	2	82	30	82	30	delete ; [Peter Burt, UK]	EDITORIAL:
2-2741	2	82	31	82	47	This section is problematic. It emphasizes contested conclusions of one small group of authors, ignoring other work by others e.g. Holland et al, that strongly contests the conclusions drawn here. In particular, the use of simple linear trends to estimate whether there are changes over time in quantities whose temporal evolution might be highly non-linear in time is known to be problematic because it invokes the wrong null hypothesis(and erroneously partitions 'signal' variability into 'noise', thus artificially lowering apparent signal-to-noise of trends. The authors of this chapter are well aware of this. Quoting from page 8-9 of this chapter, "There are no a priori physical reasons why the long-term trend in climate should be linear in time. Historical climatic time series often have trends for which a straight line is not a good approximation... ". Yet, a naive application of linear trends is used here to dismiss the existence of trends in Atlantic tropical cyclone activity. Furthermore, results are emphasized only for 'hurricanes' when in fact overall tropical cyclone activity (e.g. annual basin-wide TC counts) yields different conclusions. This section needs to be rewritten in a way that more objectively reflects the diversity of views among experts publishing in this area. [Michael Mann, USA]	TAKEN INTO ACCOUNT: This section has had to be reduced and much of the assessment of Tropical Cyclones is now contained within Ch 14. However we feel we have re-written the text to better reflect the current state of the literature. In saying that most of our conclusions are still based on the fact that there are several competing analyses which do not agree on either the number or intensity of events or even how events should be defined. Therefore we feel justified in maintaining our low confidence statement as this reflects the diversity in the literature.
2-2742	2	82	33	82	33	[Peter Burt, UK]	EDITORIAL:
2-2743	2	82	41	82	41	Provide a reference or describe (and ideally both) how the data is adjusted. [Karen Rosenlof, United States of America]	REJECTED:This figure and text no longer appear in the section due to necessary amendments.
2-2744	2	82	49	82	51	This is not necessarily the case. For example, simply measuring the storm's intensity at that time it reaches its peak intensity adds considerable information to the mere existence of a cyclone. The demand for specifically targeted measurements over the lifetime of the cyclone is artificial and should be deleted from this document. [Kerry Emanuel, United States of America]	ACCEPTED: This sentence has been removed.

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2-2745	2	82	53	82	57	There is a paper almost through at the Journal of Climate that shows no trend in tropical cyclones. A homogeneous database of global landfalling tropical cyclones, Jessica Weinkle* and Roger Pielke, Jr., Ryan Maue Abstract In recent decades, economic damage from tropical cyclones (TCs) around the world has increased dramatically. Scientific literature published to date is strongly suggestive that the increase in losses can be explained entirely by increasing wealth in locations prone to tropical cyclone landfalls. However, no homogenized dataset of tropical cyclone landfalls has been created. We have constructed such a homogenized global landfall TC database. We find no long-term global trends in the frequency or intensity of landfalling TCs for the period with reliable data, providing very strong support for the conclusion that increasing damage around the world over the period(s) of record can be explained entirely by increasing wealth in locations prone to TC landfalls, and adding confidence in the fidelity of economic normalization analyses. [Marcel Crok, The Netherlands]	TAKEN INTO ACCOUNT: This paper has been assessed by the authors.
2-2746	2	82	54			change "homogeneous" to "relatively more homogeneous". The data set is probably not homogeneous for various reasons (view angle of satellite in Indian Ocean, etc. [Thomas Knutson, U.S.A.]	ACCEPTED: However this sentence has been removed due to a necessary reduction in text.
2-2747	2	82	55	82	57	Elsner et al. identify a significant upward trend in their data, but in my view, did not demonstrate that the trend was unusual compared with natural variability. [Thomas Knutson, U.S.A.]	REJECTED: Reference to the trend being unusual with respect to natural variability is within the scope of Ch 10 and not Ch 2.
2-2748	2	82				New paper: Lau, W. K. M., and Y. P. Zhou (2012), Observed recent trends in tropical cyclone rainfall over the North Atlantic and the North Pacific, J. Geophys. Res., 117, D03104, doi:10.1029/2011JD016510. [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This paper has been assessed by the chapter authors
2-2749	2	82				2.7.3 The counts of storms without taking intensity into account are meaningless. Fig 2.43 is disputed among different publications and should not be used. The only credible record is for the N Atlantic after 1944 (when reconaissance flights began). Webster et al (l 24) did find significant changes (in contrast to what is stated) but they were dependent on unreliable data. There is no homogenous satellite data record (l 54) and the reanalysis that was done was based on ISCCP which is itself seriously flawed and being reprocessed. The writeup here needs substantial revision. The Knutson et al work shows convincing relations with SSTs (and the differences): how else to attain skill in number variations. This point should be made. But their models fail with intensity and does not do the stronger storms well. In fact the AR4 conclusions are not bad and require modest refinement. This topics is also reviewed in Chapter 14 and it is quite a good review except it omits anything on precipitation. Maybe most of this belong in Chapter 14? [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT:Yes much of the discussion of tropical cyclones is rightly contained within Ch 14. Fig. 2.43 has been replaced with a figure that shows broader changes in tropical cyclone activity from other ocean basins and not just the North Atlantic. We do not agree that the counts of storms is meaningless but we do agree that more reference to studies which include intensity measures should be included.
2-2750	2	83	1	83	6	This paragraph is seriously remiss. It completely fails to point out that power dissipation is highly correlated with sea surface temperature, which greatly ameliorates concerns about tropical cyclone data quality, nor does it do any justice to the serious attempts to correct for cyclone wind measurement deficiencies by using central surface pressure measurements, which have been comparatively stable in time and were used by Emanuel (2005) to correct earlier wind estimates using pressure-wind relationships. These corrections were not made by Maue (2009), a major deficiency of that work. [Kerry Emanuel, United States of America]	TAKEN INTO ACCOUNT: The discussion of the influence of SSTs and tropical cyclone potential intensity is contained within Box 14.3. We note that most of the discussion is contained within this box.
2-2751	2	83	4			after "by data quality concerns" I would add "and by uncertainties in the role of natural climate variability in the existing trends." [Thomas Knutson, U.S.A.]	REJECTED: Reference to the trend being unusual with respect to natural variability is within the scope of Ch 10 and not Ch 2.
2-2752	2	83	5	83	5	Change “globally” to “in the Northern Hemisphere” as Maue (2009) only analysed ACE in the Northern Hemisphere (where, I admit, most but not all of the ACE occurs). [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This section has been shortened with much of the detail now in Ch 14. This reference has been removed here.
2-2753	2	83	6	83	6	40-year → 40 year [Peter Burt, UK]	EDITORIAL:
2-2754	2	83	8	83	15	The paragraph appears to exhibit large editorial bias. It attempts to read definite conclusions into what earlier assessments merely omitted. It also once again ignores the strong correlation between Atlantic tropical cyclone power dissipation and sea surface temperature, and as mentioned in the previous comment, also ignores the very necessary correction of hurricane wind data in the 1950s and 1960s, corrections that were	TAKEN INTO ACCOUNT: This section has been revised and much of the discussion on the relationship between SSTs and tropical cyclone potential intensity is contained within Box 14.3. We do state that there

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						indicated long before the issue of hurricanes and climate arose. There is no question that Atlantic hurricane power dissipation has trended upward from the 1950s to the present, and the omission of this important fact and the correlation with SST is highly misleading. It appears that this report's authors are happy to add fictitious cyclones to the early part of the record but at the same time omit corrections to reported wind speeds that have a robust basis, errors of commission and omission that work in the same direction. [Kerry Emanuel, United States of America]	has been an upward trend in power dissapation so would argue that this was not omitted but we have amended our conclusions to better reflect that there have been robust increases in the intensity of the strongest storms in the Atlantic since the 1970s although there is not a consensus in the literature that this holds over longer time periods or for other ocean basins.
2-2755	2	83	12	83	12	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2756	2	83	13			"record is too short at present to be reliable" I would say "…to reliably differentiate between possible influence of natural variability vs anthropogenic forcing" [Thomas Knutson, U.S.A.]	REJECTED: Reference to the trend being unusual with respect to natural variability and anthropogenic forcing is within the scope of Ch 10 and not Ch 2 but we have removed the reference to the record being "too short to be reliable".
2-2757	2	83	17	84	2	Worth mentioning in section 2.7.4 that Allan et al (2009), which you reference, found that the NAO was probably not the main influence on extreme North Atlantic/west European storms in autumn but a pattern more like the east Atlantic Oscillation. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: Due to length constraints and considering other peer-reviewed literature we feel it is sufficient to state that there are "substantial decadal and longer fluctuations".
2-2758	2	83	17	84	53	If it is possible, the extratropical storms evolutions figures over the various regions of the Northern hemisphere for the recent several decades should be provided as a global map. [Zong-Ci Zhao, China]	REJECTED: To date there is not enough literature for us to recreate this figure as a global map.
2-2759	2	83	17	85	2	Not all of the material in this section pertains to extremes, and some points from Sections 2.6.6.1 through 2.6.6.3 have been reproduced, consider merging some of this with previous sections. [George Kiladis, USA]	ACCEPTED: Repeated material removed and conclusions merged with old section 2.6 (now section 2.7).
2-2760	2	83	17	85	2	The overall sense one gets from this section on extratropical storms is that there is considerable uncertainty and a lot of "apparent discrepancies" among the studies reported. This is reminiscent of the situation with tropical cyclones a few years ago. Rather that skirt the issue with statements about differences in methods, consider making a straightforward statement about the poor state of understanding and the disparities in approach and results, and work to resolve these for AR6. Also, be very careful about reporting trend results from reanalyses. The sentence on page 83 (lines 50-53) is potentially problematic. If the utility of 20CR for this sort of trend study is dubious, should this "result" even be reported in the AR5? Similar comments apply to page 84 (line 50-53) and elsewhere. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: This section has been rewritten. We highlight that trends calculated using reanalyses are to be treated with caution but that does not mean that they should be excluded. We highlight both the utility and problems with reanalyses in Box 2.3.
2-2761	2	83	20	83	21	Replace "since the 1950s (Trenberth et al., 2007, Table 3.8), reporting on several papers showing" with "since the 1950s (Trenberth et al., 2007, Table 3.8). Several papers showed". [Robert Waterland, United States of America]	EDITORIAL:
2-2762	2	83	22	83	24	Again, "somewhat" is very unspecific and should be replaced by a description of what was revised to what extent. [Uwe Stoeber, Germany]	ACCEPTED: We have added "with respect to the confidence levels associated with observed trends" and explain the main differences in the next few paragraphs.
2-2763	2	83	29	83	29	insert comma after 'However' [Peter Burt, UK]	EDITORIAL:
2-2764	2	83	32	83	32	Gulev et al.'s abstract states "The western Pacific and Atlantic are characterized by an increase in cyclone intensity and deepening during the 42-year period, although the eastern Pacific and continental North America demonstrate opposite tendencies in most cyclone characteristics." [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: Sentence rephrased as "..while others show opposite trends in eastern Pacific and North America"
2-2765	2	83	39	83	39	Geng and Sugi (2001) only analysed the North Atlantic. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED. That reference is dropped here.

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2-2766	2	83	46	83	46	insert comma after 'Subsequently' [Peter Burt, UK]	EDITORIAL: amended
2-2767	2	83	49	83	53	This isn't an effect of the way 20CR are produced, it is an effect of the amount of data going in and how it reduces further back in time. This is discussed in Cornes and Jones (2011) - see above comment on p68. [Philip JONES, UK]	ACCEPTED: text amended accordingly
2-2768	2	83	50	83	50	140-year → 140 year BUT if it is 20th Century renalysis data how can it be 140 years long! [Peter Burt, UK]	NOTED: While this reanalysis product is 140 years long it is referred to as the 20th Century renalysis data
2-2769	2	84	20	84	20	"…also notes…" --> "…also noted…" [Hai Lin, Canada]	EDITORIAL: no change made
2-2770	2	84	24	84	24	The best Wang et al. reference is: Wang, X. L., H. Wan, and V. R. Swail, 2006: Observed changes in cyclone activity in Canada and their relationships to major circulation regimes. J. Climate, 19, 896–915. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: reference has been updated
2-2771	2	84	33	84	33	"…also notes…" --> "…also noted…" [Hai Lin, Canada]	EDITORIAL: no change made
2-2772	2	84	44	84	44	others → other [Peter Burt, UK]	EDITORIAL: amended
2-2773	2	84	55	84	55	"the AR4": remove "the". [Hai Lin, Canada]	EDITORIAL: amended
2-2774	2	84	56	84	56	change text 'However, unlike in AR4, it is assessed here that...' [Peter Burt, UK]	EDITORIAL: amended
2-2775	2	85	5	86	33	The spatial scales and statistical conceptualizations represented in this section are the ones with which the community of authors writing it are familiar. This is a really important question, to which the answer should be a more resounding yes. Broadening the scope of "extreme event" to examine longer time scales and other processes, and analyses that are not global may net quite a few more fish. One example is the Luce and Holden, 2009, doi:10.1029/2009GL039407, mentioned earlier, but I've even seen one that looked at effects of precipitation extremes on Butterflies from the early 2000s. I'm more worried that an overly restrictive conceptualization of the problem is marginalizing pertinent - critically pertinent - literature in favor of literature structured to fairly exacting standards. What I'd really like to see here is a table of 20-50 studies showing trends in non-mean quantities. Four columns should be adequate: citation, processes, [Charles Luce, United States of America]	REJECTED. The remit of the FAQ is to address changes in climate extremes specifically and the suggestions made by the reviewer are beyond the mandate of WGI. References to changes in run-off or stream flow are covered in section 2 but are outside the scope of this FAQ. In addition references to studies which look at impacts of climate extremes are within the scope of WGII and not WGI.
2-2776	2	85	5	86	53	FAQ 2.2: I suggest some further work on the language to make this FAQ clearer to the general reader. This could include some modifications of the initial "summary answer" paragraph to capture more of the findings (e.g. on extreme rainfall) from the body of the FAQ. There are also places where the body of the text could usefully be reworded to make it more immediately understandable. [David Wratt, New Zealand]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2777	2	85	7	85	33	I am not sure what message this FAQ tries to convey. It posts a question without giving a clear answer. All I can learn is it is difficult to define what is an extreme. A natural question I would ask is if you can not define what is an extreme, how can you make an assessment on changes in extreme? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2778	2	85	9	85	9	Replace "For" with "For some". [Robert Waterland, United States of America]	REJECTED. We believe the peer-reviewed evidence for changes in temperature extremes is strong enough to make this conclusion.
2-2779	2	85	11		12	Seems that there should be discrimination between low confidence because data are inadequate vs low confidence because there is no signal. i.e. suggest adding "largely because data are inadequate." [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT. A valid point which we have tried to address in the revised text of the FAQ to discriminate between low confidence from lack of data and low confidence because of lack of signal or conflicting results in the literature.
2-2780	2	85	14	85	15	This sentence should not open this section. It is an aside and distraction to the point at hand. The point is not that there are not consistent definitions, rather that there are many definitions because extremes intervene in many ecological and economic processes is strongly non-linear ways. Both ecological and economic systems are structured to buffer extremes to some depth and to some duration, and different aspects or specific	TAKEN INTO ACCOUNT. FAQ has been substantially re-written and this sentence no longer opens it. However our answer maintains the scope of WGI which means that we do not discuss the impacts of

