Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1	2	0	0	0	0	First of all I want to convey my sincere admiration to the authors for attempting such a difficult enterprise as is reviewing all the scientific literature on atmospheric and surface observations. This task is made even more daunting (both from a scientific and writing viewpoint) by the mandate to build on the earlier AR4 report and the need to interpret differences therefrom. Not only does the text become inevitably repetitive, but the authors have had to constantly assess whether the most recent studies support earlier conclusions and what constitutes sufficient evidence to revise or question earlier statements. While I am fully aware of these major difficulties, I still think that the current draft feels rough and of uneven quality, with a lack of consistency (in background scientific information provided, length, level of regional detail, treatment of statistical significance, presence of error bars in graphics, summary statements) from section to section. The precipitation, surface humidity and cloud sections, in particular, seem incomplete. Some lack of uniformity is due to the fact that research has not progressed at the same rate in all observational fields; a a result, while some sections report significant improvements in our understanding, others simply list a myriad of varied regional results that cannot be interpreted in any particular way (is it necessary then to list them?). There are also several places where insufficient effort to assessing (as opposed to listing) results has been paid. From an editing perspective, I must also recommend that the authors undertake a thorough revision of the entire document looking to improve its readability and usefulness to a graduate student, post-doc or scientist not expert in the particular field itself. Currently the document is rather difficult to parse even for an interested reviewer. My take may be flawed but I am thinking of this document as something one could refer a graduate student to in order for him/her to gain an overview on a particular topic. There is a surprising absence of connecting words within paragraphs whose presence would allow the reader to follow the flow of ideas and gain a clearer notion of what the ovrall results mean (by the time the summary sentence arrives, the reader is sometimes at loss). In fact I wonder if there was a deliberate effort to postpone the “narrative touches” until the final draft. In places, the document also seems to be lacking an organized story (with some undesirable jumps, backtracking and repetitions within the text). [Ileana Bladé, Spain]	Thanks
2-2	2	0	0	0	0	Chapter 2 reports new work on the global abundance and trends of all the important greenhouse gases (GHGs), however there are no citations to papers describing and using the isotopic composition and trends of GHGs, a likely more important source of information about GHG sources and sinks than the abundance and trend data. This research could be reviewed in Chapter 6. Please ensure that these papers are reviewed in either Chapter 2 or Chapter 6. The GHG isotope variation papers that should be discussed are: Allison, C. & R. Francey, Verifying global trends in atmospheric carbon dioxide stable isotopes, J. Geophys. Res., 112, D21304, doi:10.1029/2006JD007345, 2007. Rayner, P., R. Law, C. Allison, R. Francey, C. Trudinger & C. Pickett-Heaps, The inter-annual variability of the global carbon cycle (1992-2005) inferred by inversion of atmospheric CO2 and δ13CO2, Global Biogeochem. Cycles, 22, GB3008, doi:10.1029/2007GB003068, 2008 Levin, I., T. Naegler, B. Kromer, M. Diehl, R. Francey, A. Gomez-Pelaez, P. Steele, D. Wagenbach, R. Weller & D. Worthy, Observations and modelling of the global distribution and long-term trend of atmospheric 14CO2, Tellus B, 62(1), 26-46, 2010 Welp, L., ...R. Francey, C. Allison et al., El Nino effects on the isotopic composition of oxygen in atmospheric CO2, Nature, 477, 579-582, 2011 Ferretti, D., J. Miller, J. White, K. Lassey, D. Lowe & D. Etheridge, Stable isotopes provide revised global limits of aerobic methane emissions from plants, Atmospheric Chemistry and Physics, 7, 237-241, 2007. Petrenko, V., D. Etheridge, R. Weiss, E. Brook, H. Schaefer, J. Severinghaus, A. Smith, D. Lowe, Q. Hua & K. Riedel, Methane from the East Siberian Arctic Shelf, Science, 329, 1146-1147, 2010 Park, S., D. Etheridge, P. Fraser, P. Krummel, R. Langenfelds, P. Steele, C. Trudinger et al., Trends and seasonal cycles in the isotopic composition of nitrous oxide since 1940, Nature Geosciences,5, 261-265, 2012 [Government of Australia]	Rejected - outside scope of Chapter 2.
2-3	2	0	0	0	0	Papers on greenhouse gas trends, sources and sinks that should be reviewed in Chapter 2 or Chapter 6: Francey, R., C. Trudinger, M. van der Schoot, P. Krummel, P. Steele & R. Langenfelds, Differences between trends in atmospheric CO2 and the reported trends in anthropogenic CO2 emissions, Tellus B, 62: 316–328, doi: 10.1111/j.1600-0889.2010.00472.x, 2010 - this is an important paper because it describes an apparent inconsistency between 'bottom-up' and 'top-down' estimates of fossil fuel CO2 emissions.	Rejected - outside scope of Chapter 2.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Pan, Y., ...J. Canadell, et al., A large and persistent carbon sink in the world's forests, 1990-2007, Science, 333, 988-993, 2011 Peters, G., ...J. Canadell. M. Raupach et al., Rapid growth in CO2 emissions after the 2008-2009 global financial crisis, Nature Climate Change, 2, 2-4, 2012 Raupach, M., J. Canadell & C. Le Quere, Anthropogenic and biophysical contributors to increasing atmospheric CO2 growth rate and airborne fraction, Biogeosciences, 5, 1601-1613, 2008 Raupach, M., Pinning down the land carbon sink. Nature Climate Change, 1(3), 148-149, 2011 Le Quéré, C., C. Rodenbeck, E. Buitenhuis, T. Conway, R. Langenfelds, A. Gomez, C. Labuschagne, M. Ramonet, T. Nakazawa, N. Metzl, N. Gillett & M. Heimann, Saturation of the Southern Ocean CO2 sink due to recent climate change, Science, 316, 1735-1738, 2007 Le Quere, C., C. Rodenbeck, E. Buitenhuis, T. Conway, R. Langenfelds, A. Gomez, C. Labus change, M. Ramonet, T. Nakazawa, N. Metzl, N. Gillett & M. Heimann, Response to Comments on 'Saturation of the Southern Ocean CO2 Sink Due to Recent Climate Change', Science, 319, doi:10.1126/science.1147315, 2008. Law, R., R. Matear & R. Francey, Comment on 'Saturation of the Southern Ocean CO2 Sink Due to Recent Climate Change, Science, 319, doi: 10.1126/science.1149077, 2008. Patra, P., M. Takigawa, K. Ishijima, B.-C. Choi, D. Cunnold, E. Dlugokencky, P. Fraser, A. Gomez-Pelaez, T.-Y. Goo, J.-S. Kim, P. Krummel, R. Langenfelds, F. Meinhardt, H. Mukai, S. O'Doherty, R. Prinn, P. Simmonds, P. Steele, Y. Tohjima, K. Tsuboi, K. Uhse, R. Weiss, D. Worthy & T. Nakazawa, Growth rate, seasonal, synoptic & diurnal variations and budget of methane in lower atmosphere, J. Meteorological Society of Japan, 87:4, 635-663, 2009 Patra, P., ...R. Law, Z. Loh, et al., , TransCom model simulations of CH4 and related species: linking transport, surface flux and chemical loss with CH4 variability in the troposphere and lower stratosphere, Atmos. Chem. Phys., 11, 12813-12837, 2011. Le Quéré, C., M. Raupach, J. Canadell, G. Marland, L. Bopp, P. Ciais, T. Conway, S. Doney, R. Feely, P. Friedlingstein, K. Gurney, R. Houghton, J. House, C. Huntingford, P. Levy, M. Lomas, J. Majkut, N. Metzl, J. Ometto, G. Peters, C. Prentice, J. Randerson, S. Running, J. Sarmiento, U. Schuster, S. Sitch, T. Takahashi, N. Viovy, G. van der Werf & I. Woodward, , Trends in the sources and sinks of carbon dioxide, Nature Geosciences, 2, doi:10.1038/ngeo689, 2009 Chevallier, F., P. Ciais , T. Conway, T. Aalto, B. Anderson, P. Bousquet, E. Brunke, L. Ciattaglia, Y. Esaki, M. Fröhlich, A. Gomez-Pelaez, L. Haszpra, P. Krummel, R. Langenfelds, M. Leuenberger, T. Machida, F. Maignan, H. Matsueda, J-A. Morguí, H. Mukai, T. Nakazawa, P. Peylin, M. Ramonet, L. Rivier, Y. Sawa, M. Schmidt, P. Steele, S. Vay, A. Vermeulen, S. Wofsy & D. Worthy: CO2 surface fluxes at grid point scale estimated from a global 21-year reanalysis of atmospheric measurements, J. Geophys. Res., 115, D21307,doi:10.1029/2010JD013887, 2010 [Government of Australia]	
2-4	2	0	0	0	0	Papers on long-term ice core CO2 record that should be reviewed in Chapter 2 or Chapter 5 include: Ahn, J., ...D. Etheridge, M. Rubino et al., Atmospheric CO2 over the last 1000 years: a high resolution record from the West Antarctic Ice Sheet (WAIS) Divide ice core, Global Biogeochemical Cycles, 26, GB2027, doi:10.1029/2011GB004247, 2012 Other ice core/firn papers that need to be reviewed in Chapter 2 or Chapter 5: Worton et al., CF4, C2F6, Environ. Sci. Tech., 41, 2184-2189, 2007 (may have been reviewed in the 4th Assessment) Witrant et al., trace gases in firn, Atmos. Chem. Phys. Disc., 11, 23029-23080, 2011 Buizert et al., trace gases in Greenland ice core, Atmos. Chem. Phys. Disc.,11, 15975-16021, 2011 [Government of Australia]	Rejected - outside scope of Chapter 2.
2-5	2	0	0	0	0	Important paper on CFC lifetimes that should be included in Chapter 2:	Rejected - outside scope of Chapter 2.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Rigby, M., ...P. Krummel, P. Steele, P. Fraser, et al., Re-evaluation of the lifetimes of the major CFCs and CH3CCl3 using atmospheric trends, Atmos. Chem. Phys. Discuss., 12, 24469-24499, 2012 [Government of Australia]	
2-6	2	0	0	0	0	Important paper on CCl4 regional emissions that should be included in Chapter 2: Xiao, X., R. Prinn, P. Fraser, R. Weiss, P. Simmonds, S. O'Doherty, B. Miller, P. Salameh, C. Harth, P. Krummel, A. Golombek, L. Porter, J. Elkins, G. Dutton, B. Hall, P. Steele, R. Wang & D. Cunnold, Atmospheric three-dimensional inverse modelling of regional industrial emissions and global oceanic uptake of carbon tetrachloride, Atmos. Chem. Phys. Discussions, 10, 12225-12260, 2010 [Government of Australia]	Added.
2-7	2	0	0	0	0	Important paper on NF3 (update to global trend and revised calibration) that should be included in Chapter 2: Arnold, T., ...P. Steele et al., Nitrogen trifluoride global emissions estimated from updated atmospheric measurements, PNAS, submitted [Government of Australia]	Arnold et al. citation added
2-8	2	0	0	0	0	Important paper on PFCs that should be included in Chapter 2: Laube, J., ...P. Fraser, et al., Distributions, long-term trends and emissions of four perfluorocarbons in remote parts of the atmosphere and firn air, Atmos. Chem. Phys., 12, 4081-4090, 2012 [Government of Australia]	Already in appendix.
2-9	2	0	0	0	0	Important paper on CF3SF5 should be included in Chapter 2. CF3SF5 data should be added to Table 2.A.1 (Appendix 2.A) and should be included in the Radiative Forcing calculations in Chapter 8: Sturges, W., ...P. Fraser et al., Emissions halted of the potent greenhouse gas CF3SF5, Atmos. Chem. Phys., 12, 3653-3658, 2012 [Government of Australia]	Rejected - With low constant abundance and no current emissions, despite large radiative efficiency, it was not included here.
2-10	2	0	0	0	0	Executive summary of this chapter is inconsistent in style, formatting and referencing to other Chapters. I.e. it does not have bolded main statements or square bracket references to relevant chapter sections as do other chapters. Consistency across chapters in this is necessary. [Government of Australia]	accepted. ES has a better version now.
2-11	2	0	0	0	0	It is not easy to trace statements from the SPM to the exec summary of this chapter to the chapter text. For example, the first statement under Atmosphere Observations in the SPM (p. 3, line 1), states that 'Each of the last three decades has been significantly warmer than all preceding decades since the 1850s' {2.4}. This statement is not in the Executive Summary and is very hard to find in the Chapter text (it is actually only shown in a figure). Such a statement is useful for policy makers, however without a clear reference to the Chapter text and the publications used to support that statement, it loses its usefulness. [Government of Australia]	accepted. ES has a better version now.