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						systems are buffered in different ways. This is not the place for excuses, this is the place to describe the fact that consequences to the things we really care about are commonly structured by rare events, and how those rare events are shifting is a problem with multiple dimensions and scales. [Charles Luce, United States of America]	these extremes on e.g. "ecological or economic processes" which is within the remit of WGII.
2-2781	2	85	14	85	15	This is a refreshingly honest remark. [Dian Seidel, USA]	NOTED
2-2782	2	85	23	85	23	insert comma after 'Generally' [Peter Burt, UK]	EDITORIAL. Re-write has removed this sentence
2-2783	2	85	27	85	27	insert comma after 'Primarily' [Peter Burt, UK]	EDITORIAL. Re-write has removed this sentence
2-2784	2	85	29	85	29	delete 'time' (tautology) [Peter Burt, UK]	EDITORIAL. Re-write has removed this sentence
2-2785	2	85	32	85	33	The standard style for the WG1 FAQs is for them to be "stand-alone", ie readable without reference to underlying text, and without references (except sometimes in Figure captions). I suggest the references here to the Nicholls and Alexander paper and the Peterson and Manton paper, and to Box 2.5, should be removed. [David Wratt, New Zealand]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2786	2	85	35	85	35	superscript 'th' (x2) [Peter Burt, UK]	EDITORIAL. Amended.
2-2787	2	85	40	85	41	The statement about heat waves containing consecutive extremely hot days or nights having substantially larger impacts than individual hot days seems to me to be an "impacts" assessment, which is the province of WG2. I suggest it should be removed from this FAQ. [David Wratt, New Zealand]	ACCEPTED. Reference to "impacts" is within the remit of WGII.
2-2788	2	85	43	85	44	"land areas with available data have experienced increased numbers of heat waves since the middle of the twentieth Century." [Dale Hurst, United States of America]	ACCEPTED. Text revised.
2-2789	2	85	45			The WG1 FAQ standard style is for the FAQs to be "stand-alone", ie readable without reference to underlying text. I suggest removal of the reference here to Section 2.7.1 - and if necessary provision of a brief discussion of the "warming hole" in its place. [David Wratt, New Zealand]	ACCEPTED. FAQ has been substantially re-written to account for the "stand-alone" nature of the FAQ.
2-2790	2	85	48		49	may suggest' is almost like saying nothing…. [Larry Thomason, United States of America]	ACCEPTED. This has been changed to "suggests"
2-2791	2	85	49	85	49	10-year → 10 year [Peter Burt, UK]	EDITORIAL. Re-write has removed this sentence
2-2792	2	85	52	85	52	It would be good to update FAQ 2.2. Figure 1a to 2010. [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT. However this figure has now been removed.
2-2793	2	85		85		FAQ 2.2. Is the type of extremes included limited to climate extremes by purpose? Then it could be an idea to add one box on hydrological extremes (soil moisture and runoff). [Lena M. Tallaksen, Norway]	REJECTED: The space for the FAQ is strictly limited so the answer has been restricted to climate extremes on purpose.
2-2794	2	85				FAQ 2.2: Fig 1: A) stippling not visible. Abbreviations used in the caption (eg., WSDI) are not introduced. Consider the inclusion of a wider range of data products. B) units needed. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT. This figure has been removed and a new summary figure has been introduced to include more extremes.
2-2795	2	85				FAQ 2.2: Heavy precipitation could be given more focus in this FAQ, including the good material on extreme snowfalls given in Section 2.3.1.3. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written and a new summary figure has been introduced to try and summarise more extremes. However note that Section 2.3.1.3 only covers average snowfall changes rather than extreme snowfall (as is within the scope of this section). The literature on extreme snowfall events in a global context is minimal.
2-2796	2	85				FAQ 2.2: Consider incorporating some of the material from box 2.5 directly into the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written

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2-2797	2	86	2	86	2	Replace "extremes there are generally less coherent" with "extremes, secular changes are less apparent". [Robert Waterland, United States of America]	REJECTED. Editorial change.
2-2798	2	86	3	86	6	This seems to contradict the summary statement on the same page (lines 49-51) [Dian Seidel, USA]	REJECTED. P86 L49-51 is summarising land-based mean precipitation since 1900. Here we are focussing on extreme precipitation (for which most observations only exist after 1950) and for which the evidence suggests likely increases over many land regions.
2-2799	2	86	4			This statement is meaningless and needs to be spelled out.Please spell out this increases in intensity and increase in drought. Much better in section 2.8. [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2800	2	86	12			By tropical cyclone "activity" do the authors mean both frequency and intensity? FAQ 2.2 Fig 2 shows frequency only. I recall that the AR4 suggested there had been little change in observed frequency overall, but some increase in the frequency of the most intense cyclones. I suggest it would be useful to clearly address both frequency and intensity here. [David Wratt, New Zealand]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2801	2	86	13	86	14	Is there a difference between "tropical cyclones", as stated in text, and "landfalling cyclones/hurricanes", as stated in FAQ 2.2, Figure 2? They don't seem the same, a cyclone does not necessarily have to be landfalling, right? Maybe rephrase. [Birgit Hassler, USA]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written
2-2802	2	86	13	86	15	Please consider including the findings on the tropical cyclone landfalling trend and associated extreme precipitations and high winds in different regions. This section does not have any research results on these aspects. Please note that the ESCAP/WMO Typhoon Committee has also tasked an expert team to conduct the second assessment on the impact of tropical cyclone activity in the Typhoon Committee region with a focus on the possible changes in tropical cyclone track and impact areas, including landfalling statistics/trends. The report will be published later in 2012. [Tsz-cheung Lee, Hong Kong]	REJECTED. This is beyond the scope of the FAQ which should stand-alone without reference to specfic literature.
2-2803	2	86	14	86	16	Please consider including the results of the following studies on the trend of landfalling tropical cyclones in China, From 1949 to 2006, there is a slight decreasing trend in the number of landfalling tropical cyclones in China, but not significant at 5 % level. No trend for the number of landfalling typhoons (Yang et al., 2009). Xiao and Xiao (2010) also reported a slight decreasing trend in the number of landfalling tropical cyclones in China from 1983 to 2008. References : - Yang, Y., M. Ying, and B. Chen, 2009: Climatic changes of landfall tropical cyclones in China over the Past 58 years. Acta Meteorologica Sinica, 67(5):689-696. (In Chinese with English Abstract) - Xiao F.J. and Z.N. Xiao, 2010 : Characteristics of tropical cyclones in China and their impacts analysis, Natural Hazards, 54 (3), 827-837. [Tsz-cheung Lee, Hong Kong]	REJECTED. This is beyond the scope of the FAQ which should stand-alone without reference to specfic literature. However we note the references for other parts of Section 2.
2-2804	2	86	19			The landfalling hurricanes is an extremely small sample and not considered reliable of total storms or possible landfalling storms. Also the adjustments are controversial. [Kevin Trenberth, USA]	TAKEN INTO ACCOUNT. FAQ has been substantially re-written and the Fig. with landfalling storms has been removed and replaced with a summary figure
2-2805	2	86	33	87	11	There is no mention here of trends in LLGHGs, which are significant and, quite frankly, drive the climate change we're all observing. Given that the title of 2.8 is "consistency and conclusions", the continuing rates of increase need to be noted, particularly for CO2, for which the rate has continued to increase as well. Mention could also be made about the trend in the net contribution of fluorocabons (negative over the past 15 years) and their threat for the future, i.e., the anticipated increasing influence of the CFC replacements. Data for these trends of all LLGHGs are remarkably consistent among one another. [James Butler, United States of America]	These points are included in the (enhanced) executive summary, and they are not necessary in this section, consistency of observations.
2-2806	2	86	36	87	11	This illustrates my point well. All these problems do not belong in the greenhouse theory. [VINCENT GRAY, NEW ZEALAND]	Noted
2-2807	2	86	36	87	11	It would be usefule to note the section of the chapter that addresses each bullet in the conclusions. I'd also like to see values and error estimates in the conclusions, or at least a statement regarding the level of confidence. This thing to take into consideration here is how a climate blog wil comment on these conclusions. Are they really the most important points in the chapter. Are these things there is high	Taken into account. This section is about the consistency between observations. No attempt to provide the main conclusions so this word is removed from the heading. Conclusions with likelihood

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						confidence in? Are these the points you want to see pulled out of the chapter into the executive summary for the report? [Karen Rosenlof, United States of America]	statements are in the Executive Summary at the start.
2-2808	2	86	36	87	11	Some uncertainties should be mentioned, such as lack of observed data in some parts of the world and re-analysis data issues. [Zong-Ci Zhao, China]	Taken into account. An additional paragraph has been added to the ES
2-2809	2	86	38	86	47	To what extent do averaged surface temperatures and extremes of temperature represent “independently measured climate variables”? [David Sauchyn, Canada]	Taken into account: independently measured put between brackets
2-2810	2	86	43	86	44	"Globally averaged surface temperatures … all show significant warming trends, as do upper air observations from radiosondes and satellites." Unless "upper air" excludes the stratosphere the last part of this statement is incorrect. [Dale Hurst, United States of America]	Accepted
2-2811	2	86	43	87	11	Style issue. Bullet points should start with lower case letter and each point should end with a ;, except for the penultimater one which should be , and, and the last one with a full stop. [Peter Burt, UK]	Editorial
2-2812	2	86	53	86	55	in summer negative correlation is observed between temperature and precipitation, that’s true but what about the other seasons specially the winter season which the most rainy season in many places in the world, followed by spring season and autuman. [ALI GEATH ELJADID, LIBYA]	Noted, but it is too detailed
2-2813	2	86	53	86	55	About the sentence: "Over land, a strong negative correlation is observed between precipitation and surface temperature in summer and at low latitudes throughout the year, and areas that have become wetter, such as the eastern USA and Argentina, have not warmed as much as other land areas." Please specify, since not all Argentina has became wetter. [Rubén D Piacentini, Argentina]	Rejected. Information is provided for large areas only.
2-2814	2	86	53		55	Wow I was delighted to see this but it is out of the blue and not traceable to other parts of the report (or a reference). [Kevin Trenberth, USA]	Taken into account. Basis added to section 2.3.
2-2815	2	87	4	87	4	Give the start date of the increasing zonal flow period. [Dian Seidel, USA]	Accepted
2-2816	2	88	1	88	62	should literatures assessed in AR4 assessed here again? I saw references as early as 1990s. [Xuebin Zhang, Canada]	In some cases previous references are needed to compare data or to explain some methodology
2-2817	2	88	3	116	7	Reference for paper from AGU Journals are uncompeleted. Paper number should be provided. [Kaicun Wang, China]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2818	2	88		116		All AGU refs (to GRL and JGR) are incomplete. You've missed off the DOI numbers. [Philip JONES, UK]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2819	2	92	27		28	Now published: Volume 12, Issue 4, pages 362–367, October/December 2011 [Julia Hargreaves, Japan]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2820	2	94	17			Fall, S., D. Niyogi, A. Gluhovsky, R. A. Pielke, E. Kalnay, and G. Rochon, 2010 should be 2009 [Philip Lloyd, South Africa]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2821	2	97	46	97	47	This reference should include co-author names or "et al.": Hurst, D. F., S. J. Oltmans, H. Vömel, K. H. Rosenlof, S. M. Davis, E. A. Ray, E. G. Hall, and A. F. Jordan. Journal information to be added: Journal of Geophysical Research-Atmospheres, 116, D02306, doi:10.1029/2010JD015065. [Dale Hurst, United States of America]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2822	2	98	38	98	40	Error in the Jones et al. submitted reference. The authors after Osborn are given as "H.C., S.M.…" and should be "C. Harpham, M. Salmon". [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2823	2	102	52	103	1	The 'Mac' / 'Mc' references are out of alphabetical order, they should start on page 102 line 29 [Peter Burt, UK]	Editorial - DOI values will be included where

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							necessary in the final formatted chapter reference section.
2-2824	2	105	7			It is cecessary to complete the bibliographic reference [José Daniel Pabón-Caicedo, Colombia]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2825	2	106	34	106	35	The missing details of the reference Rajeevan et al are: Vol.35, L18707, doi 10.1029/2008GL035143. [Madhavan Nair RAJEEVAN, India]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2826	2	107	7	107	7	Reynolds et al reference should be J Climate Vol. 15 pp. 1609-1625. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted: correction has been made.
2-2827	2	112	34	112	35	Several mistakes in reference (e.g. middle initials missing, numbers in chemical formula to be subset, page numbers missing etc. Similar mistakes in many other references, I guess somebody will have to carefully go through this. [Martin Vollmer, Switzerland]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2828	2	112	36	112	40	"Vomel" should be "Voemel" or "Vömel". There are 7 occurrences of this in Chapter 2, including these two references. [Dale Hurst, United States of America]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2829	2	113	21	113	21	Please replace "Wang, X." with "Wang, X.L.L." [Xiaolan Wang, Canada]	Editorial - DOI values will be included where necessary in the final formatted chapter reference section.
2-2830	2	117	3	117	3	Table 2.13: "Overview of surface and tropospheric O3 trends …" [Dale Hurst, United States of America]	Section completely updated.
2-2831	2	117				Table 2.13: For North American surface ozone trends using results from Pozzoli et al. you show the highest and lowest ozone trends for winter but do not specify the regions associated with these trends. But for summer data you do specify the regions associated with the highest and lowest trends. Please be consistent and either give the regions for all trend values, or do not give the regions. Also there are some minor errors in the reported values: Central US summer: -0.4 +/- 0.37 should be -0.40+/-0.37 Western US winter: 0.4+/-0.31 should be 0.41+/-0.30 Also the table says that Pozzoli et al use ~100 CASTNET stations to determine regional trends in the US. But they actually use a total of only 48 stations. [Owen Cooper, USA]	Reference to Pozzoli removed and use an updated compilation.
2-2832	2	117				Table 2.13: For European trends by Pozolli et al. 2011, there were 81 European ozone stations, not 100. And, -0.1 +/- 0.28 should be -0.10 +/- 0.23 [Owen Cooper, USA]	Reference to Pozzoli removed and use an updated compilation.
2-2833	2	117				Table 2.13: For N. America surface ozone trends reported by Chan, 2009, you need to note that these are temperature adjusted values. Also, the results from Chan and Vet 2010 are not reported in this part of the table so the reference to this paper should be removed. And in the “Remarks” I don’t know what is meant by “Californian”. Also Chan 2009 does not report winter trends, just annual. [Owen Cooper, USA]	Entry removed
2-2834	2	117				Table 2.13: For Mace Head trends, the data set used by Hess and Zbinden (2011) is for 1990-2009, not 1990-2010. Also, 0.18 +/- 0.33 should be 0.18 +/-0.11. In the remarks it is noted that largest trends are in winter and spring. But nowhere in this paper could I find seasonal trends, just annual. [Owen Cooper, USA]	Correct
2-2835	2	117				Table 2.13: For Central and Northern Europe free troposphere the trends reported by Hess and Zbinden (2011) are for 1990-2009, not 1990-2010. [Owen Cooper, USA]	Correct
2-2836	2	117				Table 2.13: For Europe, free troposphere the remarks regarding Logan et al., 2011 state that the strongest	Correct

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						trends are in winter and summer, but summer is statistically insignificant. [Owen Cooper, USA]	
2-2837	2	117				Table 2.13: For Europe, free troposphere I don't think the trend using all MOZAIC airports should be shown [Logan et al., 2011]. This combined time series has a stronger influence from Paris and Brussels and Vienna in the earlier years, with the final years being dominated by Frankfurt. I think it's better to use the trend from Frankfurt/Munich which are much closer together. This trend is 0.26 +/- 0.10 with the strongest trends being in winter and spring. [Owen Cooper, USA]	Done
2-2838	2	117				Table 2.13: For Jungfraujoch 0.99+/-0.45 should be 0.00+/-0.46 and needs to be labeled as winter. 0.69+/-0.80 needs to be labeled as summer. [Owen Cooper, USA]	Corrected
2-2839	2	117				Table 2.13: Zugspitze and Jungfraujoch are listed as free tropospheric sites. While these sites do protrude above the boundary layer much of the time, technically they are not truly free tropospheric as there will always be some influence from air flowing over the surrounding peaks (unless the data have been filtered to remove events with surface interactions). Therefore it would be best to refer to these sites as "alpine". [Owen Cooper, USA]	Done
2-2840	2	117				Table 2.13: For N. America surface sites of Yreka and Lassen you assumed annual average ozone at these sites of 33 and 42 ppbv, respectively. Where did you get these numbers? Did you just eye-ball them from Figure 20 of Oltmans et al., 2008? [Owen Cooper, USA]	Yes.
2-2841	2	117				Table 2.13: For N America Surface, some of the reported ozone trends at the 8 pacific US stations are incorrect. They should be: Winter 0.43+/-17 Spring 0.46+/-13 Summer 0.24+/-16 [Owen Cooper, USA]	Corrected
2-2842	2	118	1	118	1	"Japanese" is misspelled. Why is there a question mark after "effect"? [Dian Seidel, USA]	This entry has been changed
2-2843	2	118				Table 2.13: For Asia Surface the entry for Mt. Happo needs to be split in two. For the first part give the springtime ozone trend at Mt. Happo and give the site's elevation. In the second part state that 8 low elevation remote sites were analyzed. All showed positive ozone trends over 10 year (1998-2007), but only one was significant. For both of these parts the reference is Tanimoto et al., 2009, GRL. The reference to Tanimoto 2009, Atmos. Environ. should be removed from the table because specific results from this paper are not cited. [Owen Cooper, USA]	This entry has been changed
2-2844	2	118				Table 2.13: For Asia, troposphere you cite Beig and Singh (2007) and state that tropospheric column ozone above continental south Asia is increasing at the rate of 0.7-0.9 % per year. But this is just the maximum value. Their figure 1 shows that across broad regions of south Asia the rate of increase is more like 0.3-0.7% per year. [Owen Cooper, USA]	Corrected
2-2845	2	119		120		In Box 2.4 Table 1, the years of record would be a very useful addition. [Philip JONES, UK]	Rejected. The goal of this table is to provide a definition of indices that can be applied to any suitable data set. Where these definitions are used for calculation of indices (Figures 1-2 of Box 2.4 and Table 2.14), the data sets used and their time periods are specified.
2-2846	2	119		122		Box2.4 Table 1. Should make it clear that the indices are possible measures of climate phenomenon and are NOT the actual phenomenon. The indices do not always measure the climate phenomenon in isolation, and some are looking for a real phenomenon to be associated with. [Gareth S Jones, UK]	Rejected. This is explained in the text of the Box 2.4 (now 2.5), cannot be explained within this table.
2-2847	2	119		122		The AMO is not a measure of Atlantic ocean thermohaline circulation. The AMO is a measure of SSTs in the Atlantic that can be potentially influenced by changes in AOTC but can be influenced by radiative forcing (e.g. aerosols Booth, Nature 2012). Fig3.12 shows index of the AOTC. [Gareth S Jones, UK]	Accepted. Heading changed.

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2-2848	2	120		121		In Table 1 for Box 2.4, when referring to Barnston & Livezey (1987) in rightmost column, NH Z500 must be replaced by NH Z700 - the authors studied the 700 hPa field, not 500 hPa one. [Andrey Shmakin, Russia]	Accepted.
2-2849	2	120		121		In Table 1 for Box 2.4, when referring to Barnston & Livezey (1987) in rightmost column, NH Z500 must be replaced by NH Z700 - the authors studied the 700 hPa field, not 500 hPa one. [Andrey Shmakin, Russia]	Accepted
2-2850	2	122		122		Box 2.4, table 1. The row describing the Atlantic Multidecadal Oscillation (AMO) index is described as the "Atlantic Ocean Themohaline circulation" climate phenomenon. This should be renamed something like "Atlantic multidecadal SST variability" or similar. The AMO is an index of SST change. Based on some climate model control simulations, it has been suggested that observed multidecadal SST variability is driven by changes in the Atlantic meridional overturning circulation (AMOC) (e.g. Knight et al., GRL, 2005). However, other studies have suggested that the AMO is driven partly by changes in radiative forcing (see lines 1-11 page 33, chapter 10). There are currently no multi-decadal observations of the AMOC, so it is impossible to say with certainty that multi-decadal SST variability is associated with variations in the AMOC. Sustained monitoring of the AMOC (e.g. Cunningham et al., Science, 2007), will be required before there is observational evidence for links between the AMOC and SST variability. [Chris Roberts, Uk]	Accepted. Heading changed.
2-2851	2	123	1	123	55	All forms of linear trends are inappropriate and irresponsible since they exaggerate the importance of the least reliable obervations and conceal the iregularities and periodicities of climate time series. It is deplorable that you do not consider these important matters [VINCENT GRAY, NEW ZEALAND]	Rejected. The claim about exaggerating least reliable data is unsubstantiated, while simplified descriptions (e.g., linear trends) of anything always "conceal" something. See also 2-2922.
2-2852	2	123	1	123	57	This Appendix 2.A needs work, obviously, but more than I can do now. [Dian Seidel, USA]	Noted
2-2853	2	123	1			Appendix 2.A: this appendix needs rewriting to *discuss* and *explain* these methods and their differences, at a non-expert scientific level, rather than just describing them. Need to get across some understanding of how these methods differ conceptually. Also need to discuss how these methods relate to detection (cross-referencing and being consistent with the D&A chapter). What about higher order AR models? Is there evidence to discount them? Again perhaps there is evidence that can be referenced to the D&A chapter. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Rejected. The idea of this appendix is to provide the details to technically-minded readers. The developed discussions are for the main text, not for appendices.
2-2854	2	123	7	123	7	Avoid "AR1" due to confusion with the first assessment report rather than 1st-order autoregressive model. Use AR(1) instead. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2855	2	123	26	123	26	"taking into account uncertainties" would be more informative to say what the uncertainties are, e.g. "taking into account observational uncertainties". [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2856	2	123	46			please introduce the meaning of / in the equaions it is needed for understang the next tables in page 124 and 125 [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Accepted
2-2857	2	123		123		I enjoyed to read this Appedix. It is very useful, but this kind of writing cause readers to confuse. You have shown the equation in difficult way . In addition you have presented a number of equation and then introduceed rho, u, ….. Of course I know it is usual for quations. but it is good for one formula, two formula not more. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Accepted
2-2858	2	124	21	124	23	you have presented the result of sen and ws2001 as a equation with three value. The third one is not rm, and it relates to equation, but the reader misunderstand. Please show it in modified shape. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Editorial. The format is tr+u-d, where u and d are upper and lower parts of 90% confidence interval (explained on p.123, l.46 & l.52)
2-2859	2	124	49		50	Same as21 [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Editorial. Same as 2-2858
2-2860	2	124				Figure 2.7. The lower plot is described first and the upper plot second. I found the terminology "straight average" a little unusual. [Elizabeth Kent, England]	Misplaced comment. Page number should be 134, not 124
2-2861	2	125	1	125	4	Same as21 [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Editorial. Same as 2-2858
2-2862	2	125	21	125	24	Same as21 [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Editorial. Same as 2-2858

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2863	2	127	1	127	8	Box 2.2. Fig 1. The top graph is quite a big step back in format from previous IPCC reports. The annual error bars are essential and are missing from this dot format, where the barred coloured format of previous IPCC reports for HadCRUT probably works best as well - as extensively used outside IPCC for climate change communication. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Considered, see responses to comments 2-375 to 430
2-2864	2	127	1			Box 2.2, Figure 1: state the length of the spline used in the lower panel. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	See responses 2-375 to 2-430
2-2865	2	127	5	127	7	Illustrates the absurdity of "trend lines". No attempt is made to examine the reasons for the variability, which are partly due to poorer reliability of earlier data.. No plausible figures for uncertainties are given. They may exceed the variabilities shown [VINCENT GRAY, NEW ZEALAND]	See responses 2-375 to 2-430
2-2866	2	127				Box 2.2, Figure 1: Add in figure caption an explanation of what data exactly is shown as global annual mean [Birgit Hassler, USA]	See responses 2-375 to 2-430
2-2867	2	127				Box 2.2, Figure 1: add units to the description of the y-axis (temp anomaly) [Birgit Hassler, USA]	Corrected.
2-2868	2	127				Box 2.2, Figure 1: On y-axis, temp may be written as Temp. On x-axis, time may be written as Time. [Yutaka Kondo, Japan]	Corrected
2-2869	2	127				Fig 1 Upper panel: misleading comparison of short and long trends. AR4 was widely criticised for doing this (inserted after review). Lower panel is fine. Delete upper panel. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	See responses 2-375 to 2-430
2-2870	2	127				These figures are not complex and don't cover the box, therefore if you show here the trend line for the periods without assuming the errors, the readers may see the difference between them. I suppose it can clarify this box and also the box related to method of estimation [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	See responses 2-375 to 2-430
2-2871	2	127				Add units, and a better label, to the vertical axes of both plots. Add "temperature anomaly" after "global annual mean" to the caption. [Dian Seidel, USA]	Corrected.
2-2872	2	127				Fig 2.1, 2.4, 2.7. 2.12: We would recommend for these figures that the sequence of the panels is changed, so that the upper panel shows the mean anomaly, and the lower panel shows the inter-model anomalies. This ordering would then be consistent with the captions, which first refer to the mean anomaly. Suggest also that the titles on the y-axis is made more specific to avoid any confusion between the two plots, eg, 'Inter-model anomaly' and 'Global mean anomaly'. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. We decided in the authors meeting to greatly simplify these diagrams. They now consist of solely the timeseries of anomalies (relative to a common base period) and annual means. Labels on axis etc. have been modified accordingly.
2-2873	2	128	1	128	1	It would be great to have some kind of uncertainty spread on graph b (beyond showing the structural uncertainty in a) [Marcus Sarofim, USA]	Taken into account. See response to 2-2872
2-2874	2	128	1	128	11	The top panel is not an anomaly….it's a difference. This comment also applies to Figure 2.4, and 2.7 [Karen Rosenlof, United States of America]	Taken into account. See response to 2-2872
2-2875	2	128	1	128	11	I would like to see all series not just average and differences from it maybe in 3rd pane . In any case do not show GHCN in black as this confuses given multi model mean also in black, black currently means two diffeent things in two plots [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2872
2-2876	2	128	1	128	11	Please start the graph at 1850 or 1880 so that the error bars on the period 1800-1850 do not dominate the plot rather than the trends over 1900-2010. [Geert Jan van Oldenborgh, Netherlands]	Accepted. We now start the timeseries plots for the surface in 1850 and the trend tables in 1880.
2-2877	2	128	1			Figure 2.1: give the versions of each dataset. E.g. is this CRUTEM3 or CRUTEM4? Also caption states they have been smoothed with a digital filter to emphasize variations on interannual timescales -- but doesn't say what timescales these are emphasized against? Have shorter or longer timescales been removed? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted. The timeseries always use the latest version of the data products - so CRUTEM4 in this case. We have tried to make this clearer in the redraft. See 2-2879 for resolution of filter issue.
2-2878	2	128	4	128	10	Again noi interest shown in the variabilities. It should be noted that a temperature increase as small as 1ºC over such a long period is hardly likely to be disastrous.No plausible figures for uncertainties are given. They may exceed the variabilities shown. Also the absence of an increase in all of the figures since 2001 has been	Noted. No specific changes to the figure were requested by the reviewer so none have been made.