2-12	2	0	0	0	0	This chapter makes little mention of the ENSO despite IPCC 4AR frequently mentioning it and showing that it influences a large number of climate factors including temperature. There's also no explicit mention of the 1976-1977 Pacific climate shift, which is odd given that the executive summary of IPCC 4AR chapter 3 (Observations: Surface and Atmospheric Climate Change) stated "The 1976-1977 climate shift, related to the phase change in the Pacific Decadal Oscillation and more frequent El Ninos... This shift was even mentioned in chapter 9 ("Understanding and Attributing Climate Change"). It's not simply more frequent El Ninos but that the ENSO has been dominated by conditions on the El Nino side of absolutely neutral (SOI=zero) since May-June 1976. ENSO conditions are a continuum, not three states divided by artbitrary thresholds, so be wary of any papers that talk only of El Nino or La Nina "events". Add comments to the chapter accordingly. [John McLean, Australia]	Rejected. ENSO is mentioned at many instances; the arguably most prominent change in the ENSO system on decadal scales is the dominance of positive SOI since a few years, which is discussed and is an update to AR4.
2-13	2	0	0	0	0	The dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. SOI = 0), as has been the case since 1976 due to the Pacific Climate Shift, would be expected to decrease Walker Circulation and increase Hadley Circulation, and by doing so spread warm air into the mid-latitudes, causing temperatures there to increase above the 1945-76 levels. Further, the Stefan-Boltzmann equation shows that the dispersion of warm air reduces the radiative cooling so it's not only warm air distribution that's occurring but a reduction in	Rejected. The most prominent change in SOI on decadal scales is the dominance of positive SOI since a few years, which is discussed at several places (strengthening of the Walker circulation).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						cooling LWR, which itself leads to higher minimum temperatures. The additional warm air and warmer conditions caused by increased Walker Circulation would also contribute to increased heat waves. (references for that shift - Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000) and Trenberth et al (2002) - Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures) [John McLean, Australia]	
2-14	2	0	0	0	0	This chapter seems ignorant of basic hydrology that says under drought conditions, with a dry land surface and reduction in vegetation moisture, evapotranspiration will be reduced, which means that less heat used in evaporation and more heat will be available to warm the Earth's surface. A reduction in precipitation, especially in the 25-35 latitude bands where humidity is lower, will therefore very likely result in higher average global temperatures. [John McLean, Australia]	This is a speculative statement that is not backed up by any references, etc. and may or may not be true. Thus we decline to add any text. Further, the authors are well aware of the basics that with a reduced latent heat flux more goes to the sensible heat flux and temps increase.
2-15	2	0	0			FAQs in this chapter are embedded within sections of the chapter rather than placed all together at the end like other chapters. Suggest making this consistent and placing the FAQs at the end. [Government of Canada]	editorial
2-16	2	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
2-17	2	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
2-18	2	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
2-19	2	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	We will include reference to the specific sections.
2-20	2	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
2-21	2	0	6			Two sections (2.2 and 2.6) lack the very effective short summary sub-sections that you have used elsewhere. We would encourage that these are added to complete a consistent structuring across all sections. [Thomas Stocker/ WGI TSU, Switzerland]	Will include short summary statements.
2-22	2	0	8			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
2-23	2	0	9			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Will avoide to use personal pronouns

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-24	2	0	10			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	The main transfer from Chapter to Annex II is the data on temperature and GHG concentrations. Done.
2-25	2	0		0		The information contained in the Likelihood table (Table 1.1) and Confidence figure (Figure 1.12) is critical to interpreting conclusions throughout the document. This information should be repeated in the front of the SPM, the TS and each Chapter and the terminology should be applied consistently. [Government of United States of America]	accepted
2-26	2	0		0		It does not appear that the language for confidence and certainty was used consistently throughout this section. [Government of United States of America]	accepted. Consisistency checked
2-27	2	0				Compared to the FOD version the structure of the chapter is clearer and better understandable for the readers. The reorganization of the table of contents gives a more organic and intuitive vision of the main items regarding atmosphere and surface observations treated in the chapter. The same comment regards the executive summary. The fractioning of the summary in different sections gives a more intuitive picture of the main changes occurring in atmospheric and surface observations. [Florinda Artuso, Italy]	Thanks
2-28	2	0				A prominent or even dominant feature of the assessment in chapter 2 of changes in the hydrological cycle is a search for changes in globally averaged precipitation. No strong conclusion is arrived at. Some sections could leave the reader with the impression that this is because either the uncertainties obscure any trends or the region-to-region variations cancel each other out. But the latter impression may be just what the assessment should convey. The report mentions repeatedly (as in section 7.6.2 and elsewhere) the expectation that the hydrological cycle wiil become more intense in a warmer world (wet regions get wetter and dry regions drier). So I suggest that chapter 2 should reconsider its relative lack of emphasis on evidence for intensification (of totals and of extremes). This point about the observations is particularly important because most of the mentions of wet-get-wetter elsewhere are supported mainly or only by model results or appeals to process understanding. One source that deals partly with observations is Zhang, X., F.W. Zwiers, G.C. Hegerl, F.H. Lambert, N.P. Gillett, S. Solomon, P.A. Stott, and T. Nozawa, 2007, Detection of human influence on twentieth-century precipitation trends. Nature, 448(7152), 461-465. To facillitate assessment of such evidence, identified regional trends could be sorted according to some suitable measure, perhaps the magnitude of the quantity (precipitation minus evaporation). Changes in | P-E | over the ocean, where water supply is not limiting, could be easier to assess than changes over land. [J. Graham Cogley, Canada]	accepted
2-29	2	0				Same for Ch2 - I have read the parts to do with extremes and I find the statement "..there is medium confidence that the length or number of warm spells, including heat waves has increased" much too cautious. [Dim Coumou, Germany]	Noted. This is a 'global' conclusion which we feel is justified. This assessment has been made in part due to definitional differences in heat wave studies coming to different conclusions and the fact that for large regions of the globe e.g. Africa and parts of South America there are very few studies from which to draw firm conclusions.
2-30	2	0				Congratulation to Co-lead authors, and lead authors for preparing such huge information with new materials and in a new style. You begin with very excellent beginning. [Rahimzadeh Fatemeh, Iran]	Thanks
2-31	2	0				Chapter 2 should have a FAQ section asking: “How do we know that the increase in GHG is caused by humans?” [Dietrich Feist, Germany]	Out of the scope of the Chapter
2-32	2	0				Now a generally well written chapter with balanced conclusions. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Thanks
2-33	2	0				The analyses presented in the Chapter confirmed most of conclusions of AR4 but now with more scientific understanding. As was in AR4 there are not many analyses for South America. Some analyses presented in Chapter 2 of PBMC and in many other studies for South America, e.g., temperature tendencies in South America, could have been considered - there are some analyses based in situ measurements but not for South America. [Government of Brazil]	What is PBMC?

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-34	2	0				About land use change and urban heat island effects there are not comments about land use changes in South America. There is a conclusion that is: "based primarily upon the range of urban minus rural comparisons and the degree of agreement with a broad range of reanalyses products it is concluded that is likely that residual biases arising from Urban Heat Islands and Land-Use Land-Cover changes account for no more than 10% of the land surface air temperature warming trend globally and 25% regionally in rapidly developing regions." However, there is no comments about how these numbers were calculated. [Government of Brazil]	Noted. There are no known analyses of these issues in S. America which can be cited. The summary is directly traceable to the discussion in the rest of the sub-section which precedes it.
2-35	2	0				On the second page, for temperature, is mentioned: "1986-2005 (reference period for the modelling chapters and the Atlas in Annex 1). However, on page 6 is mentioned: "the time interval 1961-1990 has been chosen as the climatological reference period (or normal period) for averaging". It would be good if the analyses were done for the same periods. Farther, there are analyses for period 1979-2011 for many variables but not for global mean temperature (box 2.2, table 1). [Government of Brazil]	BASE / TREND PERIODS
2-36	2	0				An overall very complete report. Congratulations to all authors. [Government of Chile]	Thanks
2-37	2	0				Some figure has the zero line drown and others not. It is possible to draw the zero line in all graphs to easily see the above and below zero anolamies? (when correspond) [Government of Chile]	Noted. Some degree of homogenisation of the plots has been done.
2-38	2	0				Section: 2.2.3.2 Only aerosol mass presented. Aerosol number is climatically even more important, and there are already several long term data e.g. those obtained at SMEAR II station Finland [European Union]	A table entry to these measurements is available in section 2.2.3
2-39	2	0				In this chapter global mean temperature are used. It is necessary to explain how they are defined and computed and what is the accurracy of the value for a given year [Government of France]	Noted. There are substantial discussions within both the chapter (including boxes 2.1 and 2.2) and the appendix of all the aspects surrounding calculation of global mean estimates and the uncertainties surrounding their calculation. It is unclear why the reviewer believes this is not the case or how they propose it should be remedied. Without such specific feedback and noting no specific changes were requested none are made here.
2-40	2	0				In general, some parts of the Chapter include too much detailed discussions which let a feeling of confusion which attenuates the major conclusions of the Chapter. It is typically the case of 2.4.1.3 which discusses the important issue of urbanization on land air temperature. It is likely that many readers may be lost by some of the presented details which are sometimes very undirectly linked to the conclusion of 2.4.1.3. [Government of France]	Noted. Explicit guidance from TSU was that we cover in some detail contentious issues. Specific comments on this section have led to some modifications but we note that several commenters, including TSU, suggested the current section was sufficient and well written so wholesale changes are not being made in response to this comment.
2-41	2	0				Many typo errors (several tens), like missing blanks, in Chapter 2. [Government of France]	editorial
2-42	2	0				The "trends" addressed here are "technical " (i.e. over any period one can extract a "trend") which is a priori fine, but when addressing the very latest years/decade, or changes in "trends" this may be perceived as questionable, e.g. wrt multidecadal oscillations. See e.g. p5 lin 37-38 "largely offset by recent changes" This point may need to be addressed in chapter 1, which should introduce e.g. appendix 2.A. (See this chapter p7, lines 11-16.) . Recent changes may not be climate change, and this could be clear. [Government of France]	BASE / TREND PERIODS
2-43	2	0				Chapter two gives a good overview of observed and future change in the different elements. However, we would like to encourage you to improve the readability and that you especially focus on this in the key stories that you would like the chapter to communicate to non-specialists. [Government of NORWAY]	Noted.
2-44	2	0				Comparing trends for different time periods, e.g., 1901-2005, 1979-2008, 1950-1999, etc., is very confusing throughout this chapter and may even be misleading due to interdecadal variability. There needs to be a discussion about the background decadal variability which makes it more difficult to compare means and growth rates between different time periods. There is some discussion of this in Box 2.5 but it is very limited and does not link back to observed changes in GHG, aerosols, hydrology cycle, etc. [Government of United States of America]	Noted. Standard trend periods are 1901-present, 1901-1950, 1951-present and 1979-present. In few instances only, other periods are used. Tables will be updated where possible.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-45	2	0				Section 2.1: Suggest that the authors consider moving Box 2.2 ahead of Box 2.1 [Government of United States of America]	Changed.
2-46	2	0				Many of the figures are difficult to read and, therefore interpret even in the high resolution file, This is particularly true of figures that include global maps with small symbols indicating statistical significance on top of colors. See for example figures 2.22,2.25,2.29,2.30,2.31,2.32, 2.33, 2.37, and Box 2.4, Figure 1. [Government of United States of America]	Noted. We have worked with NCDC's graphics team to optimize all figures herein.
2-47	2	0				the observation chapter has been delt quite well particulary when comparing new result in this this asseessment and those in AR4. Authors have been quite clear in addressing challenges in observations which could impact data; they have indicated inherent errors due to changing technologies, site changes and more. [Government of United Republic of Tanzania]	Noted.
2-48	2	0				So much improvement from the first order draft! Great job! [Birgit Hassler, United States of America]	Thanks
2-49	2	0				There is an inconsistency between the way that uncertaintes are dealt with. Uncertainty estimates from individual analyses (such as ERSST or HadSST3) are not shown. The assumption seems to be that comparing central estimates spans the full uncertainty range. This isn't the case. See Figures in Morice et al. (2012) showing the ensemble spread from parametric uncertainty in HadCRUT4 compared to the structural uncertainty estimated by comparing central estimates. The parametric uncertainty is important in the sense that it is comparable to or larger than structural uncertainty during the most recent period of warming from 1979. The same thing holds for ERSST uncertainty estimates in the period 1920-1950: large, neglected uncertainty. This is an important omission. It is also inconsistent with the way such uncertainties are dealt with in Chapter 3 and furthermore leads to mistakes being made in the SPM where land temperature series and sea-surface temperature series are labelled, erroneously, as not having available uncertainty estimates. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Uncertainty estimates of individual analyses are shown when practicable, e.g., Fig. 2.18. If Reviewer refers to Fig 2.19, it is clearly impractical to add error bars to all curves there: this figure is barely readable as is. That being said, the attempt will be made to emphasize for the reader the presence of invisible uncertainty envelopes around curves representing individual analyses
2-50	2	0				in situ & in-situ both used [Elizabeth Kent, United Kingdom]	Editorial: to ensure consistent usage
2-51	2	0				sea-surface temperature and sea surface temperature [Elizabeth Kent, United Kingdom]	Editorial: to ensure consistent usage
2-52	2	0				Long-term (and long term) is used throughout. It appears to be defined (Chapter 1, page 11, line 57) as being > 40 years. On page 48, line 52 trends for the satellite era are described as "long term". The O'Dell paper quoted refers to an 18 year climatology and the Rausch paper 25 years. Given the variability in trends calculated over different periods the authors should be specific about the period over which each "long term" trend is calculated. [Elizabeth Kent, United Kingdom]	Editorial: to ensure consistent usage
2-53	2	0				Despite the few blunt comments below, I would like to say that on the whole I think this chapter is good. [Paul Matthews, United Kingdom]	Thanks
2-54	2	0				[No comment - merely entry on blank line to ensure that processing doesn't abort at this point.] [John McLean, Australia]	Noted.
2-55	2	0				[No comment - merely entry on blank line to ensure that processing doesn't abort at this point.] [John McLean, Australia]	Noted.
2-56	2	0				Annex II. In your discussion of historical record refer ahead to Annex II tables and proof the numbers you want in these: Table AII.1.3: Historical global decadal-mean global surface-air temperature (°C) relative to 1961–1990 average. Also, check on the historical abundances of the GHG (incl. pre-industrial) and present-day (2010?) values Table AII.1.1: Historical abundances of the Kyoto greenhouse gases AII.4: Abundances of the Well Mixed Greenhouse Gases Table AII.4.1: CO2 abundance (ppm) Table AII.4.2: CH4 abundance (ppb) Table AII.4.3: N2O abundance (ppb) Table AII.4.4: SF6 abundance (ppt) Table AII.4.5: CF4 abundance (ppt)	Contributed to Annex II since LA4 in Hobart

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Table AII.4.6: C2F6 abundance (ppt) Table AII.4.7: C6F14 abundance (ppt) [Michael Prather, United States of America]	
2-57	2	0				Chapter 2 reviewed by me there was no need for any change. Thanks to all authors. [YAĞAN Serpil, Turkey]	Thanks
2-58	2	0				This chapter could benefit from a more controlled use of the word "trend". This caught my eye particularly when reading section 2.2.3 where "trends" are computed on some ten years of data and (as noted in specific comments) clearly a consequence of low frequency variability. Is there not a general IPCC guideline on the use of the word? Could it be used only when computing over the traditional 30-year climate-averaging period or longer? Could "decadal change" be used instead for changes computed over shorter periods? [Adrian Simmons, United Kingdom]	A separate box will clarify our use of the word trend on different temporal scales
2-59	2	0				Another general comment is that a number of tables of values might usefully drop the last figure after the decimal point. This is especially the case where confidence is not high in individual figures or where various estimates are shown that differ quite considerably one from the other. Table 2.11 is just one example where showing one rather than two figures after the decimal point would make it easier to the eye of the reader without sacrificing useful information. [Adrian Simmons, United Kingdom]	considered
2-60	2	0				The final general comment is that it is puzzling why just three figures find their way into the supplementary material. I could not see the logic for these three in particular being supplemental rather than core. Is it simply that some arbitrary limit on the number of figures has been exceeded? If so, this is not reader-friendly, as to go looking elsewhere for a supplementary figure is a nuisance when there are so few of them. I would also question why some of the text is supplemental, and provide a detailed comment on this. [Adrian Simmons, United Kingdom]	It is impossible for the LAs to know which text the reviewer refers to. Chapter CLAs moved some information to the SM to meet page limits. This decision was based on relative contributions to radiative forcing.
2-61	2	0				1) The chapter was in fairly good shape in the FOD but this draft is disappointing. It has not kept up with recent literature. It has failed to address many comments I made in the last round. It fails to do an assessment by taking into account the fact that many studies are flawed and demonstrated to be so by use of obsolete or erroneous data, flawed methods, incomplete data, etc. Instead it lumps them all together as equally credible and says there is “low confidence”. This comment applies especially to reanalyses: the older reanalyses are known to have major problems and should be discounted. Newer ones also have problems that are documented in many papers (not referred to) but ERA-interim, for example, is much better that any earlier ones. Unfortunately the term “low confidence” applies to this assessment. [Kevin Trenberth, United States of America]	Noted. ERA-40 reanalyses are removed from two of the figures, and one figures showing ERA-Interim is moved from the supplemental material to the main text. The assessed literature still relies to a large extent on the older reanalyses.
2-62	2	0				2) The chapter is supposed to be by all authors and a basic procedure in IPCC is that no-one should assess their own work. In section 2.3 this is blatantly ignored and the biases of one lead author (Martin Wild) are very apparent. The CLAs and other LAs should take ownership of this and other sections. [Kevin Trenberth, United States of America]	The revised section has been reviewed by expert CLAs, LAs and CAs to make sure that the section is balanced.
2-63	2	0				3) Another problematic section is the one on DTR. How can such statements be made damning any knowledge about DTR without also damning all the studies on max and min temperature? It is clear that different authors were responsible for different sections [Kevin Trenberth, United States of America]	Noted. We have added text to clarify that medium confidence in DTR does not beget medium confidence in max and min temperatures.
2-64	2	0				4) The order of the sections is strange. Why are extremes discussed before the changes in atmospheric circulation and the phenomena that cause the extremes? Section 2.7 should probably be moved up to provide context. [Kevin Trenberth, United States of America]	Rejected. Extremes were moved closer to the changes in the mean.
2-65	2	0				5) The whole document is not very useful or helpful by failing to frame the analyses that are put forward. What should be the expectations? Why should a linear trend be a useful descriptor of the record of any quantity, especially for long periods? Why shouldn’t one expect to see signals of volcanic eruptions, depletion of stratospheric ozone and its recovery, and natural variability such as ENSO. If climate change is what is sought in the record then all these other factors need to be factored in. If the mean changes, then what is the change in variance (rather than the change in a percentile)? For instance, the increasing precipitation on land	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						is associated mainly with recent La Niña events, and there are corresponding decreases over the ocean. But the ocean precipitation is not included or discussed and the results are highly misleading. In dealing with extremes, one would like to know whether they are getting worse or not given the same conditions, and not be biased by the changes in the mean climate or incidence of ENSO etc. This is a major shortcoming. It relates to how the extremes are analyzed and presented. One might regress against ENSO to get that signal out and look at residuals. It also relates to possible normalizations such as percentage changes in moisture variables (consistent with Clausius Clapeyron) instead of absolute values, and further normalizations relative to changes in the hydrological cycle per unit of change in temperature. There are many such studies in the literature, which is itself confusing because the normalization can be done in multiple ways (monthly means, annual means, local, regional or global temperatures, etc). The SREX did a poor job on this and this assessment ignores all these issues completely. Now the assessment is dependent on the literature, but the issues are discussed e.g. in Trenberth (2011), and they should be properly addressed. [Kevin Trenberth, United States of America]	
2-66	2	0				6) Precipitation is a unique variable because it is intermittent. It changes in multiple ways through changes in amount, intensity, frequency, type, and sequences. None of this is adequately discussed in an integrated way. In dealing with changes in the hydrological cycle only amount is mentioned. Intensity and frequency are partly shuffled off into extremes, type is in a different section on snow. [Kevin Trenberth, United States of America]	considered in extremes section
2-67	2	0				7) The layout of the report is reasonable within each section in that it starts with a brief summary from AR4 and notes the main progress. Most of these assessments are reasonable but not the one on tropical storms where all of the comments in AR4 still apply today and provide quite a nice summary of the actual state of affairs. [Kevin Trenberth, United States of America]	new text
2-68	2	0				8) There is no discussion of major extreme events that have occurred since AR4 such as the “Black Saturday” heat waves and wild fires in Victoria, Australia in 2009, the Russian heat wave and Pakistan floods in 2010, the Texas heat waves and wild fires in 2011, and the record breaking heat waves and wild fires in the US in 2012. While there may not be much in the literature about the recent events in the last year, the time series should be updated and include some basis for commentary. In 2012 we could also mention the very wet spring and summer in the U.K., the flooding in southern Russia, North Korea, Beijing, etc., and heat in Siberia. Where is the relevance of this chapter to the real world? [Kevin Trenberth, United States of America]	new literature included
2-69	2	0				9) Curiously, in several places, older literature is included without any good reason. This is material that was dealt with in AR4, for instance, and it is selectively brought back and is biased. It includes a lot of discredited papers by climate change deniers, for instance. It is evident in several places, especially 2.4.1.3. It is recommended that this be left dead and buried, and not resurrected. [Kevin Trenberth, United States of America]	the reasons for the inclusion are explained
2-70	2	0				10) There are some sections especially in 2.7, such as 2.7.6.3 especially, that would seem to belong in chapter 14. Careful cross referencing and consultation is required. [Kevin Trenberth, United States of America]	Noted. Cross-referencing will be improved, section 2.7.6.3 (now 2.7.6.2) is shortened.
2-71	2	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on GHGs, Ozone, Aerosols, Clouds, Land Cover, Burnt Area and Soil Moisture. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendael and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	considered
2-72	2	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on GHGs,	repeated comment

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ozone, Aerosols, Clouds, Land Cover, Burnt Area and Soil Moisture. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendael and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	
2-73	2	0				The current draft is excellent. [Robert Waterland, United States of America]	Thanks
2-74	2	1	1	200	70	please check text: often blank space between two words is missing and the two consecutive words are attached each other [Claudio Cassardo, Italy]	editorial
2-75	2	1	1			general comment on entire text: This draft is a significant improvement over the FOD! [Karen Rosenlof, United States of America]	Thanks
2-76	2	1		72		General comment about this chapter. I struggled to keep track of all the acronyms used. Please consider putting an acronym list in the chapter (I could not see one listed in the contents list). [Peter Burt, United Kingdom]	editorial
2-77	2	1		100		In developing future scenarios of anthropogenic forcing, why don't consider the possibility of increasing aerosol release? [Joshua Xiouhua Fu, United States of America]	The issue of geoengineering is not discussed in this chapter but to some extent in chapters 6 and 7. We further refer to the special report on geoengineering released by IPCC in 2012.
2-78	2	1		100		In the next assessment, I would like to at least contribute to the writing of the section on blocking. [Anthony Lupo, United States of America]	Noted.
2-79	2	1		100		In this chapter and elsewhere in the assessment, make sure referenceing is in Chronological order (page 58 line 26-27). While it is a small point, in journals this would be required. It will help readability. [Anthony Lupo, United States of America]	Editorial.
2-80	2	1		200		9. This paragraph refers to the entire Chapter 2. Chapter 2 reviews some of the published information on the topic "Atmosphere and Surface". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scietists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	Rejected. Chapter 2 is not about Anthropogenic Global Warming, not attribution statements are made, and no models are used.
2-81	2	1				trends are named as "increasing" and "decreasing", where "positive" and "negative" would be appropriate in several places in this chapter. Since there is a distinct difference in meaning, please correct throughout. [Jan Cermak, Germany]	Changed in many instances.
2-82	2	1				General comment: What is was missing overall in chapter-2 is a critical review on the existing climate data records from satellites. There are so far only a very few ECV climate data records existing from satellites. There are however, currently a lot of initatives going on to generate high quality data records from satellite, like the ESA Climate Change Initative, the EUMETSAT scope-CM program and the NASA Measures program. These efforts should be highlighted somewhere. I was also missing a reference to GCOS, which basically establishes the references for ECVs and measurements. [Alexander Loew, Germany]	satellite data are considered
2-83	2	1				In general, a good chapter. Most of my comments are only about details, though one comment on precipitation	Thanks

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						trends may be controversial. [David Parker, United Kingdom of Great Britain & Northern Ireland]	
2-84	2	1				I had some rather serious concerns about the first draft of this chapter and am happy to see that it is much improved. However, it is still a hard chapter to read, even for "experts", and I imagine non-experts will be overwhelmed by the material and complexity. If the main audience of the technical chapters is the academic community, particularly graduate students, I do fear that this chapter will likely send some of those readers straight into the world of models! [Dian Seidel, United States of America]	Noted.
2-85	2	2	3	2	3	Are decreases in Europe and North America for NOx or CO? [Qi Tang, United States]	The changes discussed are mainly for Nox, but also CO. The sentencece will be clarified.
2-86	2	2	3	2	5	The 2.2.3 and 2.5.7 Sub-sections of 2nd Chapter deal with measrement and morphological aspects of aerosols (surface and vertical) and clouds (feedbacks on radiation and tmperature), respectively. Apart from the surface-level and columnar parameters, vertical distributions of aerosols are also needed to identify the aerosol layer structures, which are needed for reliable estimation of radiative forcing exerted by aerosols. The active remote sensing systems provide aerosol distributions with ultra-high spatiotemporal resolution. The passive remote sensing systems yield columnar aerosol optical, microphysical and radiative characteristics, but their operation is limited to daytime and clear-sky conditions. [Panuganti, C.S. Devara, India]	The reviewer's concerns are abundantly addressed in Chapter 7.