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						concealed. [VINCENT GRAY, NEW ZEALAND]	
2-2879	2	128	8	128	9	Need to give details of the smoothing used. [Gareth S Jones, UK]	Noted. It has been decided to change the plotting to annual means so the filtering will not be present in the next draft.
2-2880	2	128	9	128	10	If applying a land mask to the GISS data why not go all the way and mask with the actual station coverage to compare "like-for-like"? Should at least explain further the reasons for this choice. [Gareth S Jones, UK]	Noted. This is the version GISS requested that we use. We use the latest products from each group as they calculate them as the calculation method is part of the structural uncertainty and because to do ourr own calculations would add unnecessary confusion.
2-2881	2	128		128		Figure 2.1 ought to show error ranges. See earlier comments about Berkeley pre-1850 being almost all European data. [Philip JONES, UK]	Taken into account. See response to 2-2876
2-2882	2	128				I had a lot of difficulties to understand the figure. Anomaly of the top panel, between 1850 and 1890, Berkeley is a mirror image of Crutem. I don't believe it's just luck so how is that possible? Then I understood that the top panel is not anomaly but difference! And as you use anomaly (instead of difference) on so many following figures, I assume it's not a mistake so I'll spend some time on that comment. Anomaly, for me in the context of climate change, is a drift from a normal (pre- industrial) situation. I agree that, in another context, it would be a difference from an average and it is the case of your figures. [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2883	2	128				Part 2: So you are absolutely right in your choice of wording but I would ask you to change the word for the following reason: with temperatures, a lay reader expects the anomaly to be an increase in the troposphere (and for the well informed, a decrease in the stratosphere). Imagine a climate skeptic using just the top panel at a meeting... signed IPCC; where is the temperature increase? The word “difference” has been used on AR4 instead of anomaly so you don't want to use “difference” either? Either you could precise that “difference” is not used in AR5 in the same way than in AR4; or what about offset? [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2884	2	128				Part 3: In addition, on that legend, (part of my difficulty to understand the figure) I concentrated on the meaning of “multi-dataset mean series behavior" (that I still don't understand, multi-dataset average?) and I forgot the important word: offset. [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2885	2	128				Part 4: To finish, you use that type of figures very often... so, for lay readers (see comment page 2-12 line 32), on that first figure, the legend should be very clear and I would add more about why you display results that way. Suggestion: top panel is there to show the uncertainties; the amplitude being close to the uncertainties... the change in the bottom panel is much bigger than the uncertainties (notice the change in scale), so it shows the change is real... [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2886	2	128				Figure 2.1: There is no uncertainty information on the diagram. It could be argued that structural uncertainties are perhaps larger than uncertainty estimates made by individual groups, but including that information on the diagram would serve to highlight that point all the more clearly. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected. It is difficult to conceive how to incorporate and discuss such estimates which are derived very differently and have distinct maturity between the various datasets. We could include ensemble uncertainty estimates if enough existed but this again has an issue that they are not like-for-like comparisons. Such ensembles are included in FAQ 2.1 in some cases.
2-2887	2	128				Figure 2.1: The anomaly baseline is not mentioned. This problem affects a number of other figures. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. We will include climatology periods in the legend to all graphics where this is an important consideration.
2-2888	2	128				Figure 2.1. All available datasets available. I think the caption is trying to say that the number of datasets varies prior to 1880 rather than the mean varies prior to 1880. On lines 9&10 I didn’t understand the description of what had been done to the GISS data regarding the land mask. [Elizabeth Kent, England]	Accepted. Changes have been made to clarify.
2-2889	2	128				Fig 2.1 This figure (and many others in the chapter, 2.4, 2.7, 2.12) is confusing. Two different things are	Taken into account. See response to 2-2872

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						shown, mean anomoly and deviation from mean anomoly, but both are just labelled 'anomoly'. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	
2-2890	2	128				Fig 2.1 Caption: “smoothed with a digital filter” is not adequate. Enough detail must be given for reproducibility. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2879
2-2891	2	128				Figure 2.1: Which are globals and which are land surface temperature. If obvious it should be tell in the figure caption. [Gillles Molinié, France]	Taken into account. See response to 2-2872
2-2892	2	128				The disparity between the CRUTEM and Berkeley time series in the top panel of Fig. 2.1 deserve comment in the text or caption. Consider adding the range of datasets to the bottom panel; this will serve to show the relative agreement, which is not obvious from the top panel because of the different vertical scale. This suggestion also applies to Figs. 2.4, 2.7, and possibly others, where multiple versions of time series are shown. In the caption, explain (or remove) "digital filter". [Dian Seidel, USA]	Taken into account. See response to 2-2872 and 2-2876
2-2893	2	129	1	129	1	Axes legend suggests that it is showing the anomaly relative to 1961-1990 - and yet, the light green plot clearly has a non-zero 1961-1990 average. If the axis legend only applies to the black line, that should be made clear… [Marcus Sarofim, USA]	Accepted: explained better
2-2894	2	129	1			Figure 2.2: lower panel is lablled as being anomalies relative to 1961-1990, but the curves (e.g. green one) do not have zero mean for 1961-1990. Perhaps it is a reference level localised in space but pooled across measurement types? Perhaps need to make that clear to avoid questions later. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted: explained better
2-2895	2	129	4	129	8	Merely shows how unreliable this procedure is [VINCENT GRAY, NEW ZEALAND]	Rejected: unclear what procedure is meant.
2-2896	2	129	5	129	5	Define "ERI" [Dian Seidel, USA]	Accepted. Now defined in the figure legend.
2-2897	2	129				Figure 2.2: The drifting buoy data have been reduced to the coverage of (i.e. colocated with) the ship data, but not vice versa. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted: explained better.
2-2898	2	129				Figure 2.2. I initially thought this figure was wrong, but then realised that the time scales on the upper and lower panels were different. It might avoid such confusion if the two plots were aligned in time. [Elizabeth Kent, England]	Accepted: figure is remade
2-2899	2	129				Fig 2.2 Why different shape of green curve in lower panel ? Why cooling in 1970s and much less warming overall ? Does this mean hull contact sensor is a really bad way of measuring temps or is it related to changing coverage. Would be good to discuss this in text. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Capton clarified. Green curve is dominated by ERI, not hull measurements.
2-2900	2	130	1	130	1	The lack of warming in the last decade is mentioned in both Ch2 and Ch 10. However in figure 1 of box 2.2 the lack of warming is not visible due to smoothing. I encourage you to update at least this figure 2.3. An alternative is to show the 9 years of AMSR, see e.g. http://www.drroyspencer.com/wp-content/uploads/AMSRE_SST_2002_thru_July_7_2011.gif [Marcel Crok, The Netherlands]	Rejected. Figure 2.3 shows time period of the record considered. Figure 2.4 is up to end of record.
2-2901	2	130	1			Figure 2.3: captions says that a month is missing in 1996, but the larger gap in 1992 is not mentioned. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure 2.3 is remade (as the one better suited to illustrate the discussion) and its discussion is rewritten.
2-2902	2	130	4	130	6	Illustrates the absence of significant change since 2001 [VINCENT GRAY, NEW ZEALAND]	Noted
2-2903	2	130	6			You report the gap in 1996, but what about the gap in 1992? [Francois DANIS, France]	Taken into account. See response to 2-2901
2-2904	2	130				Figure 2.3: It would be easier to read this graph if it displayed the difference between the series. [Peter Guttorp, USA]	Rejected. It would be less useful for the reader. Figure 2.3 is remade anyway.
2-2905	2	130				Figure 2.3: It is mentioned why there is a gap in the ARC data in 1996, but not mentioned why there is a gap in both data sets (ARC and HadSST3) in 1992. Add an explanation? [Birgit Hassler, USA]	Taken into account. See response to 2-2901

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2-2906	2	130				Figure 2.3. Add "realisations" after 100 [Elizabeth Kent, England]	Editorial
2-2907	2	130				The cryptic notes at the bottom right of the figure can be removed. [Dian Seidel, USA]	Taken into account. See response to 2-2901
2-2908	2	130				Fig 2.3: The caption highlights the ARC gap in 1996, but there is also a large gap in both datasets following 1992 which lacks an explanation. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. See response to 2-2901
2-2909	2	131	1	131	8	Please decribe the 4 panels in order. Also consider difference colors besides orange and red. [Karen Rosenlof, United States of America]	Taken into account. See response to 2-2872
2-2910	2	131	1			Figure 2.4: probably easier to write the caption if you labelled the panels (a)-(d). Currently (c) is not described at all, and (a) and (b) descriptions are confused. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2872
2-2911	2	131	4	131	5	Not “anomalies” but offset! See comment page 2-128. [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2912	2	131	4	131	7	Evidence of unreliability. Supposed "trend: may not eist. [VINCENT GRAY, NEW ZEALAND]	Rejected. Trend meaning is discussed in Box 2.2
2-2913	2	131	4	131	7	The caption is a little confusing, as it isn't clear which is the "top panel", since it is divided into three sub-panels. [Nick Rayner, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2872
2-2914	2	131	4		7	The figure caption is not coherent with the 4 panels : top = ICOADS and MOHMAT43N ; second HadSST2 and JadSST3 ; third COBE, ERRST and HadISST. The lower panel is correctly described [Jean Poitou, France]	Taken into account. See response to 2-2872
2-2915	2	131	5	131	7	I believe that HadSST3 is a product that provides realisations sampling observational uncertainties. What is shown here? The mean, median ...? [Gareth S Jones, UK]	Accepted. Median.
2-2916	2	131				Figure 2.4: the different orange/red curves are hard to distinguish in the graph. Maybe change some of the colors? [Birgit Hassler, USA]	Taken into account. See response to 2-2872
2-2917	2	131				Figure 2.4: The caption is confusing. It might help to label the separate panels and refer to them in order. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2872
2-2918	2	131				Figure 2.4: There are 100 realisations of the HadSST3 data set, but only a single line is shown here. There is space on the diagram to show the uncertainty associated with the bias adjustments. It would help to put the relative size of the bias uncertainties in to context. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected. The figure is remade, showing 100 realizations in the new version will make it harder to read and will break the uniformity across figures of this type within the section.
2-2919	2	131				The key is misleading, because the black curve is COBE data in the third panel, but it is the mean of all datasets in the fourth panel. The fourth panel should show the range of all datasets. The caption needs to be more specific than "lower" and "top" panels, because there are four, not two. Change "on" to "of" in line 4. [Dian Seidel, USA]	Taken into account. See response to 2-2872
2-2920	2	132	1	132	9	The figure is too small to tell what is dotted and what is solid. [Karen Rosenlof, United States of America]	Taken into account. The figure is excluded.
2-2921	2	132	1			Figure 2.5: needs to be made bigger because the dotted contours are not legible. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Combined with 2-2920
2-2922	2	132	4	132	8	Linear trends are completely unsatisfactory as they exaggerate the least reliable data and conceal irregularity and periodicity. [VINCENT GRAY, NEW ZEALAND]	Rejected. The claim about exaggerating least reliable data is unsubstantiated, while simplified descriptions (e.g., linear trends) of anything always "conceal" something.
2-2923	2	132		132		I noticed an inconsistency in global maps - sometimes the Dateline is in the middle and other times it's the boundary. It would help people like me if there were a consistent geographic representation so we could directly compare maps without phase-shifting the longitudes in our brains. [John Christy, USA]	Taken into account. Combined with 2-2920
2-2924	2	132		132		"higher than 90% confidence" What does this mean? I presume it doesn't mean that there is >90% confidence	Taken into account. Combined with 2-2920

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						that the trend is the actual value shown? What is the null hypothesis? [Gareth S Jones, UK]	
2-2925	2	132				Figure 2.5: It would be good to make the indications of 'significance' consistent as far as possible with those in Figure 2.8. The areas that are blanked out are those close to zero trend, so the procedure is of questionable usefulness even leaving aside the problem that the definition of significance is model dependent. In fact Figure 2.13 does not include this significance information at all which suggests it is not essential. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. The significance is shown in a consistent way across the Chapter. (Although this Figure is excluded).
2-2926	2	132				Fig 2.5: Misleading comparison of short-term and long-term trends. The trends (both warming and cooling) are much larger for the diagrams on the right, as would be expected for any fluctuating signal. This is an elementary statistical error made repeatedly in this chapter. Plot the 1901- trends only. Then the figures will be larger, the colours will work better and you won't be liable to accusations of cherrypicking and false comparisons. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Rejected. What's elementary for this reviewer, can still provide a useful demonstration of climate variability properties for the readers of the AR5. But the figure is dropped anyway.
2-2927	2	132				Fig 2.5: "linear trend slope estimate". This was not clear to us - could this be stated more simply as "linear trend"? [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. Use of expression "Linear trend" would be inconsistent with the definition of "trend" in Glossary. But this figure is dropped anyway.
2-2928	2	133	1			Figure 2.6: a nice way to present it! [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted
2-2929	2	133	4	133	8	The uncertainties are unbeliebvable because they assume the multiply averaged observations from uncertain original obsrvations as being constants, whereas they should add a substantial amount to the eventual uncertainties. [VINCENT GRAY, NEW ZEALAND]	Rejected. The uncertainty model is thoroughly documented in Morice et al., 2012.
2-2930	2	133	7	133	7	delete "would" [Karen Rosenlof, United States of America]	Accepted
2-2931	2	133				Figure 2.6. I found the legend a little confusing as I was looking for lines to define the range of uncertainty rather than the size of the coloured bars. Suggest also removing dashed borders to legend colour blocks [Elizabeth Kent, England]	Accepted. Changes have been made along the suggested lines.
2-2932	2	133				Fig 2.6 Remove this ridiculous figure. No such figure (with temperature binned into decades) was used in any of the previous IPCC reports (nor have I seen it in publications), so why is it being used now? The answer, sceptics will say, is to try to 'hide the decline' in warming this century. For this to appear in the final report would be an own goal and expose the IPCC to ridicule. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Rejected. It has been used in several peer reviewed publications and WMO reports.
2-2933	2	133				This is a nice plot. Why not show time as the horizontal axis, as is done for most of the other plots in the chapter? Also, consider adding a symbol for the HadCRUT4 results, not just a range, for two reasons: (1) it would help show the spread of the 3 datasets, and (2) it would avoid confusion that there are just 2 datasets. Also, it doesn't seem that the mean of the 1960s, 70s, and 80, is a zero anomaly, as the caption suggests. [Dian Seidel, USA]	Noted. HadCRUT4 is available as an ensemble product so it is hard to give a best estimate for each decade. We prefer the axes in this arrangement.
2-2934	2	133				Surprised that the error bars in the 1850s are not wildly bigger than those much later in the period. [Larry Thomason, United States of America]	Noted. The error model is described in the paper cited.
2-2935	2	133				Fig 2.6: Suggest that uncertainty is also shown for GISS and NCDC. It might be useful to also overlay the 30 year trends. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. This would make the figure unnecessarilly busy and detract from the key message. A lot of iterations with communications experts went in to this incarnation of the figure.
2-2936	2	134	1	134	6	Note which panel is what in caption. Also, see comment #72 [Karen Rosenlof, United States of America]	Taken into account. See response to 2-2872
2-2937	2	134	1			Not “anomalies” but offset! See comment page 2-128. [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2938	2	134	4	134	5	Again no interest in the fluctuations or in the change in reliability of the final data. Still the overall rise of less than 1ºC since 1850 is hardly disastrous, even if it is true.. [VINCENT GRAY, NEW ZEALAND]	Noted. No specific changes were requested.
2-2939	2	134				Figure 2.7: No baseline given for the anomalies. [John Kennedy, United Kingdom of Great Britain & Northern	Taken into account. See response to 2-2887