2-87	2	2	30	2	32	Sentence is incomplete and wrong. It needs correction. [Umesh Kulshrestha, India]	The sentence is clarified.
2-88	2	2		72		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. Some figures look like relatively low quality jpegs that should be improved at some point. [Larry Thomason, United States of America]	noted
2-89	2	3	0	3	0	The IPCC makes itself look foolish by treating atmospheric composition with greater priority than temperature. This is making the huge assumption that greenhouse gases have a major influence. Remove this section or move it to much later in the chapter. [John McLean, Australia]	Rejected - inconsistent with bulk of peer-reviewed literature.
2-90	2	3	1	3	10	The first paragraph about the "Atmosheric Composition" is a bit confusing. There is given only the tendency for long-lived greenhouse gases and other gases for the last decade. In the first passage of the executive summary there should be also informed about the whole trend - more than only the last ten years- for the long-lived greenhouse gases and others. [Government of Germany]	Relative changes since PI added.
2-91	2	3	1	4	20	Overall, in my opinion, I see improvement in several sections with more caution and humility applied when making claims about what we know about the climate. [John Christy, United States of America]	thanks
2-92	2	3	1	4	25	In chapter two the introduction is dealing with the uncertainties of measurements but any comments in the executive summary are missing. It would be advantageous if one can find at least one or two sentences about principal uncertainites in measurement [Government of Germany]	Taken into account
2-93	2	3	1	5	48	I did not have time to carefully cross-reference the executive summary with the chapter contents, so I don´t have any comments on the executive summary except to note that it reads well. [Ileana Bladé, Spain]	Thanks
2-94	2	3	1			Consider adding a table which lists each topic by certainty of it occurring (or certainty that it has occurred) in addition to the present organization by topic. [Government of United States of America]	Considered. This is the intention of the ES
2-95	2	3	1			Reading section 2.8 it appears that all the different observations are largely consistent, and collectively provide an unequivocal and cohererent picture of a warming climate. It would be nice to see this highlighted in the summary of the chapter, in addition to a list of all the individual changes. [Reto Knutti, Switzerland]	Thanks, this is reflected in the ES
2-96	2	3	1			I miss numbers in the ExSum. Almost all conclusions are qualitatively, while numbers are available. [Guus Velders, Netherlands]	Giving numbers in the ES depends on the variable and the variability of the parameter considered. If no values are given, the summary statement at the end of each section is providing these numbers.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-97	2	3	1			Executive summary: As outlined in our general guidance, the ES currently lacks lines of cite to the underlying Chapter sections. Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. [Thomas Stocker/ WGI TSU, Switzerland]	Done
2-98	2	3	1			Executive summary: As outlined in our general guidance, the first sentence (or two) of each paragraph in the Executive summary should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Done
2-99	2	3	3	3	3	It is not clear which changes are meant - "changes" in comparison to which period or what? [Government of Germany]	Noted. Text has changed
2-100	2	3	5	4	25	For consistency with rest of Ch 2 Exec Summary and because it would be very helpful to readers, findings in Atmospheric Composition, Radiation Budget, and Temperature subsections should state whether the AR5 findings are consistent with, stronger than, or less consistent with, findings in AR4. [Government of United States of America]	Noted- Text has changed
2-101	2	3	5		10	It would be very useful here in the ES to give present day (give year) and pre-industrial (also give specific period) abundances of the big 3 GHG with uncertainties. These are important numbers as they drive the RF calculation later, so put them up front. When putting uncertainties on the mean tropospheric abundances for today consider that surface measurements at remote sites have some representativeness/sampling error. For uncertainties in the pre-I, consider the century scale variability in the recent Holocene noted in Chapter 6. [Michael Prather, United States of America]	Noted -Text has changed
2-102	2	3	6	3	6	Remove reference to "likelihood" or move to end of sentence. It is most odd starting a chapter with reference to uncertainty. It is also odd that the first sentence references likelihood but then lines 7 to 10 do not. [Government of Australia]	We adopted the uncertainty language for atmospheric components
2-103	2	3	6	3	6	It is virtually certain that long-lived greenhouse gases increased from 2005 to 2011'. Why is this not unequivocal like sea level rise (as per statement in SPM on SLR)? [Government of Australia]	Text modified to "certain".
2-104	2	3	6	3	7	To what extent did Kyoto ‘control’ the atmospheric burdens of the GHGs? Perhaps there as an attempt to manage these burdens under the Kyoto accord. [David Sauchyn, Canada]	Modified text.
2-105	2	3	6			Numbers of this increase should be provided for the major GHG [Dietrich Feist, Germany]	Increases now given for major gases.
2-106	2	3	6			"controlled by kyoto protocol"; inappropriate. First, Kyoto doesnt control anything; second, it is not specific just name the gases. [Stephen E Schwartz, United States of America]	Noted -Text has changed
2-107	2	3	7	3	7	We propose that you replace "mole fraction" with "concentrations". Both terms are used many times throughout the chapter when you are describing what we are used to call concentrations (measured in ppm or ppb). By using two different terms when describing what we understand as the same you introduce uncertainty with regard to interpretation and the important message you try to communicate could be lost. We suggest that you do a search throughout the chapter and replace accordingly to be consistent when using these terms. NB! It is also the case in several figure captions. [Government of NORWAY]	Rejected - the correct unit is mole fraction, which is not concentration.
2-108	2	3	7	3	8	It is true that the decadal increases in CH4 (2001-2010, 1991-2000) are similar, but it would seem to me that the corresponding decadal increases in CO2 and N2O are larger in 2001-2010 than in 1991-2000, but perhaps not statistically significant (this needs to be checked rigorously) - perhaps this would be a better way to describe the data. [Government of Australia]	Rates of increase for CO2 and N2O for 2005-2011 were very similar (nearly identical) to the previous decade (1996-2005).
2-109	2	3	8	3	8	Spell out exactly what you mean by "previous decade" (e.g., 1995-2004). [Government of Australia]	Reworded.
2-110	2	3	8	3	9	On the topic of atmospheric CH4, should we mention rates of increase during the period of comment? [Jeffrey Taylor, United States of America]	same as above
2-111	2	3	10	3	10	include a "still" before "small" [Ingeborg Levin, Germany]	Noted- Text has changed
2-112	2	3	10	3	10	HFCs, PFCs, and Sf6 growth rates are only rapid relative to their current atmospheric abundance, suggestion:	Considered - Text now includes it

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						add "relative" to qualify "rapid" [Stephen Montzka, United States of America]	
2-113	2	3	10		10	Consider deleting final sub-sentence 'but their relative contribution to....'. This finding is outside the scope of Chapter 2. [Thomas Stocker/ WGI TSU, Switzerland]	Disagree.
2-114	2	3	12	3	12	The emissions of ozone depleting substances (ODS) are not controlled by the Montreal Protocol - the Montreal Protocol controls production and consumption, it does not control fugitive emissions or emissions from Banks or emission from landfills. The classic example is CCl4. [Government of Australia]	ES text has been reworded
2-115	2	3	12	3	12	Should be "ozone-depleting" (here and presumably elsewhere) [James Renwick, New Zealand]	Change throughout.
2-116	2	3	14	3	15	The burgen of CFC-11, -113, CCl4 and CH3CCl3 decreases already before 2005. Be more specific. [Guus Velders, Netherlands]	Considered - Text has changed
2-117	2	3	15	3	16	Is it the spatial distribution or the mix of species that is changing? [Dian Seidel, United States of America]	Considered - Text has changed
2-118	2	3	15	3	16	Explain what is meant with 'the distribution of the emissions is changing'. [Guus Velders, Netherlands]	Considered - Text has changed
2-119	2	3	18	3	18	We propose "…..water vapor which is an important radiative forcing component" [Government of NORWAY]	Removed the RF statement.
2-120	2	3	18	3	20	This paragraph states that there is high confidence that the net change of stratospheric water vapour during 1992-2011 is small - but it does not say what sort of a change it is? Increase? Decrease? [Government of Australia]	The confidence statement has changed to low.
2-121	2	3	18	4	25	It makes no sense to report a degree of confidence for observational data. Either the data shows what is claimed, or it doesn't. If you insist on stating a degree of confidence then describe why you have done so. [John McLean, Australia]	ES text has been reworded
2-122	2	3	20			summary of water…how do you justiy high confidence that the net change during 1992-2011 is small. You have to combine records to do so, the step like decrease in 2000 is large, as is the increase at the end of the record. Also, there is aerosol contamination in this periods (1992-1995) impacting satellite retrievals. And, how do you define small? (when from a radiative standpoint, .5 ppmv seems to matter). [Karen Rosenlof, United States of America]	ES text has been reworded
2-123	2	3	22	3	22	We propose: "….from approximately 1980 through the mid-1990s has slightly recovered by approximately 2000, and remained…." [Government of NORWAY]	ES text has been reworded
2-124	2	3	22	3	23	Some have argued and published that Antarctic stratospheric ozone is recovering and the recovery is statistically significant. This is not discussed in the body of Chapter 2 but should be. Possible ozone recovery needs to be discussed in the body of Chapter 2 and needs to be brought forward into the Executive Summary [Government of Australia]	The ES now reads: It is certain that global stratospheric ozone has declined from pre-1980 values. Most of the decline occurred prior to the mid 1990s; since then ozone has remained nearly constant at about 3.5% below the 1964-1980 level. decade, about 3.5% below the pre-1980 values.
2-125	2	3	22	3	23	High confidence should part of the text like in the previous paragraphs in order to be clear what has this calification: the decline of the ozone, the partially recovering or the fact that remain constant, or alll of them. [Government of Chile]	the ES statement now reads:The ES now reads: It is certain that global stratospheric ozone has declined from pre-1980 values. Most of the decline occurred prior to the mid 1990s; since then ozone has remained nearly constant at about 3.5% below the 1964-1980 level. decade, about 3.5% below the pre-1980 values.
2-126	2	3	22	3	23	I think the word 'recovery' should not be used here. The WMO(2011) assessment was very cautious with its description of the ozone trend and talked about the different stages of recovery. I suggest the same is done here. [Guus Velders, Netherlands]	the ES statement now reads:The ES now reads: It is certain that global stratospheric ozone has declined from pre-1980 values. Most of the decline occurred prior to the mid 1990s; since then ozone has remained nearly constant at about 3.5% below the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							1964-1980 level. decade, about 3.5% below the pre-1980 values.
2-127	2	3	22			Add "Global" to start of sentence. [Government of Australia]	Done
2-128	2	3	23	3	23	Some jarring precision justaposed with imprecision in language - "remained constant until 2010 at a level 2.5% below". Was it constant for 10 years at 2.5% below to one decimal place as implied ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	the ES statement now reads:The ES now reads: It is certain that global stratospheric ozone has declined from pre-1980 values. Most of the decline occurred prior to the mid 1990s; since then ozone has remained nearly constant at about 3.5% below the 1964-1980 level. decade, about 3.5% below the pre-1980 values.
2-129	2	3	23	3	39	In these statements a different style is used with the confidence level given in parenthesis at the end of each statement. This approach should be avoided, and the uncertainty language integrated directly into the sentence. [Thomas Stocker/ WGI TSU, Switzerland]	Done
2-130	2	3	24	3	25	Important to point out little lower stratospheric temperature change since 1995 (a relatively long time.) [John Christy, United States of America]	Noted. This is discussed in Section 2.4.
2-131	2	3	25	3	25	"indicate tropospheric ozone trends" - odd phrasing. There will always be trends of some sort (maybe near zero). What does this sentence mean ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-132	2	3	25	3	28	Same as before respect to medium confidence. [Government of Chile]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-133	2	3	25	3	28	The main text (page 15 to 16) indicates an overall picture of increasing ozone levels (admitably with considerable variation) this paragraph does not reflect this. [European Union]	The tropospheric ozone summary now accuratley reflects the findings of the main text.
2-134	2	3	25	3	28	This finding on tropospheric ozone trends is confusingly worded; it is difficult to be certain what the finding means without going to the full text later in the report. [Government of United States of America]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-135	2	3	27	3	28	This sentence says that there is only medium confidence that “the trends are mixed”. What would the opposite be - “not mixed”, “uniform”? I should have thought that the confidence was pretty high that the trends over Europe and N. America aren't uniform. [William Collins, United Kingdom of Great Britain & Northern Ireland]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-136	2	3	28	3	28	What is a ``mixed'' trend? [Government of France]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-137	2	3	28	3	28	… trends are "mixed"; better would be … trends are "variable" [Ingeborg Levin, Germany]	The sentence now reads: Confidence is medium in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-138	2	3	30	3	31	We propose to split this paragraph into two sentences, the first sentence could end after; "… gas trends." while the second sentence could be; "NO2 has increased by more than a factor 2 in Asia, while there has been decreases in Europe and North…." [Government of NORWAY]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-139	2	3	30	3	32	Agree- but the appropriate citations describing a global surface decline in CO in AGAGE and NOAA data are not given! [Government of Australia]	In the ES generally no sentences are given. Detailed refereces to NOAA and AGAGE are given in the supplement.
2-140	2	3	30	3	32	Same as previous comment [Government of Chile]	In the ES generally no sentences are given. Detailed refereces to NOAA and AGAGE are given in the supplement.
2-141	2	3	30	3	32	Add the corresponding time period. [Xiaobin Xu, China]	added
2-142	2	3	31			"factor 2" is meaningless (possibly factor OF two?) [Government of Australia]	The sentence now reads: Confidence is medium in large-scale increases of tropospheric ozone across the Northern Hemisphere since the 1970s. Confidence is low in ozone changes across the Southern Hemisphere due to limited measurements. It is likely that surface ozone trends in eastern North America and western Europe since 2000 have levelled off or decreased and that surface ozone strongly increased in East Asia since the 1990s. Satellite and surface observations of ozone precursor gases NOx, CO, and non-methane volatile organic carbons indicate strong regional differences in trends. Most notably NO2 has likely decreased by 30-50% in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							Europe and North America and increased by more than a factor of 2 in Asia since the mid-1990s. [2.2.2.3, 2.2.2.4]
2-143	2	3	33	3	57	I suggest to bring a comparison with the result of AR4 [Rahimzadeh Fatemeh, Iran]	Chapter 2 results expand on the AR4 results, generally where necessary this is done in the main text.
2-144	2	3	34	3	38	Need to explain what the implications for global optical depth are. [Government of Australia]	Due to opposing regional trends and large variability, global trends estimates of AOD are not robust. This is explained better in ES and text.
2-145	2	3	34	3	38	We found constrast trends of mass and optical concetrations from 1992 to 2010 over Europe, America, Canada, and China. The trends of mass concentration of aerosols (PM10) are oposite to those of optical extinction over America, Canada, and China from 1992 to 2010. Reference: Wang, K.C., Dickinson, R.E., Su, L. and Trenberth, K.E., 2012. Contrasting trends of mass and optical properties of aerosols over the Northern Hemisphere from 1992 to 2011. Atmospheric Chemistry and Physics, 12(19): 9387-9398. [Kaicun Wang, China]	We thank the reviewers for this references; we have added a reference to Wang (2009) regarding the use of visibility measurement. We are reluctant to use and discuss the 2012 paper since It heavily depends on urban PM measurements- with sometimes known quality issues, that are likely not representative for the larger scales of relevance for climate change
2-146	2	3	34		38	AOD, positive trend; etc. Try to be quantitative with uncertainties. [Stephen E Schwartz, United States of America]	We choose to have more descriptive formulations of trends- since it is difficult to capture strong regional differences in numbers. The reader is referred to the specifics of the section for more detail
2-147	2	3	35	3	36	We propose to split the long sentence into two sentences, for example: "… in Eastern and southern Asia. Europe and Eastern USA have negative trends during the 1990s and 2000s, ….." [Government of NORWAY]	Sentences are split now.
2-148	2	3	37	3	38	Here high confidence, Does it only apply to the last sentence? (i.e., "with a host of observations 37 showing declining particulate matter air pollution at surface stations in Europe and the USA") [Government of Chile]	We have rephrased the ES statement:It is virtually certain that aerosol column amounts have declined over Europe and North America since 1985 and increased over southern and eastern Asia since 2000. These shifting aerosol regional patterns have been observed by remote sensing of aerosol optical depth (AOD), a measure of total atmospheric aerosol load and in agreement with time-records of visibility in those regions. Declining aerosol loads over Europe and North America are consistent with reported reductions of particulate mass observed at ground-based in-situ monitoring stations. Confidence in satellite based global average AOD trends is low.
2-149	2	3	38	3	38	Suggest adding a sentence similar to this: "AOD trend from early 1980s to early 2000s is slightly negative for the mean of global ocean (Mischenko et al. 2007a; Zhao et al., 2008) but natural for the mean of global ocean and land (Li et al., 2009)". New References: 1) Li, Z., T. X.-P. Zhao, R. Kahn, M. Mishchenko, L. Remer, K.-H. Lee, M. Wang, I. Laszlo, T. Nakajima, and H. Maring, Uncertainties in satellite remote sensing of aerosols and impact on monitoring its long-term trend: a review and perspective, Ann. Geophys., 27, 2755-2770, 2009. 2) Zhao, T. X.-P., I. Laszlo, W. Guo, A. Heidinger, C. Cao, A. Jelenak, D. Tarpley, J. Sullivan, Study of Long-term Trend in Aerosol Optical Thickness Observed from Operational AVHRR Satellite Instrument, J. Geophys. Res., 113, D07201, doi:10.1029/2007JD009061, 2008. [Xuepeng (Tom) Zhao, United States of America]	thank you; the ES statement now becomes more focussed. It is virtually certain that aerosol column amounts have declined over Europe and North America since 1985 and increased over southern and eastern Asia since 2000. These shifting aerosol regional patterns have been observed by remote sensing of aerosol optical depth (AOD), a measure of total atmospheric aerosol load and in agreement with time-records of visibility in those regions. Declining aerosol loads over Europe and North America are consistent with reported reductions of particulate mass observed at ground-based in-situ monitoring stations. Confidence in satellite based global average AOD trends is low.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-150	2	3	38			no reliable trend... -> what does "reliable" mean here? Statistically significant? Of a given minimum magnitude? Please clarify. [Jan Cermak, Germany]	High confidence is attributed to regional trends, and low confidence to global trend. Calibrated uncertainty language is now used.
2-151	2	3	38			replace "reliable" by "statistically significant" [Karen Rosenlof, United States of America]	High confidence is attributed to regional trends, and low confidence to global trend. Calibrated uncertainty language is now used.
2-152	2	3	41	3	41	Please replace "to" with "from the" in the start of the sentence. [Government of NORWAY]	Noted - we do not see a "to" on this line
2-153	2	3	41	3	42	The first sentence is a somewhat cryptic. What variations? [Olivier Boucher, France]	Accepted - the sentence has been revised to be more specific
2-154	2	3	41	3	42	"Satellite records .. radiation budget". Odd formulation in this sentence. How could there not be decadal variations in some quantity ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted - the sentence has been revised to be more specific
2-155	2	3	41		42	"continuation of the decadal variations"; no idea what this phrase is intended to mean. [Stephen E Schwartz, United States of America]	Accepted - the sentence has been revised to be more specific
2-156	2	3	42	3	44	The juxtaposition of these two thoughts, about albedo changes and ENSO is awkward. Some connecting language is needed. [Dian Seidel, United States of America]	Accepted - the sentence has been revised.
2-157	2	3	43	3	44	I would add "within observational uncertainty" after "records". [Norman Loeb, United States of America]	Accepted - "within observational uncertainty" has been added
2-158	2	3	46	3	46	The text here needs to be revised to make it clear that it is the land surface not the ocean surface that is being referred to e.g. by changing 'surface' to 'land surface'. Observations over the ocean are extremely limited so it's not possible to say with confidence what the changes are in ocean surface solar radiation. [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	Accepted - "land-based" has been added to make this point clear
2-159	2	3	46	3	47	Please consider to replace "decadal" with "10 year" and "substantiated" with "confirmed" [Government of NORWAY]	Noted - we prefer to word "decadal" to facilitate the reading. The word "substantiated" is no longer used in the revised executive summary statement.
2-160	2	3	46	3	49	Do these trends occur in Asia also, and how do they relate to the trends in lines 34-38. [Government of Australia]	Taken into account - aerosol optical depth, to which the reviewer refers with lines 34-38, is now also mentioned in the executive summary. Regional differentiation in the trends are discussed in the main text, but not in the executive summary due to space constraints. A trend reversal in surface solar radiation and related variables (DTR, sunshine duration, pan evaporation) is also noted in China, but not as strong as in other parts of the globe. No trend reversal is found in India. The fact that the majority, but not all of the radiation stations show dimming/brightening tendencies is taken into account by extending the statement by "at many land-based observation sites". The trends in AOD mentioned in lines 34 - 38 in the SOD cover only a small part of the period discussed with the surface radiative fluxes, as AOD data mostly started to become available after 2000, while many surface radiation records go back to the 1950s. In the period after 2000, where both measurement types are available, over Europe the declining AOD fits to the increasing surface solar radiation, while over South East Asia, increasing AOD fits to declining surface solar radiation. This is mentioned in the text body, and taken into account now in the revised executive

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							summary.
2-161	2	3	46	3	49	This paragraph on surface solar radiation is really confusing - especially the sentence 'There is medium confidence for increasing downward thermal and net radiation at the surface in recent decades'. Please explain what this means and why it is important in plain English (especially important for an exec summary). [Government of Australia]	Noted - downward thermal radiation at the surface is an important energy flux in the climate change discussion, as it is through this flux that increasing levels of greenhouse gases in the atmosphere affect the temperature and energy content at the surface. Increasing greenhouse gases result in an increased emission of thermal radiation from the atmosphere down to the surface (i.e. in an increased flux of downward thermal radiation), thereby increasing the surface temperature. From theory and climate model scenarios we therefore expect an increase in downward thermal radiation with increasing greenhouse gases. Since the downward thermal radiation is only measured at selected sites since the 1990s, the confidence in the observational evidence for an increase is not more than medium. With increasing downward thermal radiation, also the surface net radiation (the available energy at the surface) is expected to increase. This is also of importance in the context of climate change, as with an increase of surface net radiation, more energy becomes available for evaporation (if not water limited), which globally equals precipitation. An increase in surface net radiation would therefore likely induce an intensification of the global water cycle. As with the downward thermal radiation, measurements are sparse and mostly only starting in the 1990s with high quality, thus the observed increase is of medium confidence.
2-162	2	3	46			The reduction of surface solar radiation before 1980 and the subsequent increase is observed at surface sites mainly located over land. Because satellite observations does not show such trends (or satellite observations are inconsistent with surface observations), partly because of their large uncertainty, this could be considered to be a regional trend. Suggest stating that the confidence is high for regional trend but very low for a global trend. [Government of United States of America]	Taken into account - the statement has been adjusted to emphasize that knowledge on these changes is restricted to the available observation sites over land. This point is also being picked up in the summary paragraph of the radiation section 2.3
2-163	2	3	51	3	53	It would also mention warming in early 20 the century. It's somewhat less certain, but comparable in magnitude to recent warming. It's already mentioned in Chapter 10 (pg 13 line 19) and in Ch 2 pg 33 line 45. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	ES text has been reworded
2-164	2	3	51	4	25	Numbers of the observed temperature increases should be provided wherever possible [Dietrich Feist, Germany]	ES text has been reworded
2-165	2	3	51	4	25	The Figure 2.16, Figure 2.21, FAQ2.1 Figure 2 clearly show that the global average land surface temperature were rising at a slower rate in the period since 1998 than in the period from the 1950s to the end of the 20th century. In our view, although it does not mean that the global climate change mainly characterized with warming has ceased or reversed in trend, yet the above observed fact of wide concern since the AR4 should be elaborated on or explained in a more clear-cut and detailed manner in this paragraph. [Government of China]	ES text has been reworded
2-166	2	3	51		57	Doesn't this side step the past 10 years that denier press like to bring up? You should acknowledge the lack of trend over the past decade, and note that similar periods exist in the historical record, while still in a period of overall increase. [Karen Rosenlof, United States of America]	ES text has been reworded

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-167	2	3	52	3	53	Warming is "virtually certain" rather than "unequivocal" ? Does this signal a change from AR4 or rather is it only when other aspects of climate change are assessed does overall the warming of the climate system become unequivocal ? Where does the virtually certain assessment come from ? 5 percentiles of warming from the datasets shown in table 2.7 are much greater than 0 indicating that the 1 percentiles could well be much greater than zero also. Would it be possible to substitute another phrase here if such existed ? eg is the likelihood i> 99.9 % ? I signal this because of a worry that this virtually certain formulation here could be mis construed. I notice it doesn't appear in the SPM where instead categorical statements are made - "global averaged near surface temperatures have increased" without assigning a likelihood. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	ES text has been reworded.The word certain is used
2-168	2	3	52	3	57	"Globally averaged near surface temperature" What nonsnse! Nobody has ever measured such a quantity. It would require simultaneous measurements of thermometers situated randomly over the entire earth's surface, including the oceans. You are surely referring to the so-called "Annual Global Surface Temperature Anomaly" which is not a temperature at all. but a complex multi-average of a large number of unrepresentative non-standard weather station maximum an minimum temperature measurements. This concoction bears only a very slight resemblence to any genuine global mean suface temperature [Vincent Gray, New Zealand]	ES text has been reworded
2-169	2	3	52	3	57	This summary ignores the elephant in the room, the absence of warming since 1997. (The trend in HadCRUT3 data from Jan 1997 to Jul 2012 is flat.) Please show some integrity and mention this or the IPCC will be a laughing stock. [John McLean, Australia]	ES text has been reworded
2-170	2	3	52	3	57	The use of 0.8C increase and 0.66C+-0.06 increase in this short passage is confusing. It might help to change line 55 to difference in mean tempertaure for two 30-yr periods is 0.66C so be specific. [Xuebin Zhang, Canada]	Noted. Text has changed
2-171	2	3	53	3	55	Is it possible to say something about how this increase since 1901 is related to the increase sine pre-industrial times? We also assume that you will present as updated temperature observations as possible when AR5 is finalized. [Government of NORWAY]	Data has been updated.