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						Ireland]	
2-2940	2	134				Figure 2.7: Once again, no uncertainty information has been included in the diagram beyond the structural uncertainty. The three data sets all have estimated uncertainty ranges which should be shown and discussed. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2886
2-2941	2	134				Add range of datasets tp bottom panel. [Dian Seidel, USA]	Taken into account. See response to 2-2872
2-2942	2	134				Fig 2.7 Again don't use black for both a particular series and for mean of all series. Suggest use different colour for MLOST. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2872
2-2943	2	135	1	135	1	It would be better to highlight the insignificant trends…would make it easier to see the colors plotted. [Karen Rosenlof, United States of America]	Noted. We had long discussions in Marrakech on this issue and decided to retain the current approach.
2-2944	2	135	1	135	8	I know it goes against IPCC tradition,but I always find it much clearer to indicate areas where the trend is smaller than 2 standard deviations with hatching; this draws the eye to the brightly-coloured significant regions rather then the insignificant regions. [Geert Jan van Oldenborgh, Netherlands]	Taken into account. See response to 2-2943
2-2945	2	135	1			Figure 2.8: need the word "temperature" somewhere in the caption! [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2946	2	135	4	135	7	More inappropriate trends but it does show there has not been much change since 1901 [VINCENT GRAY, NEW ZEALAND]	Noted. No specific changes were requested.
2-2947	2	135	5	135	6	I am curious to know how the uncertainties on the trend with an AR1 model is calculated with missing data ("greater than 70% complete records"). Is there a reference? [Gareth S Jones, UK]	Taken into account. This is documented within the appendix.
2-2948	2	135				Figure 2.8: Significance information should be made consistent with Figure 2.5 or (preferably) removed as in Figure 2.13. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2943
2-2949	2	135				Figure 2.8: The colour scale does not work very well for the left hand plot. A separate scale for the two, or a modified scale with more gradations close to zero would help make the diagrams more informative. It would be useful to make this consistent with Figure 2.5 and Figure 2.13 as far as possible. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. This choice was deliberate to enable direct intercomparison and we are loathed to change it from that. Colour schemes are now consistent and Figure 2.5 has been dropped.
2-2950	2	135				Figure 2.8 (caption). The caption may be written as " Global temperature trend maps…. [Yutaka Kondo, Japan]	Taken into account. See response to 2-2945
2-2951	2	135				Fig 2.8 Misleading comparison again. [Paul Matthews, United Kingdom of Great Britain & Northern Ireland]	Noted. No changes requested or made.
2-2952	2	135				Figure 2.8: In my opinion, it would be best to have a uniform way of indicating statistical significance in these type of spatial plots in WGI. This plot (the first of it's kind I believe) has the black crosses indicating 'significant', while Annex I (the Atlas) has fairly similar black hatching meaning 'not significant'. [Drew Shindell, USA]	Taken into account. See response to 2-2942. We have ensured within chapter consistency.
2-2953	2	136	1	136	2	"*G" is not identified and I don't think it is explained in the text. [Melissa Free, USA]	Taken into account. See response to 2-2955
2-2954	2	136	5			Caption needs a citation to the source of this figure. [Dian Seidel, USA]	Accepted
2-2955	2	136				Figure 2.9. The abbreviations in the legend at the top (LT, *G etc) is not described anywhere. Some acronyms are in the text but not all. [Elizabeth Kent, England]	Accepted. We have modified the caption and text that cites it accordingly.
2-2956	2	137	1	137	10	Please also indicate for reference the correspondingnear-surface temperture trends of the datasets considered in 2.2.3 on the X axis [Geert Jan van Oldenborgh, Netherlands]	Noted. This figure has been removed.
2-2957	2	137	5	137	22	Replace (Thorne et al., 2010a) => Thorne et al. (2010a) [Yutaka Kondo, Japan]	Taken into account. See response to 2-2956
2-2958	2	137				Figure 2.10: Why does the period for which the trend was calculated end in 2003? Maybe explain in text which dataset spans which time period? [Birgit Hassler, USA]	Taken into account. See response to 2-2956

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2959	2	137				Figure 2.10. Best estimates? [Elizabeth Kent, England]	Taken into account. See response to 2-2956
2-2960	2	137				This figure raises several questions. (1) Why end the trend period in 2003, when most of the datasets are up-to-date. (2) Why not include RATPAC among the datasets analyzed? (3) Why use the median of pairwise slopes method for upper-air temperature when the chapter has other recommended methods? Will the new versions of RICH and RAOBCORE be citable? [Dian Seidel, USA]	Taken into account. See response to 2-2956
2-2961	2	138	4	138	1	What is RO_AMSU? Is it GPS data vertically weighted to mimic AMSU observations? If so, state it clearly. [Karen Rosenlof, United States of America]	Noted. In trying to squeeze into length stipulations this figure has been removed.
2-2962	2	138	4	138	6	Overwhelming evidence that there has been no significant temperature change for the past ten years [VINCENT GRAY, NEW ZEALAND]	Taken into account. See response to 2-2961
2-2963	2	138				Figure 2.11: What does the blue dashed line represent? [Birgit Hassler, USA]	Taken into account. See response to 2-2961
2-2964	2	138				Why is it important to show 6 zonal time series for a 10-year period? This figure could be eliminated if the RO data were added to Fig. 2.12. The caption should make clear that these are temperature data; TLS should be spelled out on the axis labels. Also, what is meant by the "mean trend" - mean of all the trends or trend in the mean time series - and what method was used to compute it? [Dian Seidel, USA]	Taken into account. See response to 2-2961
2-2965	2	139	1	139	2	I find the difference plots harder to interpret than the traditional approach showing the actual datasets together on one plot. Red, orange and pink colors are hard to distinguish. [Melissa Free, USA]	Noted. We will work on the colours. See also response to 2-2872
2-2966	2	139	1	139	10	AN offset is not anomaly..label it as something different. Also, caption should clearly denote what is in the top panel and what is in the bottom panel. And oranges and reds are difficult to see. [Karen Rosenlof, United States of America]	Taken into account. See response to 2-2872
2-2967	2	139				“offset” and not “anomaly”? Also, top panel, I observe an anticorrelation between offset variation of HadAT and all other traces... Indeed, other offsets seem very much correlated. I didn't spend much time looking for a reason for it in the text, but couldn't find it. If there is a reason (2 different raw datasets?) it would be useful to say so. [Francois DANIS, France]	Taken into account. See response to 2-2872
2-2968	2	139				Figure 2.12: uncertainties of mean anomalies might be helpful to see [Birgit Hassler, USA]	Taken into account. See response to 2-2872
2-2969	2	139				Figure 2.12: As mentioned in comment 15 and 16, it would be helpful to have for example decadal means added to the plot so that the statements 15 and 16 can be verifyed easier with the graph [Birgit Hassler, USA]	Taken into account. This text has been removed in response to other comments so this comment is no longer applicable.
2-2970	2	139				Show the range of datasets in the bottom panels of both plots. I'm not sure I agree with the choice of "lower tropospheric" temperature, rather than the more straightforward "tropospheric" temperature time series. For the globe, the issue of stratospheric influence on the nominal tropospheric layer is not as important as it is in the tropics, where the tropopause is high. Also, consider changing "anomalized" to something like "plotted with respect to" in the caption. [Dian Seidel, USA]	Noted. We debated and rejected switching the plot to MT. See also response to 2-2872
2-2971	2	140	1	140	7	How are lower troposphere and lower stratosphere defined? This may very well be better to do with MSU data rather than reanalysis (and does ERA-Interim actually extend back to 1979?) [Karen Rosenlof, United States of America]	Accepted. We will use MSU data following significant author team discussions.
2-2972	2	140	4	140	5	More "trends" which fail to note that they are explained by greater volcanic activity at the beginning and greater influence of upwards ocean oscillations at the end [VINCENT GRAY, NEW ZEALAND]	Noted. Comment relates to subject matter of other chapters and is not relevant here. No changes made.
2-2973	2	140				Figure 2.13. State explicitly that this is temperature [Elizabeth Kent, England]	Accepted
2-2974	2	140				Figure 2.13 (caption). The caption may be written as " Linear temperature trend…. [Yutaka Kondo, Japan]	Taken into account. See response to 2-2973
2-2975	2	140				As commented above, I really advise against using this plot of trends based on ERA-Interim. [Dian Seidel, USA]	Taken into account. See response to 2-2971

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2-2976	2	141	1	141	2	Need to include RATPAC, and it would be helpful if different symbols could be used for the different datasets. [Melissa Free, USA]	Noted. Following discussions with the curators of this dataset we now have data for the same diagnostics as have been calculated for other products. This was the sole reason that they were not used last time around. We tried using different symbols but this was even more confusing. We have tweaked the colors.
2-2977	2	141	4	142	4	An interesting demonstration that the MSU temperature measurements are influenced by the temperature profile in the atmosphere, not by radiation changes [VINCENT GRAY, NEW ZEALAND]	Comment is a non-sequitor. Nowhere had we suggested that the latter was the case so we are unsure why the reviewer is choosing to make this comment or what changes they expect as a result. No changes made.
2-2978	2	141				Figure 2.14: Maybe show additional x-axis on top of the graphs to make it easier to estimate the MSL trend values out of the plot [Birgit Hassler, USA]	Noted. We tried this but it looked very messy.
2-2979	2	141				Figure 2.14: Maybe add error bars at least to the MSL trend estimates to strengthen the point of significant trends for the different layers [Birgit Hassler, USA]	Noted. It is unclear what error bars the reviewer is requesting. We played with these a little and found it made for a very confusing plot.
2-2980	2	141				Figure 2.14: Add the uncertainty estimates to the Figure caption [Birgit Hassler, USA]	Taken into account. See response to 2-2979
2-2981	2	141				Figure 2.14. As above [Elizabeth Kent, England]	Taken into account. See response to 2-2973
2-2982	2	141				Figure 2.14 (caption). The caption may be written as " Linear temperature trend …. [Yutaka Kondo, Japan]	Taken into account. See response to 2-2973
2-2983	2	141				The caption incorrectly claims that the figure shows results from "all available data products" The RATPAC data are not shown and should be. [Dian Seidel, USA]	Taken into account. See response to 2-2976
2-2984	2	142	4			Delete "period" from the caption. [Dian Seidel, USA]	Accepted
2-2985	2	142				I don't see the point of having 2 different figures. If you keep both figures you could also make a comment in the legend that comparing 2.14 and 2.15 shows the accelerating of the trends with warning that such small apparent difference is showing a change with a low/medium degree of confidence? [Francois DANIS, France]	Noted. The two figures are necessary as the satellites only come in half way through the longer radiosonde period.
2-2986	2	142				Figure 2.15: Add uncertainties on trend estimates to the figure or at least mention them in figure caption, since they are not mentioned in the text. [Birgit Hassler, USA]	Taken into account. See response to 2-2979
2-2987	2	142				This is a nice figure that could be made yet more informative. First, why use black and white arrows? The direction already shows whether increases or decrease are expected. Second, consider adding a second set of arrows indicating what observations show for trends for a particular period (chosen to maximize period length and number of variables with observations over that period). Third, add "Air Temperature (Stratosphere)". Finally, remove "indeed" from the caption. [Dian Seidel, USA]	Noted. We have played around with this figure in the past and many of these suggestions were explored and rejected. The stratosphere was in an early version and has been removed as the causal mechanism and link to a warming world is not at all obvious to a lay reader. We have made efforts to make this more accesible.
2-2988	2	143	1	143	7	A similar diagram is widely used to communicate climate science, but with stratospheric cooling added. This is a key part of the surface climate warming story, so should be added. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected. The stratospheric cooling includes a causal mechanism that is not sufficiently intuitive for a lay reader for this context.
2-2989	2	143	4	143	6	Effective demonstration of how the IPCC climate model is completely at odds with what really happens in the climate [VINCENT GRAY, NEW ZEALAND]	Rejected. Comment is a non-sequitor. Nowhere had we suggested anything about a model so we are unsure why the reviewer is choosing to make this comment or what changes they expect as a result. No changes made.
2-2990	2	143		143		Fig. FAQ 2.1: Instead of indicating rising humidity, I would suggest indicating rising specific humidity. [Alice Grimm, Brazil]	Noted. This arrow label has been changed