2-172	2	3	54	3	54	The trend of temperature was estimated for exactly 100 years in IPCCAR4. However, 110 years (1901-2010) is used in this report. It is suggested also to use the 100 years (1913-2012) for comparison. [Qingxiang Li, China]	Thanks, several time periods were used.
2-173	2	3	54	3	54	I cannot find how the 0.8 ºC rise has been estimated in the text of Chapter 2, it is not in section 2.4.3 [Geert Jan van Oldenborgh, Netherlands]	ES text has been reworded
2-174	2	3	54	3	54	The trend of temperature was estimated for exact 100 years in IPCCAR4. However,110 years (1901-2010) was used in this report. It is suggested also use the 100 years (1913-2012) for comparison. [Shaowu Wang, Beijing]	Thanks, several time periods were used.
2-175	2	3	54			"about"; better to be specific with uncertainty and specify the meaning of the uncertainty, eg5-95% conf range as in line 56 [Stephen E Schwartz, United States of America]	ES text has been reworded
2-176	2	3	55	3	56	The modelling chapters have taken 1850-1900 as the early-industrial reference period as 1886-1905 could be depressed after the eruptions of Krakatau, Santa Maria and other volcanoes. This gives a central value of 0.60 ºC for the difference between early-industrial and 1986-2005, can you provide an error estimate on this? [Geert Jan van Oldenborgh, Netherlands]	ES text has been reworded
2-177	2	3	55	3	57	Could you add an estimate for the temperature change from pre-industrial? (if this is impossible, please explain and provide advice on how to deal with pre-industrial as a reference for temperature - is this simply acceptable as a replacement for "pre-industrial"?) [Philippe Marbaix, Belgium]	ES text has been reworded
2-178	2	3	55			described -> "approximated" [Jan Cermak, Germany]	ES text has been reworded
2-179	2	3	55			Is 1886-1905 well described as "early industrial"? Industrialization has taken place at different times in different parts of the world. For the UK the industrial resolution started more than a century before 1886-1905, so this 20-year period would be better characterised as mid-industrial not early-industrial. [Adrian Simmons, United Kingdom]	ES text has been reworded

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-180	2	3	56	3	57	"5 to 95% confidence interval" is mis-worded. Does ±0.06°C describe the 95% confidence interval or the inner 90th percentile (5-95%)? [Dale Hurst, United States of America]	ES text has been reworded
2-181	2	3	56	3	57	Is this statement of the confidence interval standard? I seems this is really a 90% confidence interval, based on a two-tailed test. [Dian Seidel, United States of America]	ES text has been reworded
2-182	2	3	57	3	57	We feel that it is important that you also include information about the rate of change per decade and how this has changed. [Government of NORWAY]	Accepted. Text has been changed with this suggestion
2-183	2	3				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 Thomason, United States of America]	Accepted.
2-184	2	3				I think that this chapter is very well written and very clear. However, it is highly dependent on papers that are submitted (even at this late date) which 1) I cannot see and 2) may NEVER be publisdhed. These must be removed prior to final accveptance if they remain in publication limbo and any weight given to their findings expunged. [Larry Thomason, United States of America]	Noted. Publication and acceptance of references has been carefully checked
2-185	2	4	1	4	1	Please include information about to what extent the global temperature in the last decades have been the warmest since observations started. E.g. a simplified description of the results described at page 33 line 44 - page 34 line 2. [Government of NORWAY]	Accepted. Text has been changed with this suggestion
2-186	2	4	2	4	3	It is important that AR5 editors correct all unqualified statements regarding the global status of any parameter. To avoid confusion, it is essential to use a qualifier such as "globally averaged", as properly applied in this sentence: "It is virtually certain that globally averaged land surface air temperatures have risen since the late 19th Century." [Forrest Mims, United States of America]	Taken into account
2-187	2	4	2	4	4	The rise in global SAT should be quantified in this statement, especially given the fact that the next statement goes on to say that the influence of UHI effects and land use change has not been more than 10% "of the observed trend". This trend should be quantified. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. Text has been changed
2-188	2	4	2	5	47	These are merely the opinions of biased "experts". They are not based on scientific studies.involvong comparisons wiyj actual future behaviour [Vincent Gray, New Zealand]	The assessment was done based on cited references
2-189	2	4	2		25	Try to be quantititative with uncertainties. [Stephen E Schwartz, United States of America]	The IPCC uncertainty guide was used.
2-190	2	4	6	4	6	We propose that you consider to include an explanation of urban heat island, something like: "An urban heat island is a metropolitan area which is significally warmer than its surrounding rural areas. It is likely that this and land use change effects have not raised the …" [Government of NORWAY]	It is explained in the section.
2-191	2	4	6	4	6	There are very few specific, numerical estimates in the Chapter summaries and the SPM itself. So those that do get included really should be the most solid and relevant ones. The claim that unresolved non-climatic biases in land surface temperature data amounts to no more than 10% of the global trend is a guess. Expressing it numerically gives it the appearance of having a scientific foundation, but it is conjured from thin air in the text, it is not an estimate derived from actual data analysis. It is a guess coming at the end of a review of the underlying literature that, among other things, concedes that a previous IPCC report had no supporting evidence for its dismissive assertions on the topic. What you now say in the text is that the issue is disputed and the conflicting lines of evidence have not yet been resolved. That is what the summary should say. You should not just go around making up numbers. [Ross McKitrick, Canada]	ES text has been reworded
2-192	2	4	6	4	7	"land surface air temperature ... 10% of observed trend": IIt is a bit frustrating to have numbers for one variable (trend over land+ocean, paragraph above), and a discussion of possible errors for a different variable (trend over land only). You may perhaps add a trend over land in the last sentence? In addition, it may be useful to clarify that the trend is given without (or with!) an attempt at removing this heat-island effect. [Philippe Marbaix, Belgium]	ES text has been reworded
2-193	2	4	6	4	9	These ES paragraphs are taken from the text. The only one I take issue with is this one. This should be 'very likely'. The only paper that has looked at large scales that says the issue is large is that by McKitrick. This uses circulation data at an annual timescale and the same socioeconomic data for every grid box across	ES text has been reworded

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Russia, and similarly across other large countries. ERA-Interim shows a very similar warming to CRUTEM4 across the NH land areas. How can there be a large urban effect and these two datasets agree so well? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	
2-194	2	4	7	4	7	The following wording is suggested to add clarity: This is a global average value; [Klaus Radunsky, Austria]	ES text has been reworded
2-195	2	4	7	4	9	Sentence is not clear. It needs modification to make it meaningful. [Umesh Kulshrestha, India]	ES text has been reworded
2-196	2	4	11	4	11	We propose: "daily" instead of "diurnal". Rationale: It is important that the executive summary is easy to understand. [Government of NORWAY]	Diurnal is commonly used.
2-197	2	4	11	4	13	This comment (and in the accompanyting text) is misleading because the work I have done indeed accounted for the removal of inhomogeneities. The relieance on the NCDC dataset is dangerous because the spurious gradual warming found in many stations is not removed, it is rather smeared throughout the NCDC result, likely contaminating the entire dataset. [John Christy, United States of America]	Noted. We have read and cited the work of the reviewer in the main text. This is one piece of several lines of evidence that have gone into forming this conclusion. The findings do not rely on the NCDC dataset. The findings rely on an improved understanding of the issues in the raw record found by many independent groups as detailed in the text.
2-198	2	4	11	4	13	This has strong regional differences. In southeast Australia, the diurnal range may have increased. [Government of Australia]	Taken into account
2-199	2	4	11	4	25	The absence of numbers in these statements is notable to the reader. Please quantify trends wherever possible. [Thomas Stocker/ WGI TSU, Switzerland]	Text has been rewritten
2-200	2	4	19	4	22	This conclusion is highly misleading. It would be accurate to add regarding the "encompass" comment, "which is inconsistent with model projections which show significant amplification (not agreement) of the tropospheric trend relative to the surface, especially in the tropics." To avoid mentioning models, the additional statement could be, "in other words, there is no amplification of the bulk tropospheric trends relative to the surface, especially in the tropics." This finding is important for model evaluation. [John Christy, United States of America]	Noted. The chapter is about observations and not about models or theoretical expectations. Further chapters discuss these issues in detail and contain their own findings in this regard in their summaries.
2-201	2	4	19	4	22	The wording here seems contradictory: virtually certain that there has been warming but only medium to low confidence about the rate of change and its vertical structure. Presumably this means that we know it has warmed, but we don't know by exactly how much. Might it be better to reverse the order and say "although there is only medium to low confidence about the rate of change, it is virtually certain that it is positive" (or somesuch) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	ES text has been reworded
2-202	2	4	20	4	21	How is it consistent to have low to medium confidence in rate of change yet be virtually certain it is warming ? What aspects of rate of change are being referred to with this confidence statement ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	ES text has been reworded
2-203	2	4	21	4	22	Sloppy language. Do you mean that one 90% confidence interval for surface warming is within the 90% confidence interval for tropospheric warming? [Olivier Boucher, France]	ES text has been reworded
2-204	2	4	21	4	22	Unclear. Do you mean that surface data are included, or surface trend values are within the range of upper-air values? [Dian Seidel, United States of America]	ES text has been reworded
2-205	2	4	24	4	25	How is it possible to have low confidence in the cooling rate yet be virtually certain it is cooling ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	ES text has been reworded
2-206	2	4	25	4	25	I assume "whole stratosphere" refers to all vertical layers, not all lat/lon areas, but that is not clear from the text. [Dian Seidel, United States of America]	ES text has been reworded
2-207	2	4	25			It is stated that there is low confidence in the cooling rate for the lower stratosphere. This statement is problematic for two reasons. Firstly, a look at the upper panel of 2.24 indeed shows some variability among estimates, but all agree on the basic qualitative features, and they to my mind make talk of "the cooling rate" as almost irrelevant. The net cooling is nowhere near being represented by a uniform cooling rate over the period since 1979. This is not simply due to the direct effects of El Chichon and Pinatubo: there is no evidence	ES text has been reworded

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of cooling since about 1995. A comment on this rather than on low confidence might be more appropriate. [Adrian Simmons, United Kingdom]	
2-208	2	4	25			Secondly, even if the concept of a cooling rate made sense, a statement about "low confidence" in the cooling rate is not really sound without some quantitarive statement about the size of the cooling rate. For example, again referring to the upper panel of Fig. 2.24, one could argue that there is high confidence that the cooling rate is at least w and high confidence that the cooling rate is no larger than x, where w and x are the minumum and maximum cooling rates among the estimates shown. One could say also that there is low confidence in the accuracy of the median cooling rate y because of spread z among the estimates shown. [Adrian Simmons, United Kingdom]	ES text has been reworded
2-209	2	4	27	4	27	Please consider to replace "Hydrological Cycle" with "Water Cycle" [Government of NORWAY]	rejected
2-210	2	4	28	4	29	".. over land is low prior to 1950 and medium afterwards because of data incompleteness" should be ".. over land is low because of data incompleteness particularly prior to 1950", because confidence in global precipitation change after 1950 is not evaluated in the body text (2.5). [Government of Japan]	ES text has been reworded
2-211	2	4	28	4	47	These statements about unchanged precipitation are in conflict with claims that sea level recently fell because of increased precipitation over land. [John McLean, Australia]	ES text has been reworded
2-212	2	4	28		32	Can this be phrased more assertively? Change in precip is less than ... rather than little confidence in change. [Stephen E Schwartz, United States of America]	ES text has been reworded
2-213	2	4	29	4	29	Be clear what is meant by "incompleteness". This can have a range of meanings. [Government of Australia]	ES text has been reworded
2-214	2	4	29	4	30	Was there no change or was this a problem of data in-filling? [Xuebin Zhang, Canada]	ES text has been reworded
2-215	2	4	30	4	31	Please check that this is substantially different from the AR4. In AR4 ch 3, it is written that there is no statistically significant trend over the 20th century. Is the new finding different? (it seems that there is a revision of the assessment for the first half of the century, but not for the trend over the century, as stated in this AR5 draft) [Philippe Marbaix, Belgium]	ES text has been reworded
2-216	2	4	30			land-based precipitation -> "precipitation over land" [Jan Cermak, Germany]	accepted, text changed
2-217	2	4	30			This statement: '...little change in land-based precipitation since 1900. This is different from AR4...' seems to contradict the statement in the following comment. [Michael Coffey, United States of America]	ES text has been reworded
2-218	2	4	31			"has" should be "had" [Adrian Simmons, United Kingdom]	ES text has been reworded
2-219	2	4	34	4	34	Spell out NH: northern hemisphere [European Union]	accepted
2-220	2	4	34	4	35	change to "from 1900 to 2010" or "over the period 1900-2010" [Olivier Boucher, France]	ES text has been reworded
2-221	2	4	34	4	35	Confidence is low in the mid-latitude and high-latitude NH precipitation trends due to the uncertainties in the data before 1950. It would therefore be much more informative to make a statement about the trend since 1950, to which a higher confidence level can be assigned. [Geert Jan van Oldenborgh, Netherlands]	accepted, text changed
2-222	2	4	37	4	37	last decade (spell this out in full - e.g., 2002-2011) [Government of Australia]	ES text has been reworded
2-223	2	4	37	4	37	Spell out SH [European Union]	accepted, text changed
2-224	2	4	37	4	37	"reversing the drying trend …". I was under impression that the discussion is more about long-term trend. I wonder why such decadal changes are discussed here. [Xuebin Zhang, Canada]	Text has been rewritten
2-225	2	4	40	4	41	Note that snowfall events and snowfall are two different things. Are the two phases used here by purpose? [Xuebin Zhang, Canada]	Noted. Paragraph deleted.
2-226	2	4	40	4	43	Perhaps there is more information on the number of snowfall events than on the ratio of annual snowfall to annual precipitation and the equivalent seasonal ratios. The latter are much more interesting and intrinsically much more likely to have changed if the temperature has increased, yet they are scarcely mentioned in the chapter. [J. Graham Cogley, Canada]	Noted. Paragraph deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-227	2	4	40	4	43	Spatial variation, which is not completely accounted for, should be deleted. [Qingxiang Li, China]	Noted. Paragraph deleted.
2-228	2	4	41	4	42	This statement regarding increasing snowfall in Antarctica appears to contradict a statement given in the ES of Chapter 4 (page 4, line 31). Chapter 4 states that during the period 1979-2011 there is no change in total snowfall in Antarctica. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Paragraph deleted.
2-229	2	4	41	4	43	Two sentences seem contradictory - "Antarctica is the exception" sentence seems to contradict "Confience is low for changes in snowfall over Antarctica" sentence. I think like earlier statements you are talking about confidence in some aspect of the magnitude over and above whether or not it is increasing. [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted. Paragraph deleted.
2-230	2	4	42	4	43	Need to be clear what sort of "Antarctic changes" in snowfall. The Confidence relates to long term changes only. [Government of Australia]	Noted. Paragraph deleted.
2-231	2	4	45	4	47	Average runoff has not changed in the majority of rivers, but year-to-year variability has increased. The evidences or references are needed to specify or support this conclusion in text. [Jianting Cao, China]	Noted. Paragraph deleted.
2-232	2	4	45	4	47	This statement appears to be based upon two studies (Millman et al., 2008; Dai et al., 2009; see section 2.5.2 on page 2-43; lines 17-8). The evidence from other reputed published work needs to be accounted for prior to making an emphatic claim. The study by Dai et al (2009) is based upon analysis at most downstream drainage points of major river systems, which is impacted greatly by anthropogenic disturbances. The claim in that study that direct human influence on annual stream flow is likely small compared with climate forcing during 1948-2004 is certainly not true in Australian context. [Government of Australia]	Noted. Paragraph deleted.
2-233	2	4	45	4	47	"year to year variability of run-off has increased." Is this consistent with rainfall variability? [Government of Australia]	Noted. Paragraph deleted.
2-234	2	4	45	4	47	Strong statement, but no indication on uncertainty/ confidence. This is expected, as this differs from AR4 assessment. [Government of France]	Noted. Paragraph deleted.
2-235	2	4	45	4	47	Given the change from AR4 to AR5, a statement of confidence seems in order but is not given. [Dian Seidel, United States of America]	Noted. Paragraph deleted.
2-236	2	4	46	4	47	I could not find the statement "global runoff increased during the 20th century" in the AR4. It is very important to avoid erroneous quotations, including to previous IPCC reports. Please check that your text accurately cites AR4 (see especially AR4 3.3.6 and TS) [Philippe Marbaix, Belgium]	Noted. Paragraph deleted.
2-237	2	4	47	4	47	Certainty level for the statement "but year-to-year variability has increased"? [European Union]	Noted. Paragraph deleted.
2-238	2	4	47			This statement of increased variability does not appear to be supported by the expanded discussion later in the chapter (Section 2.5.2). [Government of United States of America]	Noted. Paragraph deleted.
2-239	2	4	49	4	49	Change "absolute moistening" to "absolute humidity" or "specific humidity". [Government of Australia]	Noted. Text has changed
2-240	2	4	49	4	50	The statement that absolute himidity of the atmosphere near teh surface has incrased is based on one dataset with large holes in it that ends in 2003. A longer dataset that runs until 2011 with better coverage does not show a trend (ERA-interim). This should reduce the confidence to medium at most. [Geert Jan van Oldenborgh, Netherlands]	There exist at least three datasets up to 2003.
2-241	2	4	49	4	56	This paragraph is quite confusing for those who are not experts in the field because it uses five different terms to describe the humidity in the atmopshere: absolute moistering, relative humidity, troposheric water vapour, troposhperic specific humidity, troposheric relative humidity. It is suggested to keep the number of scientific terms (that should be included and explained in a glossary) to the minimum. It is noted that the SPM uses the term "trospheric specific humidity" only. [Klaus Radunsky, Austria]	Noted. Text has changed
2-242	2	4	50	4	50	We propose: "However, during recent years this has declined over land, …" [Government of NORWAY]	editorial
2-243	2	4	50	4	52	What does "recent" mean here? [Olivier Boucher, France]	We use "recent" compared to 1970.
2-244	2	4	52	4	53	With the exception of arctic and alpine sites, tropospheric water vapor constitutes most of the total water vapor	Taken into account. See response to 2-245

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						column. Therefore, the following sentence is contradicted by the newly expanded and recently published NVAP-M study, which shows no trend in global column water vapor measured over land and oceans by radiosondes, GPS and a fleet of satellites: "Radiosonde, GPS and satellite observations indicate increases in tropospheric water vapour at continental scales,…" New language is proposed in the next row below. [Forrest Mims, United States of America]	
2-245	2	4	52	4	53	The contradiction in the previous row above can be rectified by following the sentence above with the following proposed language: "These studies suggest that it is likely that tropospheric specific humidity has increased since the 1970s. However, the recently reanalyzed and expanded NVAP-M study found no trend in the global abundance of total column water vapor from 1989 to 2010." Citation: Thomas H. Vonder Haar, Janice L. Bytheway and John M. Forsythe. Weather and climate analyses using improved global water vapor observations. GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L15802, 6 PP., 2012. doi:10.1029/2012GL052094. [Forrest Mims, United States of America]	Rejected. Vonder Haar et al. explicitly state that their dataset is not suitable for trend analysis."The results of Figures 1 and 4 have not been subjected to detailed global or regional trend analyses, which will be a topic for a forthcoming paper. Such analyses must account for the changes in satellite sampling discussed in the auxiliary material. Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data."
2-246	2	4	54	4	56	In view of the improved 2012 NVAP-M findings (see citation in previous row above), the following sentence should be deleted or qualified to reflect the uncertainty reflected in NVAP-M: "Because tropospheric temperatures have also increased, significant trends in tropospheric relative humidity have not been observed." [Forrest Mims, United States of America]	Taken into account. See response to 2-245
2-247	2	4	55	4	56	This statement is not correct. Near-surface atmospheric relative humidity over land has shown a recent decline. See section 2.5.5. There is also evidence that this drying extends through the boundary layer. You may wish to assign low confidence to this, but the evidence should not be simply ignored. Several later comments relate to this point. Please also see chapter 12 (page 12-4, lines 41-45 for example) for evidence from modelling. [Adrian Simmons, United Kingdom]	Noted. Text has changed
2-248	2	5	6	5	6	Please consider adding the full name of SREX here. [Sai Ming Lee, Hong Kong, China]	accepted
2-249	2	5	6	5	6	This sentence should be a conclusion or comment about observations, not the leading statement of the paragraph. [John McLean, Australia]	accepted
2-250	2	5	6	5	7	This statement is inconsistent with the plotted time series of HadEX2, HadGHCND and GHCNDEX data sets in Figure 2.32 on page 177. The statement seems to suggest (or could be mistakenly interpreted) that the decrease in the overall number of cold days and nights and the increase in warm days and nights began in 1951. A visual inspection of the annual time series anomalies data in Figure 2.32 on page 177 suggests otherwise. The TN10p and TX10p annual time series anomalies initially seem to fluctuate randomly about some constant mean until the mid-1970s after which the two series abruptly exhibit decreasing linear trends. On the other hand, the TN90p and TX90p annual time series anomalies initially exhibit slight decreasing linear trends until the mid-1970s after which they both exhibit increasing linear trends. [Reynold Stone, Trinidad and Tobago]	REJECTED. Time periods and methods for trend calculation were chosen according to the standard periods outlined in Box 2.2. Within the text we highlight that the trends are applicable over the period since 1951 and 1979 to 2010 and therefore the statement holds.
2-251	2	5	6	5	7	In other words, the significant changes exhibited by these four indices began in the mid-1970s and not in 1951 as the statement seems to suggest. This observation was previously reported by Alexander et al. (2006, pages 7 & 8) who, in referring to these four indices, stated: “Changes in all these percentile-based indices seem to have occurred around the mid 1970s which corresponds with changes in mean global temperature [Folland et al., 2001].” [Reynold Stone, Trinidad and Tobago]	REJECTED: See response 2-251. In addition the Alexander et al. 2006 paper referred specifically to the HadEX dataset while our assement here is based on many datasets.
2-252	2	5	6	5	7	Thus, it is more accurate to state the following. “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 between the mid-1970s and 2010”. [Reynold Stone, Trinidad and Tobago]	REJECTED. See response 2-251.
2-253	2	5	6	5	7	It would therefore appear that the use of unsegmented linear regression models to describe the patterns observed in these four indices over the entire 1951-2010 period is inappropriate. Discontinuous segmented (piecewise) regression models are more appropriate to capture the observed structural changes. [Reynold Stone, Trinidad and Tobago]	REJECTED. See response 2-251.
2-254	2	5	7			"cold days and nights" doesnt sound very extreme; qualify: 2 sd colder or something like that. [Stephen E	REJECTED: These are standard terms used within

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Schwartz, United States of America]	the scientific literature as outlined in Box 2.4, Table 1.
2-255	2	5	8	5	9	Another specific example of where rounding would make sense. Given the range, would it not be sufficient to talk of warming trends between 2.5 ± 0.6 and 5.8 ± 1.3 days per decade? [Adrian Simmons, United Kingdom]	TAKEN INTO ACCOUNT: These values are no longer referred to in the ES.
2-256	2	5	9	5	9	… dependent on idex). Which index ? [Ingeborg Levin, Germany]	TAKEN INTO ACCOUNT: This sentence has been removed.
2-257	2	5	12	5	12	The statement about consistency with AR4 is secondary to reporting the observations so move these words to the end of the sentence. [John McLean, Australia]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-258	2	5	12	5	13	"… that there have been more statistically significant regional increases than decreases in the number of heavy precipitation events since the 1950s." [J. Graham Cogley, Canada]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-259	2	5	12	5	14	95% percentile of what? Daily precipitation? 1-hour precipitation? Etc [Olivier Boucher, France]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-260	2	5	12	5	14	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: Based on the body of peer-reviewed scientific literature assessed, this statement holds.
2-261	2	5	12			Consistent with AR4 conclusions, it is likely that the number of heavy precipitation events (e.g., 95th13 percentile) has increased significantly' heavy precipitation is still precipitation. [Michael Coffey, United States of America]	Noted.
2-262	2	5	13	5	14	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. In particular, the climate of Mexico is quite different from that of Canada. [Rubén D Piacentini, Argentina]	TAKEN INTO ACCOUNT: The reference to North America also includes Mexico and parts of Canada.
2-263	2	5	14	5	14	I believe "North America" here is really US. [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: The reference to North America also includes Mexico and parts of Canada.
2-264	2	5	16	5	16	"Consistent trends …", consistent with what? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-265	2	5	16	5	17	I was so glad with the formulation in the FOD which said: "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." The current sentence doesn't fit the state of the science: there is medium or high confidence that there is no trend in floods. So please change this accordingly [Marcel Crok, The Netherlands]	REJECTED: This summaries here refer to the extreme events section which does not cover streamflow.