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2-2991	2	143		143		FAQ 2.1 Fig1, Panel for "Air temperature near surface" seems rather high in the atmosphere! [Gareth S Jones, UK]	Taken into account in figure revisions
2-2992	2	143				FAQ2.1, Figure 1: I suggest changing "air temperature near surface" to "air temperature in the lowest few kilometres of the atmosphere (troposphere)", to minimise potential confusion with "air temperature over land". [David Wratt, New Zealand]	Accepted
2-2993	2	144	4	144	7	Unconvincing display of changes in climate, of decreasing accuracy, several of which may not be indicators of "warming". There are no temperature sensors on ice surfaces, the Arctic is prone to periodic change, and all indicators show little change for the last ten years [VINCENT GRAY, NEW ZEALAND]	Rejected. Nowhere in the figure caption is the implication made that we are measuring the temperature of ice surfaces for example. The rest of the comment is largely conjecture without support in the literature.
2-2994	2	144	6	144	7	Replace (Baringer et al., 2010) => Baringer et al. (2010) [Yutaka Kondo, Japan]	Editorial. There are many cases where endnote citations need fixing. See also response to 2-1253
2-2995	2	144				FAQ 2.1, Figure 2: In the caption it does not mention (but should) that three of the series have been truncated. Two of the marine temperature series (Ishii et al.) and one of the sea-surface temperature series (ICOADS) start later than the original published series. This is because there are known biases in those series before their plotted start dates that have not been adjusted for. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. These caveats have been placed in the expanded appendix to this figure.
2-2996	2	144				Figure FAQ2.1, 2. It states that "further information is given in Baringer et al (2010)". I was interested in which datasets had been used in this figure - however this isn't given in the referenced paper which told me to look at www.ncdc.noaa.gov/bams-state-of-the-climate/. This link is the top level for the state of the climate and doesn't give the information required, and also I couldn't find it under the link for the 2009 report (which should I think be Blunden et al. 2010 rather than Baringer et al). [Elizabeth Kent, England]	Noted. In response to this and other comments the details are now folded in to the chapter appendix.
2-2997	2	145		145		Fig2.16 How was smoothing of the data done? [Gareth S Jones, UK]	It is in Appendix 3.A from Trenberth et al 2007
2-2998	2	145		145		Figure 2.16 has used GPCC when there is no peer-reviewed paper. You omitted CRUTS as there wasn't one, should have omitted GPCC. This is a minor point. [Philip JONES, UK]	Noted, in SOD GPCC has a submitted paper.
2-2999	2	145		145		Although it is useful to see precip trends averaged over latitudinal bands, we know coastal areas and mountainous areas are very different. Somehow, in the text and figures, it would be useful to see these areas broken out, particularly when coastal areas are of concern already due to sea level rise. I found one sentence for N American on coastal temperatures [Beverly Law, USA]	More details are shown in new figure 2.29
2-3000	2	145		166		Explain variables (acronyms) shown in the figures [Beverly Law, USA]	Noted, they are explained in the text
2-3001	2	145				Figure 2.16: Is it possible to add a second y axis indicating precipitation anomaly as a percentage of the mean precipitation? [Seth Westra, Australia]	Its possible but the authors don't feel it would add anything.
2-3002	2	146	1	146	14	It looks like essentially nothing on these plots are significant. That should be stated in the caption. [Karen Rosenlof, United States of America]	True and this is mentioned in the text.
2-3003	2	146	1	168	14	Fig. 2.17: How different is the GPCC V5 trend map? Please comment on the differences between the datasets. Given the lack of data in the last few years in GHCN the trends should look very similar. [Geert Jan van Oldenborgh, Netherlands]	We have commented on the difference in the text.
2-3004	2	146	1			Figure 2.17: should note in the caption that the trends are not field significant. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Not sure this would add anything, but will consider this.
2-3005	2	146		146		Figure 2.17. The scale for no trend is +-5% in the top panel and +-3% in the lower panel [Lena M. Tallaksen, Norway]	Yes, that is true because of the different time periods shown in each map.
2-3006	2	147	1	147	12	What spatial area do the time series plotted come from? Are they global averages (cos weigthed)? Please	Yes they are global averages as stated in the text,

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						state clearly in the caption what they are. [Karen Rosenlof, United States of America]	
2-3007	2	147	4	147	10	Useful evidence of variability but not of any consistent "trend" [VINCENT GRAY, NEW ZEALAND]	Disagree, but this is conjecture
2-3008	2	147				Figure 2.18a: Are all the shown colored grid boxes significant, and the white ones not? Please explain in figure caption. [Birgit Hassler, USA]	The plot has changed and the significant grids indicated
2-3009	2	147				Figure 2.18b-e: Are these time series based on ERA reanalyses? If so then mention that in figure caption. [Birgit Hassler, USA]	The databases used are mentioned in the new figure 2.30
2-3010	2	147				I recognize this is not a finished plot, but consider the following. (1) Add "trend" to the label for a), (2) add labels for panels b)-e), including both the variable and the spatial domain, (3) in the caption, be clear that data for land or ocean regions are not really globally averaged. [Dian Seidel, USA]	The databases used are mentioned in the new figure 2.30
2-3011	2	148	1	148	10	State in the caption the data source for the top panel. [Karen Rosenlof, United States of America]	New figure 2.31
2-3012	2	148	5	148	8	Useful evidence of variability but not of any consistent "trend" [VINCENT GRAY, NEW ZEALAND]	New figure 2.31
2-3013	2	148				Figure 2.19 does not appear to say which SST data set has been used. [John Remedios, United Kingdom of Great Britain & Northern Ireland]	New figure 2.31, no SST included
2-3014	2	149	4	149	6	Unrepresentative sampling, mainly over the sea, raises questions whether this is really "global". The rate of increase appears to be falling [VINCENT GRAY, NEW ZEALAND]	Only measurements that are representative of large well-mixed volumes of the atmosphere are used to construct global averages. Measurements from sites affected by local emissions and sinks can not be properly weighted in calculation of global means.
2-3015	2	149	4		6	Figure caption, Figure 2.20, It is important to provide some additional information on how this global annual mean is derived, for example, the number of sampling locations, sampling frequency, and some citation. [Stephen Montzka, USA]	References are given in text and appendixl.
2-3016	2	149		151		I think that the bottom panels of Figs. 2.20, 2.21 and 2.22 need more work. First, I think that the origin of the ordinate should be clearly indicated in Figs 2.20 b and 2.22 b to make it clearer that these growth rates are consistently positive. Second, and more importantly, I think that there needs to be some indication of the uncertainties of the growth rates. I do not believe that all of the multiple peaks and valleys in the growth rate curves are physically reasonable, but rather are artifacts of differentiating the dashed curved line fit to the global averages. I think that we must ensure that no figure conveys any information that we are not confident is statistically significant and correct. [David Parrish, USA]	First, similar figures have been used in other assessments without confusion. Second, the features in the growth rate are not artifacts of the method to determine them. Where possible, curves from multiple measurement programs are now shown, and all programs see featuures of similar magnitude and phase, at least for the past 2 decades.
2-3017	2	149				Figure 2.20. Caption doesn't say where the data comes from. [Elizabeth Kent, England]	Updated figures identify sources of global means.
2-3018	2	149				Extend the vertical axis labels to encompass all the data plotted (330-395 ppm for top, 0-4 ppm/yr for bottom), for easier interpolation. Also, is "instantaneous" really an accurate description of the growth rate? Is the derivative computed from instantaneous measurements, or some temporally averaged data? [Dian Seidel, USA]	Figure updated to include 2011.
2-3019	2	149				Fig 2.20 - Fig 2.22: Coordinate to avoid any redundancy with Chapter 6. [Thomas Stocker/ WGI TSU, Switzerland]	We have shared our text and figures with Chapter 6.
2-3020	2	150	4	150	6	Very unrepresentative sampling so recent rise may be unreal [VINCENT GRAY, NEW ZEALAND]	Nonsense.
2-3021	2	150	4		6	Figure caption, Figure 2.21, It is important to provide some additional information on how this global annual mean is derived, for example, the number of sampling locations, sampling frequency, and some citation. [Stephen Montzka, USA]	References provided in text and appendix.
2-3022	2	150				Relabel the top vertical axis to extend from 1600 to 1850, and have tick marks at 1600, 1650, 1700, etc., which is more standard than what is currently shown. [Dian Seidel, USA]	Figure has been updated.

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2-3023	2	151	4		6	Figure caption, Figure 2.22, It is important to provide some additional information on how this global annual mean is derived, for example, the number of sampling locations, sampling frequency, and some citation. [Stephen Montzka, USA]	See references in text and appendix.
2-3024	2	152	5	152	10	Rephrase "monthly mean measurements" because we don't measure, but compute, means. Also change "different networks" to "two networks", which is more specific. [Dian Seidel, USA]	OK as is.
2-3025	2	153	4	153	11	Delete "trends in" as the plot shows time series, not trends. Add the date ranges for the two plots, since they are different. Clarify in the caption that "MBL" is "sea level", if that's the case, and that the "free troposphere" data are from aircraft, if that's the case. Also, why use different models (linear vs polynomial) to fit data from different locations? [Dian Seidel, USA]	Noted, changes in new figure 2.7
2-3026	2	153	4			Fig caption 2.24: Replace a) => (a) and b) => (b) [Yutaka Kondo, Japan]	New figure 2.7
2-3027	2	153				An updated version of Figure 2.24 wll be included in a "soon to be submitted" paper by Parrish et al. The updated version should be included in this chapter. [David Parrish, USA]	New figure 2.7 includes Parrish et al 2012
2-3028	2	154	1			Figure 2.25: caption says that panel (a) is for the "midlatitudes", yet the figure says 60S-60N which clearly includes the tropics too. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted, new figure 2.6
2-3029	2	154	4			Fig caption 2.25: The figures do not have (a) and (b) [Yutaka Kondo, Japan]	Noted, new figure 2.6
2-3030	2	154	5	154	5	The top plot is not "midlatitude" if the data span 60S-60N. Most of that region is tropical. Maybe "non-polar" is a better descriptor. [Dian Seidel, USA]	Noted, new figure 2.6
2-3031	2	154				Figure 2.25: This figure does not show midlatitude ozone trends but gobal ozone trends (60°S to 60°N), or at least the graph is labeled that way. [Birgit Hassler, USA]	Noted, new figure 2.6
2-3032	2	154				Figure 2.25 - In revision, I would be good to add 2010 data, especially as this was so anomalous in the Arctic. It would make this consistent in time with other figures, such as the NAO/AO showing anomalous behavior in 2010. [Drew Shindell, USA]	Noted, new figure 2.6 includes 2010
2-3033	2	155	4	155	4	Replace "155apour" with "vapour". [Robert Waterland, United States of America]	Noted, new figure 2.5
2-3034	2	155	4	155	6	Format of figure caption somehow screwed up. [Birgit Hassler, USA]	Noted, new figure 2.5
2-3035	2	155	4			Fig caption 2.26: what does “water 155apour” means? ppm should be replaced by “Stratospheric water vapour (ppm)” [Yutaka Kondo, Japan]	Noted, new figure 2.5
2-3036	2	155	4			"water 155apour" shold be "water vapour" [Jean Poitou, France]	Noted, new figure 2.5
2-3037	2	155				Figure 2.26: It would be useful to include NOAA FPH data (Boulder) in the top half of this plot (anomalies), since the Boulder FPH trends and their agreement/disagreement with satellite data are discussed at length in 2.4.2.3 [Dale Hurst, United States of America]	Noted, new figure 2.5
2-3038	2	155				Figure 2.26 shows the internal anomalies for HALOE and MLS data sets that differ in absolute terms by 0.5 ppmv, making their "agreement" here appear artificially good. This should be noted in the Figure caption. [Dale Hurst, United States of America]	Noted, new figure 2.5
2-3039	2	155				Figure 2.26: "155apour anomalies" should be fixed. The pressure level(s) of the water vapor retrievals used to create these time series needs to be specified. [Dale Hurst, United States of America]	Noted, new figure 2.5
2-3040	2	155				Figure 2.26: Are these anomalies based on global mean values or zonal averages for a specific latitudinal range? [Dale Hurst, United States of America]	Noted, new figure 2.5
2-3041	2	155				Figure 2.26. 155apour [Elizabeth Kent, England]	Noted, new figure 2.5
2-3042	2	156				Why show separate curves for USA and Central East Coast USA? They don't look much different. Please add start years for each set of measurements. [Dian Seidel, USA]	Start years are added in the figure 2.8

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2-3043	2	157	1	157	9	Individual plots are too small to tell anything. Just pick one year and show it. [Karen Rosenlof, United States of America]	Figure deleted
2-3044	2	157	1			Figure 2.28: not realy big enough to see anything. Why is the central column bigger than the others? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Figure deleted
2-3045	2	157	2	157	2	the date axis is missing [JOHN OGREN, USA]	Figure deleted
2-3046	2	157		157		Fig2.28 What do the big Earths represent? And what does the Earths across the x-axis represent? Poorly described figure. [Gareth S Jones, UK]	Figure deleted
2-3047	2	157				Figure 2.28: Figure would be clearer if an axis with respective years for each of the globe plots would be added, however, detailed differences between the globe plots are hard to see in any way. Maybe think of another format of this figure? [Birgit Hassler, USA]	Figure deleted
2-3048	2	157				Figure 2.28: Add title to color bar. [Birgit Hassler, USA]	Figure deleted
2-3049	2	157				I don't really see the value of including this figure. Each little bubble of the Earth is too small to send a message. If I understand the layout correctly, the first column shows data for 2000 and the last shows data for 2009. Why are 2004 data highlighted with bigger bubbles? If these data are included, I'd suggest re-conceiving a presentation that communicates some scientific result more clearly. [Dian Seidel, USA]	Figure deleted
2-3050	2	157				Figure 2.28: I propose to add the definition of AOD, either in the figure caption or in the text. [Uwe Stoeber, Germany]	Figure deleted
2-3051	2	157				Pretty graphics but really difficult to see anything other than vague blobs, maybe a single figure for each season would be enough. I don't see what this one accomplishes [Larry Thomason, United States of America]	Figure deleted
2-3052	2	157				Fig 2.28: The purpose of showing a sequence is not obvious, more details should be provided to make this figure informative. [Thomas Stocker/ WGI TSU, Switzerland]	Figure deleted
2-3053	2	158	1	158	2	There is a typo: water 155apour [Claudio Cassardo, Italy]	Noted new figure 2.5
2-3054	2	158	1	158	9	Figure is blurry. [Karen Rosenlof, United States of America]	Corrected, new figure 2.9
2-3055	2	158	1			Figure 2.29: says that they are ten year trends -- give the time range in start/end year. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Corrected, new figure 2.9
2-3056	2	158				Figure 2.29: The quality of graphics in this figure is very poor [Dale Hurst, United States of America]	Corrected, new figure 2.9
2-3057	2	158				Figure 2.29: The quality of graphics in this figure is very poor [Dale Hurst, United States of America]	Corrected, new figure 2.9
2-3058	2	158				Figure 2.29. Terrible quality [Elizabeth Kent, England]	Corrected, new figure 2.9
2-3059	2	158				Figure 2.29: quality of the Figure should be improved [Yutaka Kondo, Japan]	Corrected, new figure 2.9
2-3060	2	158				I'm not sure what to make of 10-yr trends in AOD or any variable. I'm also surprised by the location of the red boxes showing regions of statistical significance, which one generally expects to be associated with large trends, not the near-zero trends in the South Pacific region. As with the preceding figure, please consider if this one is really needed. [Dian Seidel, USA]	Corrected, new figure 2.9
2-3061	2	159	1	162	1	Figures 2.30-2.33. Are these four figures really all needed? Doesn't seem that very much is communicated by 4 figures. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted, figures replaced by figure 2.11
2-3062	2	159				If there are PM2.5 data for other parts of the world, could they be added to this plot. Also, consider re-ordering the key so that the stations appear in the same order as their respective time series in the plot (Ispra first), and make sure the various purple lines are distinguishable. [Dian Seidel, USA]	Noted, figure replaced by figure 2.11

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3063	2	160	4	160	4	Give the start date of observations. [Dian Seidel, USA]	Noted, figure replaced by figure 2.11
2-3064	2	160		160		Fig2.31 Is there a need for this figure when it includes only 17 data points? [Gareth S Jones, UK]	Noted, figure replaced by figure 2.11
2-3065	2	160				Figure 2.31: Need to include the time period over which the indicated reductions occurred [Dale Hurst, United States of America]	Noted, figure replaced by figure 2.11
2-3066	2	160				Figure 2.31: Need to include the time period over which the indicated reductions occurred [Dale Hurst, United States of America]	Noted, figure replaced by figure 2.11
2-3067	2	160				Figure 2.31: (caption), reference time should be added. [Yutaka Kondo, Japan]	Noted, figure replaced by figure 2.11
2-3068	2	160				Fig 2.31 and 2.32: The WGI approach is to avoid political boundaries/names from maps. We would prefer if results from individual research projects are not given special prominence. Such projects can be mentioned in the context of an assessment, but simply stating results from individual research projects should be avoided. Can the results be broadened beyond the US and Europe? [Thomas Stocker/ WGI TSU, Switzerland]	Noted, figure replaced by figure 2.11
2-3069	2	161	1	161	6	I can't read this graph at all. Needs to be either bigger, or redrafted. [Karen Rosenlof, United States of America]	Noted, figure replaced by figure 2.11
2-3070	2	161	4			Fig caption 2.32: The figure caption needs clarification [Yutaka Kondo, Japan]	Noted, figure replaced by figure 2.11
2-3071	2	161		161		Fig2.32 Is this figure needed? Very few datapoints and very hard to see what is going on. [Gareth S Jones, UK]	Noted, figure replaced by figure 2.11
2-3072	2	161				Figure 2.32: Can this be reformatted to look like fig 2.31 (which is very clear) [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted, figure replaced by figure 2.11
2-3073	2	161				Figure 2.32: Please explain in the figure caption what the difference between triangles pointing upwards and downwards is (increase and decrease, respectively?) [Birgit Hassler, USA]	Noted, figure replaced by figure 2.11
2-3074	2	161				Figure 2.32: The quality of graphics in this figure is very poor [Dale Hurst, United States of America]	Noted, figure replaced by figure 2.11
2-3075	2	161				Figure 2.32: The quality of graphics in this figure is very poor [Dale Hurst, United States of America]	Noted, figure replaced by figure 2.11
2-3076	2	161				Figure 2.32: (caption), reference time should be added. [Yutaka Kondo, Japan]	Noted, figure replaced by figure 2.11
2-3077	2	161				This map presentation does not communicate very well. Removing the state boundaries might help, as would enlarging the symbols. But consider other graphical formats that might work better. Also, re-write the caption and title above the figure so they don't stress IMPROVE so much - not many readers will be familiar with that network/program. [Dian Seidel, USA]	Noted, figure replaced by figure 2.11
2-3078	2	161				Figure 2.32 - Please add to the caption the definition that a triangle pointing up is an increase and down is decrease (at least, I assume that's what's meant). [Drew Shindell, USA]	Noted, figure replaced by figure 2.11
2-3079	2	161				all the text in this figure (station names?) kind of take away from any visual impact of this figure at least at the size it is in the document. Could those be removed? [Larry Thomason, United States of America]	Noted, figure replaced by figure 2.11
2-3080	2	162	5	162	6	perhaps "open red circle" to distinguish from the filled red circles that are presumably significant trends? [Marcus Sarofim, USA]	Noted, figure replaced by figure 2.11
2-3081	2	162		162		Fig2.33 Is this figure needed? The data over America is extemely uniform and over Europe is a bit of a mess. Not clear what is being shown. [Gareth S Jones, UK]	Noted, figure replaced by figure 2.11
2-3082	2	162				Figure 2.33: Can these panels be reformatted to look like fig 2.31 (which is very clear) [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted, figure replaced by figure 2.11
2-3083	2	162				Figure 2.33: According to the text (p.57, line 21-22) there is no significant trend in the western US, however, the circles there are not red. [Birgit Hassler, USA]	Noted, figure replaced by figure 2.11

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3084	2	162				Figure 2.33: In the caption it says that not significant trends are denoted with a red circle. It is a bit confusing that with that statement only the LINE of the circle is described. Maybe rephrase? [Birgit Hassler, USA]	Noted, figure replaced by figure 2.11
2-3085	2	162				Figure 2.33 (caption) The unit of sulfate aerosols may be written as μg(S) m-3 yr-1 [Yutaka Kondo, Japan]	Noted, figure replaced by figure 2.11
2-3086	2	163	1			Figure 2.34: seems odd to give 1 decimal place precision for the residual, while everything else is integer only. Schematic makes it look like all the upward thermal radiation is absorbed by *clouds* (except for that which isn't absorbed and goes through the window). But what about absorption by GHGs? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	The residual is given in one decimal precision as the discussion of this term is taken place on this level of accuracy, based on ocean heat uptake observations. For example, Loeb et al. 2012 (Nature Geoscience) combine satellite data with ocean measurements to depths of 1,800 m, and show that between January 2001 and December 2010, Earth has been steadily accumulating energy at a rate of 0.50±0.43 Wm−2 (uncertainties at the 90% confidence level). The alternative, to round this to the next integer value, would result in the value of 1, which would be outside the uncertainty range of the observational estimates of this term.
2-3087	2	163	4	163	6	Reveals the absurdity of the IPCC model. The earth is not irrevocably flat and all the parameters go purely up and down despite the pretence that they are slanting. The earth does not rotate, there is no difference between day and night and the supposed uncertainties of the parameters admit that they are mere averages which can only be obtained by the use of non linear equations and skewed distribution curves. The idea that they can be a "balance" is ridiculous. No part of the earth is ever in equilibrium so any use of thermodymic equations such as the Stefan.Boltzmann Law is forbidden The system simply does not work and is a poor substitute for the ordinary weather forecasts, despite their limitations. The absurdities of this diagram include the fact that if you assume there is no atmosphere, incoming solar energy would be 340W/sqm and the radiation loss would be 396 W/sqm which would surely lead to absulute zero in bright sunshine. [VINCENT GRAY, NEW ZEALAND]	Figure 2.34 presents best estimates of globally and annually averaged energy flows in the climate system at the top of atmosphere, within the atmosphere and at the surface. In the averaging process, the daily cycle as well as the integration over the varying solar zenith angles has been considered. It does not assume that the Earth non-rotating. If the net shortwave fluxes absorbed by Earth is not equal to the emitted longwave radiaton by the planet averaged over the globe and over time, such an imbalance would alter the heat content of the planet and its thermal conditions.
2-3088	2	163				pretty fancy, is all this graphical power necessary? [Larry Thomason, United States of America]	Noted
2-3089	2	164	1	164	1	I found Figure 2.35 confusing as it was not clear what was TOA flux and ocean heating rate. [Roger Saunders, United Kingdom]	The text has been modified
2-3090	2	164	1	164	11	State explicitely in caption what is the upper ocean (I assume you mean the PMEL lines). [Karen Rosenlof, United States of America]	The text has been modified
2-3091	2	164				Figure 2.35: This figure is hardly discussed in the text. To me, the message of this figure remains totally unclear. [Uwe Stoeber, Germany]	The text has been modified
2-3092	2	165	4	165	7	This surely shows that any "warming"since 1980 is caused by extra solar radiation, possibly enhanced by feedback resulting from cosmic rays [VINCENT GRAY, NEW ZEALAND]	As stated for example in Wild et al. (2007) GRL, recent solar brightening cannot supersede the greenhouse effect as main cause of global warming, since land temperatures increased by 0.8°C from 1960 to 2000, even though surface solar radiation overall did not reach higher levels in 2000 than in the 1960s (in fact, globally rather lower levels).Therefore overall surface solar radiation changes from 1960 to 2000 cannot explain the overall warming over this period, as overall, surface solar radiation changes would have introduced rather a cooling than a warming over this period.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3093	2	165		165		Fig2.36 Why is the surface solar radiation at a site near Berlin interesting? No need to show this. [Gareth S Jones, UK]	Fig. 2.36 illustrates the decadal variation of surface solar radiation discussed in the main text, exemplified by one of the longest observational records, the Potsdam series. It clearly illustrates phases of dimming (1950s to 1980s), brightening (since 1980s), and early brightening (prior to 1950s) typical for many observational records.
2-3094	2	165				Fig 2.36. Dimming and brightening is a very important topic. Is it therefore possible to considerably expand this figure to provide many more results, so that a more global overview is given rather than just observations from one station (Potsdam). [Thomas Stocker/ WGI TSU, Switzerland]	We chose the famous Potsdam series as an illustrative example which clearly illustrates phases of dimming (1950s to 1980s), brightening (since 1980s), and early brightening. We thought that overplotting many series into one figure may rather degrade the illustrative character of the figure, and still would remain arbitray or incomplete in the choice of the records. We reserve the possibility to revise the figure for the final draft if a graphically satisfactory representation of such an expanded figure can be achieved. Alternatively, a more conceptual figure could be provided which summarizes the observational studies (e.g. Wild BAMS 2012 Figures 1 or 2), but as this is the observations chapters, we thought that showing direct observations is more appropriate.
2-3095	2	166	1	166	10	Is there a way to assess whether the changes in patterns shown here are statistically significant? [Karen Rosenlof, United States of America]	Noted - Statistical significance is not tested
2-3096	2	166		166		Fig. 2.37: This figure seems a somewhat confusing to me. [Alice Grimm, Brazil]	Accepted - Figure is changed
2-3097	2	166		166		Fig2.37 the colours of the lines for the 1004hPa and 1020.5hPa are pretty much indistinguishable. [Gareth S Jones, UK]	Accepted - Colours are changed
2-3098	2	166				I found that figure very difficult to follow: I can't see the trends: too many lines, can't match the colors of the legend to the colors on the graph (different color background). I don't know how difficult it is to make an average of the different datasets but instead of showing all the datasets, I would have only 3 lines for low pressure, 3 for high pressures, 3 for 100hPa, the lines corresponding to the 3 time periods. [Francois DANIS, France]	Accepted - Only one data set is shown
2-3099	2	166				Figure 2.37: In figure caption the left panel is described as averages for November to March whereas the description in the figure itself states November to April [Birgit Hassler, USA]	Accepted - Changed
2-3100	2	166				Figure 2.37: Colors for the different time periods are not well distinguishable in the graph. Maybe adjust colors? Also, maybe mention the assigned color for each of the time periods in the figure caption. [Birgit Hassler, USA]	Accepted - Colours are changed
2-3101	2	166				Figure 2.37: The lines are difficult to distinguish, especially in the upper panel. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - Colours are changed
2-3102	2	166				Consider deleting this figure. See comments on related text. [Dian Seidel, USA]	Noted - 100 hPa is omitted.
2-3103	2	166				Figure 2.37: This figure is hard to interpret due to a bad color choice, too thin lines, and poor image quality. [Uwe Stoeber, Germany]	Accepted - Colours are changed
2-3104	2	167	1	167	8	State what the units are. [Karen Rosenlof, United States of America]	Noted - Figure is changed
2-3105	2	167	1			Figure 2.38: why not reduce the time period a little and also show trends from ERA-interim (1989-present)?	Accepted - line styles are changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	
2-3106	2	167	4	167	4	Explain what Blended Sea Winds are in the caption or in the main text. [Robert Waterland, United States of America]	Noted - This reanalysis is very frequently used in the literature and hence should be shown.
2-3107	2	167		167		Fig2.38 The use of "RA20C" with panel c is inconsistent with the use of "20CR" in the main text. [Gareth S Jones, UK]	Accepted - Problem of resolution, figure is redrawn
2-3108	2	167				Figure 2.38: Add title to color bar. [Birgit Hassler, USA]	Noted - Aspect ratio is increased and scales are slightly adjusted
2-3109	2	167				Figure 2.38. Units? [Elizabeth Kent, England]	Accepted. m/s
2-3110	2	167				Fig 2.38: Units? Also not clear what 'Blended sea winds" are. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: m/s, a brief description of BSW is added to the main text (the figure is changed too).
2-3111	2	168	1			Figure 2.39: dotted lines at y=0 would help visualise trends and anomalous periods. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Noted - Figure is changed
2-3112	2	168		168		Figure 2.39: Change some of the line-styles and symbols to make them more distinguishable. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - line styles are changed
2-3113	2	168				In the bottom panel, the range of mean values for 2001/2 strength of the Hadley Circulation, shown by the dots at right, exceeds the interannual variability in almost all the time series. One outlier has a value of +3, while all the other estimates have negative values. Should that outlier be excluded from the analysis? [Dian Seidel, USA]	Noted - This reanalysis is very frequently used in the literature and hence should be shown.
2-3114	2	169	8			the "heavy solid line" doesn't seem to have been drawn yet except for the Jet stream. [Francois DANIS, France]	Accepted - Problem of resolution, figure is redrawn
2-3115	2	169				Figure 2.40: The vertical axes ranges are too large to see any trends. [Uwe Stoeber, Germany]	Noted - Aspect ratio is increased and scales are slightly adjusted
2-3116	2	170		170		Box2.4, Fig1. "20C RA" is not used anywhere else. [Gareth S Jones, UK]	Editorial
2-3117	2	170				It's surprising to see indices based on upper-air features from the 20th century reanalysis, which assimilates only surface observations. [Dian Seidel, USA]	Noted
2-3118	2	171	0	171	0	Box 2.4, Figure 2: Use of PDO here seems sub-optimal. The current plot is unnecessarily North Pacific-centric. The PDO can be regarded as the North Pacific expression of the near-global Interdecadal Pacific Oscillation (Power et al. 2006). The PDO convolutes El Niño-Southern Oscillation and Aleutian Low variability (Newman et al. JClim). It is not clear why Aleutian Low variability should be relevant for variability in e.g. the South Pacific or Indian Oceans. This makes the features in the plot beyond the North Pacific seem washed out. This gives false impression to some readers that ENSO-like decadal variability beyond the North Pacific is not important. This is incorrect (see refs below for example). This is not the case if the IPO is used because the IPO is based on an analysis of near-global SST. I therefore recommend that the PDO panel is redone using the IPO index. This will show features away from the North Pacific more clearly. Reference: Power, S.B., M. Haylock, R. Colman, X. Wang, 2006: The predictability of interdecadal changes in enso activity and enso teleconnections. J. Climate, 19, 4755–4771. See also: Folland, C. K., D. E. Parker, A. W. Colman, and R. Washington, 1999: Large scale modes of ocean surface temperature since the late nineteenth century. Beyond El Niño: Decadal and Interdecadal Climate Variability, A. Navarra, Ed., Springer-Verlag, 73–102. Folland, C. K., J. A. Renwick, M. J. Salinger, and A. B. Mullan, 2002: Relative influences of the Interdecadal Pacific Oscillation and ENSO on the South Pacific convergence zone. Geophys. Res. Lett., 29.1643, doi:10.1029/2001GL014201.	Rejected. We consider IPO and PDO to be different indices of essentially the same thing. We are not aware of qualittive distinction between these two phenomena demonstrated in the peer-reviewed literature. See also 2-892.

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						Power, S. B., T. Casey, C. Folland, A. Colman, and V. Mehta, 1999a: Interdecadal modulation of the impact of ENSO on Australia. Climate Dyn., 15, 319–324. [Scott Power, Australia]	
2-3119	2	171	1			Box 2.4, Figure 2: why not make this figure 1 and figure 1 could be second. Makes more sense to introduce the modes and then to show their time variation. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	Rejected. Technically, we define indices, and then find mode patterns by regressing impacted climate fields on the indices. See Ch 14 Box 14.1 for formal definitions.
2-3120	2	171		171		"DMI" is called "IOD" in Chapters 9 and 14, this needs to be reconciled. Refer ahead to Chapter 14 for definition. [George Kiladis, USA]	Accepted: Made clear that DMI is one particular index of the Indian Ocean Dipole (IOD)
2-3121	2	171		171		It is difficult to see the PSA 1 pattern with the color scheme used. Chapter 14 also discusses the PSA, cross reference [George Kiladis, USA]	Noted. Difficulty in reading PSA1 is due to the goal to have a common MSLP colorbar
2-3122	2	171				The title of the third row of maps says "Annular Modes and NAO: NAO and SAM" which doesn't make sense. [Dian Seidel, USA]	Accepted: the subtitle was removed
2-3123	2	171				It is not clear to me what is meant by "NAO and PNA are regrossion coefficients". "MSLP change, hPa per index s.d." is clearer. [Andrew Stepek, Netherlands]	Editorial, reworded.
2-3124	2	172				Could the HadEX results be extended past 2003? If not consider removing them. [Dian Seidel, USA]	ACCEPTED: HadEX does not extend beyond 2003 so the results from this dataset have been removed.
2-3125	2	173	1	173	1	The figures in the right column are too small. [Robert Waterland, United States of America]	ACCEPTED: Figure has been revised accordingly.
2-3126	2	173	2	173	9	There is a need to explain how the HadGHCND time series data taken from Caesar et al. (2006) extends to 2009 in Figure 2.41 since the data series in Caesar et al. (2006) only goes up to 2003. Moreover, clarification is needed on how the Duke data taken from Morak et al. (2011) also include cool days, warm days, cool nights since the Morak et al. (2011) study, from a reading of the abstract of the paper, appears to have only considered warm nights (TN90) up to the year 2003. [Reynold Stone, Trinidad and Tobago]	ACCEPTED: HadGHCND is an operational dataset, updated monthly which is why the data are able to be extended beyond the year outlined by the Caesar et al. 2006 publication. Morak et al. 2011 is the incorrect reference, it should be Morak et al. 2012.
2-3127	2	173	2	173	9	A visual inspection of the global annual time series of anomalies cool nights, Figure 2.41 (a), reveals that cool nights initially appear to fluctuate randomly and abruptly decrease around the mid-1970s. This suggests the presence of a change point. The application of the Pettitt test to the HadEX data series (Alexander et al., 2006) yielded a change point in 1976. The turning point and difference sign tests failed to reject the null hypothesis, P = 0.74 and P = 0.16 respectively, implying the absence of trends in the data series. The presence of a change point in the data series suggests that the use of linear regression is inappropriate for trend detection. [Reynold Stone, Trinidad and Tobago]	REJECTED: The use of the Pettit test in isolation (as outlined in the Vincent et al. 2012 response to the Stone 2011 paper) is of itself flawed when considering climate timeseries which vary on various temporal scales. Without use of a reference series the alternative hypothesis could indicate that the timeseries has a significant trend which may not necessarily reflect a change in the mean. In any case this figure has been updated with more recent datasets and HadEX no longer appears in the figure.
2-3128	2	173	2	173	9	In addition, the presence of a step change suggests that the assumption of linearity is violated. The linearity assumption is implicit in the use of linear regression and implies that the rate of change of cold nights is constant over time. This could be easily checked using a simple numerical procedure called the half-slope method (Wilcox, 2005). In this method, the time series is divided into two periods of equal lengths, 1951-1976, and 1978-2003, and the least squares slope computed for each period. The ratio of the slope for the later period to the slope for the earlier period is called the half-slope. A linear model is only considered appropriate if the half-slope ratio is reasonably close to 1. The slopes for the earlier and later periods were found to be -0.10 and -0.46 respectively. The half-slope ratio is therefore 4.6 indicating the violation of the linearity assumption since 4.6 cannot be considered reasonably close to 1. It is also important to note that the 95% confidence interval for the slope during the period 1951-1976 was found to be (-0.33, 0.13) and thus not statistically significant i.e. not significantly different from zero. This implies that cold nights varied randomly during the period and contradicts the claim that most global land areas have experienced significant decreases in cold nights since 1950 stated on page 76, lines 49-51. Wilcox, R.R. 2005. Introduction to Robust Estimation and Hypothesis Testing. Academic Press, Boston. [Reynold Stone, Trinidad and Tobago]	REJECTED: See above comment.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3129	2	173	2	173	9	A visual inspection of the global annual time series of anomalies warm nights, Figure 2.41 (c), reveals that warm nights initially appear to fluctuate randomly and abruptly decrease around the middle to late 1970s. This suggests the presence of a change point. The application of the Pettitt test to the HadEX data series (Alexander et al., 2006) yielded a change point in 1978. The turning point and difference sign tests failed to reject the null hypothesis, P = 1 in both cases, implying the absence of trends in the data series. The presence of a change point in the data series suggests that the use of linear regression is inappropriate for trend detection. [Reynold Stone, Trinidad and Tobago]	REJECTED: The use of the Pettit test in isolation (as outlined in the Vincent et al. 2012 response to the Stone 2011 paper) is of itself flawed when considering climate timeseries which vary on various temporal scales. Without use of a reference series the alternative hypothesis could indicate that the timeseries has a significant trend which may not necessarily reflect a change in the mean. In any case this figure has been updated with more recent datasets and HadEX no longer appears in the figure.
2-3130	2	173	2	173	9	The presence of a change point in the data series again implies that the use of linear regression is inappropriate for trend detection. The slope for the periods 1951-1976 and 1978-2003 were found to be -0.14 and 0.98 respectively yielding a half-slope ratio of -7. The negative half-slope value indicates a change in direction of slope for the two periods and is also far from 1, clearly demonstrating the violation of the linearity assumption and the inappropriateness of linear regression for trend detection. It is also interesting to note that the negative slope for the period 1951-1976 indicates a decreasing trend in warm nights during this period which clearly contradicts the claim that most global land areas have experienced significant increases in warm nights since 1950 (Page 76, lines 49-51). However, the 95% confidence interval was found to be (-0.32, 0.03) indicating that the trend is not significantly different from zero. This implies that warm nights varied randomly during the period. [Reynold Stone, Trinidad and Tobago]	REJECTED: See above comment.
2-3131	2	173	2	173	9	In summary, our analyses above demonstrate that the statistical analyses performed by the authors were flawed and therefore invalidate the conclusions that globally there was a statistically significant decreasing linear trend in cold nights and a statistically significant increasing linear trend in warm nights during the period 1951-2003 (or since 1950). Moreover, our analyses suggest that the putative statistically significant linear trends reported in the AR4 were not real but merely artifacts of at least one step change (middle to late 1970s) in both cool and warm nights likely caused by the well-known 1976/77 Pacific climate shift (Miller et al., 1994) coupled with the inappropriate use of linear regression for trend detection. This finding is consistent with that of Stone (2011) for the southern South American region. Furthermore, the abrupt changes detected in the two time series appear to be inconsistent with the gradual monotonic changes projected by general circulation models under the various human-induced greenhouse warming scenarios. Miller, A.J., D.R. Cayan, T.P. Barnett, N.E. Graham, and J.M. Oberhuber.1994. The 1976-77 climate shift of the Pacific Ocean. Oceanography, 7, 21-26. Stone, R.J. 1993. Comments on “Observed Trends in Indices of Daily Temperature Extremes in South America 1960-2000”. J. Climate, 24, 2880-2883. [Reynold Stone, Trinidad and Tobago]	REJECTED: In addition to the above responses, the assessments made in AR5 are based on many more datasets than HadEX. Datasets are now available which corroborate these findings and these are highlighted in updated figures. In addition, this chapter makes no assumptions about "the gradual monotonic changes projected by general circulation models under the various human-induced greenhouse warming scenarios" as this is outside of the remit of Ch 2. Nor is it within the remit of Ch 2 to make attribution statements about the causes of any observed changes. The 1993 paper you reference we believe should be 2011.
2-3132	2	173	3	173	4	It is always tricky to try to articulate percentage trends in percentile values, and this caption has not quite succeeded. Consider explaining how the 10th percentile values are computed and whether the reported change is in the 10th percentile value itself or in exceedances. [Dian Seidel, USA]	ACCEPTED: We have revised these trends so that the units are in days.
2-3133	2	173		173		In this figure maps, labels of color bars and plots are almost unreadable. Please enlarge. [Valentina Pavan, Italy]	ACCEPTED: This figure has been updated accordingly.
2-3134	2	173				Figures and labelling are WAY TOO SMALL!!!! [Larry Thomason, United States of America]	ACCEPTED: This figure has been updated accordingly.
2-3135	2	174	1	174	11	Bottom panel is not deciferable. Chsnging colors may help. [Karen Rosenlof, United States of America]	ACCEPTED: This figure has been updated accordingly.
2-3136	2	175				Figure 2.43: A more instructive picture would show a running rate of landfalls, or even better the difference between the average rate over the entire observed period and a running rate (parhaps using a decade-long window). See also Solow&Moore, J Clim (2000) and Katz, J Appl Met 2002. [Peter Guttorp, USA]	NOTED: A new figure has replaced this one.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3137	2	175				Given the discussion in the text, consider removing the trend lines from these plots [Dian Seidel, USA]	NOTED: A new figure has replaced this one.
2-3138	2	175				Figure is stretched out of proportion (should be wider) and could be larger to improve readability. [Larry Thomason, United States of America]	NOTED: A new figure has replaced this one.
2-3139	2	176	1	176	11	This is a complicated graph that has too many small boxes. Can the trend results be summarized in a table instead? [Karen Rosenlof, United States of America]	TAKEN INTO ACCOUNT: This figure has been amended accordingly.
2-3140	2	176	1			Figure 2.44: cannot read axis labels. Why not remove them all, and then have a single (empty) labelled panel in the space between Sicily and Corsica with bigger (legible) labels that applies to all others? [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This figure has been amended accordingly.
2-3141	2	176				On a paper printout, it's hard to distinguish the shades of red and blue. Moreover, I question the value of plotting trends significant at the 20% confidence level. Consider only plotting those trends significant at the 5% level. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: This figure has been amended accordingly.
2-3142	2	176				Individual figures way too small to read and the figure is awfully complicated. Can it be simplified and made easier to read? [Larry Thomason, United States of America]	TAKEN INTO ACCOUNT: This figure has been amended accordingly.
2-3143	2	177	5			“heatwave duration measure”. A lay reader may understand that the figure a) is showing the duration of each heatwave; where I believe you show how many days in a year are considered as heatwaves. [Francois DANIS, France]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3144	2	177	6	177	6	I can’t tell where there is any stippling on the top panel. [Karen Rosenlof, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3145	2	177	6			As it is a FAQ, a easy to understand comment like: a positive trend means that observed heat waves last longer on average? [Francois DANIS, France]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3146	2	177	7	177	11	I can't tell where the grey bars mentioned are, I just zsee lots of different colors with unreadable numbers printed next to them. [Karen Rosenlof, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3147	2	177				FAQ 2.2, Figure 1: Stippling in part (a) of the figure is hard to see, especially since latitude and longitude dividers are represented by dashed lines. [Birgit Hassler, USA]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3148	2	177				FAQ 2.2, Figure 1: What does the color of the colored lines (blueish and yellowish) represent? [Birgit Hassler, USA]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3149	2	177				The title at the top of the top panel doesn't communicate much - who knows what HadGHCND WSDI is? The color scale should be changed, because the largest positive and negative values are too similar to distinguish. For the bottom panel, the legend should explain the meaning of the blue and yellow lines. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3150	2	178	1			FAQ 2.2, Figure 2: make better use of space by choosing y-axis ranges so that data fill the graphs. [Tim Osborn, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3151	2	178	30	178	33	I suggest to make a comparison between the observed CO2 abundance / growth rate and the previous projections based on difference scenarios. The results of the comparison can be included here to show whether or not the emission control was effective. [Xiaobin Xu, China]	REJECTED: Outside the scope of this section.
2-3152	2	178				FAQ 2.2 Figure 2: A more instructive picture would show a running rate of landfalls, or even better the difference between the average rate over the entire observed period and a running rate (parhaps using a decade-long window). See also Solow&Moore, J Clim (2000) and Katz, J Appl Met 2002. [Peter Guttorp, USA]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-3153	2	178				Please check Figure 2(c). The trend shown in this diagram does not agree with the latest tropical cyclone (including tropical depression) landfalling trend (slight decreasing, but not significant) obtained from the CMA dataset from 1949 to 2010. [Tsz-cheung Lee, Hong Kong]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.
2-3154	2	178				What are the confidence intervals for these trends? If the trends are not significant, don't plot the trend lines. [Dian Seidel, USA]	TAKEN INTO ACCOUNT: This FAQ has been substantially rewritten and this figure has been removed.