2-266	2	5	16	5	17	Probably not surprising that nothing can be said for floods on a global scale. Can anything be said on a regional scale or for specific key catchments? [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-267	2	5	16	5	22	More care may be needed with language in these two findings. The first refers to "insufficient evidence," but the body of the report refers to insufficient data, which is a different thing entirely. Similarly, the finding on trends in hydrologial droughts speaks of "geographical inconsistencies in the trends" - it is not clear what a geographical inconsistency is, and the body of the report seems to be talking about the fact that trends differ from region to region. The finding and the body of the text need to say the same thing, and say it with clarity and precision. [Government of United States of America]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-268	2	5	19	5	19	Hydrological in "hydrological drought" should be deleted. [Qingxiang Li, China]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-269	2	5	19	5	19	Again report the observations and leave any comparison to AR4 to much later (if at all) [John McLean, Australia]	TAKEN INTO ACCOUNT: These summary statements have been amended.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-270	2	5	19	5	22	See my comments on page 57 - suddenly we're talking about 'dryness' - the conclusions are about drought (not the same as dryness!) [European Union]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-271	2	5	19		20	"AR4 conclusions no longer supported." State the finding first and then say that this contradicts finding in AR4. [Stephen E Schwartz, United States of America]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-272	2	5	21	5	21	It says lack of rainfall. Does it intend to exclude snow? If not, I would suggest using precipation. [Qi Tang, United States]	TAKEN INTO ACCOUNT: In this context it mostly refers to lack of rainfall.
2-273	2	5	24	5	24	Again report the observations and leave any comparison to AR4 to much later (if at all) [John McLean, Australia]	TAKEN INTO ACCOUNT: This sentence has been removed.
2-274	2	5	24	5	28	The summary in the chapter is much clearer. It says on page 57, line 35 : "In summary, current datasets indicate no significant observed trends in global tropical cyclone frequency and it remains uncertain whether any reported long-term increases in tropical cyclone frequency are robust, after accounting for past changes in observing capabilities." [Marcel Crok, The Netherlands]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-275	2	5	24	5	28	The new conclusion regarding tropical cyclone intensity, does it apply to global average? Some recent regional assessment continues to show that the number of high-intensity cyclones tends to increase, although the total number may be decreasing. Thus, it might be useful to allow such a regional stratification. [Government of Australia]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-276	2	5	24	5	28	The main text concludes the trends since the 1970s are robust but the interpretation of longer term trends are constrained by data quality concerns. This summary needs to reflect that. [European Union]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-277	2	5	24	5	28	We have constructed a homogeneous record of cyclone activity since 1923 based on instrumental data. This is basically a measure of atlantic tropical cyclone activity based on tide gauge data. In this we see a significant rising trend. Please add this paper to the discussion in this paragraph. Aslak Grinsted, John C. Moore, and Svetlana Jevrejeva (2012), Homogeneous record of Atlantic hurricane surge threat since 1923, PNAS, doi:10.1073/pnas.1209542109 [Aslak Grinsted, Denmark]	TAKEN INTO ACCOUNT: This paper has been assessed by the authors.
2-278	2	5	24	5	28	The issue of intense tropical cyclones is important. In Chapter 14, it seems unclear that the assessment changed since AR4 as much as written here. Please check that you have the full picture and appropriate balance. In ch 14, there is at least one reference to a study that identifies a trend in the strongest storms, and it is post-AR4 (Elsner et al. (2008)). You may also separate the reference to destructiveness "off all storms" as this may confuse matters - the topic here is whether an increase in the strength of the strongest cyclones was observed. [Philippe Marbaix, Belgium]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-279	2	5	24		28	Exterme events: make clearer the distinction between the points made in sentence 1 and sentence 3 of this paragraph so that it doesn't appear to contradict itself. Do both parts of the first sentence pertain to all storms, and sentence 3 to only those in the Atlantic? [Stephen Montzka, United States of America]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-280	2	5	27	5	27	Insert comma after 'However' [Peter Burt, United Kingdom]	EDITORIAL
2-281	2	5	27	5	28	This level of detail is not something for the summary. It's also cherry picking. We know activity was low in the late seventies and early eighties. However over the whole century there was no trend. [Marcel Crok, The Netherlands]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-282	2	5	27	5	28	This final sentence concerning an increase in the intensity of the strongest storms is an important one (going beyond the SREX SPM), and should have a confidence level attached to it. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-283	2	5	30	5	31	It would be helpful to explain the reason why there is insufficient evidence. [Klaus Radunsky, Austria]	TAKEN INTO ACCOUNT: These summary statements have been amended.
2-284	2	5	36	5	38	We propose to split this very long sentence into two to improve the readability: "Confidence is high that some trend features, that appeared from the 1950s or earlier to the1990s reported in AR4, have been largely offset	Considered.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						by more recent changes. For instance an increase in the mid-latitude westerly winds and the NAO index, or a weakening of the Pacific Walker circulation". [Government of NORWAY]	
2-285	2	5	40	5	40	Figure 2.39 and references therein would support a likelihood higher than "likely". [Government of Australia]	Rejected. Evidence supports only a "likely" change.
2-286	2	5	40	5	40	Please explain what 'zonal mean sense' means in plain English - especially important for an exec summary. [Government of Australia]	Accepted. Sentence changed
2-287	2	5	40	5	42	"Likely" is used here. In light of strong evidence, the strength of this word is low. Is it because models tend to simulate a smaller change then the observed, and that might indicate a role by decadal variability? [Government of Australia]	Rejected. This Chapter does not address consistency with models.
2-288	2	5	40	5	42	We propose to split this very long sentence into two to improve the readability: "Nevertheless, it is likely that large-scale atmospheric wind systems have moved towards the poles since the 1970s. Examples are widening of the tropical belt, poleward shift of storm tracks and jet streams and contraction of the polar vortex." Please consider to give a layman description of tropical belt, storm tracks, jet streams and polar vortex in footnotes. [Government of NORWAY]	Accepted. Sentence changed.
2-289	2	5	40		42	Has the change mentioned occurred in a statistically significant sense, robust acorss multiple estimates, and in both hemispheres? [Karen Rosenlof, United States of America]	Noted. The change is robust across multiple estimates and is found to be statistically significant in most of the studies. Although many studies find it in both hemispheres in all seasons, it is not equally strong in all seasons/hemispheres. "Likely" reflects this.
2-290	2	5	44	5	47	The last finding (on uncertainty) has unnecessarily complicated and odd phrasing, and it obscures the meaning. [Government of United States of America]	Noted but sentence is removed,
2-291	2	5	44	5	47	Add a sub-heading to show that this paragraph is a general statement, not just about Atmospheric Circulation and Indices of Variability. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted but sentence is removed,
2-292	2	5	44	5	47	It is suggested to split this paragraph into two sentences and stop after "during recent years". The second sentence might read as follows: However, new and improved ways of characterizing and quantifying uncertainty could further improve long-term data records to even better assess climate change in many regions of the world. [Klaus Radunsky, Austria]	Noted but sentence is removed,
2-293	2	5	44	5	47	In the discussion of recent advances in the areas of uncertainty quantification, shouldn't the impact on trend detection be mentioned? [Jeffrey Taylor, United States of America]	Noted but sentence is removed,
2-294	2	5	46	5	47	The phrase "… long-term and climate quality data records…" is not clear. Perhaps, it should be "… long-term, high-quality climate data records…" [Reynold Stone, Trinidad and Tobago]	Noted but sentence is removed,
2-295	2	6	3			"observations" specify of what: met variables, atmos composition, etc. Then no surprise when at line 43 "also"; you will already have told the reader that. Specify the scales up here; again obviating the "besides" and "also" at line 48. Note the extremes here; no also in line 55. Etc. Get your subject clearly stated at the top, so that the enumeration of topics doesnt come across as a series of afterthoughts.. [Stephen E Schwartz, United States of America]	noted
2-296	2	6	4	6	6	when defining why the ocean has thermal inertia the point about the connection between the surface and deep water is not clear. Re-word for clarity that the ocean circulation carries the warm water from the surface to the deeper waters where it is held. [Government of United States of America]	comment missplaced
2-297	2	6	5	6	5	Reference is made here to the IPCC AR5 uncertainty guidance - please state which section/chapter this is in to allow readers to find it easily. [Government of Australia]	noted reference is complete now
2-298	2	6	5			...following the IPCC AR5 uncertainty guidance...' was this meant to be the AR4 uncertainty guidance, if not we should refer to the AR5 document differently since it is this document [Michael Coffey, United States of America]	noted. Reference is complete now
2-299	2	6	7	6	7	Saying that climate varies over all spatial and temporal scales conflicts with the statistical/averaged nature of	Noted. Text has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						climate. If we define climate as a 30 year average mean, for instance, this filters out all fast time scales. The definition of the "climate" would deserve an assessment. Statistics of extreme events, for instance, require very long time series to give a proper meaning to the term ``extreme". Therefore, only slow time variations of such statistics are meaningful. On the other hand, it seems perfectly reasonable to say that the study of climate will focus on the statistics of phenomena occurring on all time and regional time scales. For instance, flash floods are extreme events that are usually spatially very localised and short in duration, but that is not the same as saying that climate occurs on all time and spatial scales. [Government of France]	
2-300	2	6	7	6	8	Unclear: how can climate vary over diurnal cycles? Consider "…climate encapsulates a variety of space and time-scales…" [Richard Allan, United Kingdom]	Noted. Text has changed
2-301	2	6	7	6	19	This paragraph has improved with respect to the corresponding one in FOD since a definition of what is considered climate change has been included: 'Climate change is considered to be statistically significant variations in either the mean state of the climate or in its variability, persisting for an extended period of time'. However, this definition may lead to confusion, and still gives to the expressions "climate variability" and "climate change" the same meaning but for a question of time scale. This confusion between 'climate variability' and 'climate change' is also present in the glossary (AIII), in which we have the following definitions: ´Climate change refers to a change in the state of the climate that can be identified by changes in the mean and/or the variability of its properties and that persists for an extended period, typically decades or longer. Climate change may be due to natural internal processes or external forcings such as modulations of the solar cycles, volcanic eruptions and persistent anthropogenic changes in the composition of the atmosphere or in land use.´ ´Climate variability refers to variations in the mean state and other statistics (such as standard deviations, the occurrence of extremes, etc.) of the climate on all spatial and temporal scales beyond that of individual weather events. Variability may be due to natural internal processes within the climate system (internal variability), or to variations in natural or anthropogenic external forcing (external variability).´ Therefore, climate variability and climate change have equal definitions but for the condition that 'climate change' persists for an extended period, typically decades or longer. However, as 'climate variability" refers to variations of the climate on all spatial and temporal scales beyond that of individual weather events, even this distinction is lost, since then a variation due, for instance, to the Atlantic Multidecadal Oscillation would be at the same time, a manifestation of 'climate variability' and of 'climate change'. According to these definitions, if climate in a region is predominantly dry for 5 years and then turns wetter, there was a 'climate variation'; if it is predominantly dry for 20 years and then turns wetter, there was a 'climate change'. If in both cases the dryness is due to natural internal processes, the definitions provided in this SOD are conceptually confusing. Which is the threshold (or boundary) between them? It should not be a question of time scale, but of causes. Natural internal climate variability should not be called "variations" or "changes" according to its time scale. What is the reasoning for this? This is obviously a very confusing situation for any reader, since it is clear that IPCC would not issue an Assessment Report on Climate Change every 7-8 years if it were analyzing predominantly natural climate variability. The predominant approach of the IPCC Assessment Reports on Climate Change, including this draft, emphasizes 'trends' in different periods ending in 2011, implying that the main interest is on the possible human effect on climate in the last century (or a little longer). Another example of emphasis on anthropogenic influences in connection with climate change appears as soon as the second next paragraph (lines 29-32): 'The global mean surface air temperature remains an important climate change measure for several reasons. Climate sensitivity is typically assessed in the context of global surface temperature responses to a doubling of CO2 and global mean surface temperature is thus a key metric in the climate change policy framework.' The need for a clearer distinction between 'climate variability' and 'climate change' appears in several places of the manuscript, as, for instance, in the synthesis of page 2-71, lines 29-33, where variability and change are used with different meanings, being the latter associated with trends (other examples: page 2-7, lines 4-6 and 12-15, and section 2.7). The Framework Convention on Climate Change (UNFCCC), in its Article 1, defines climate change as: 'a change of climate which is attributed directly or indirectly to human activity that alters the composition of the global atmosphere and which is in addition to natural climate variability observed over comparable time periods'. Therefore, UNFCCC makes a distinction between climate change attributable to human activities altering the atmospheric composition, and climate variability attributable to natural causes. This seems a much	Noted. Text has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						better way to distinguish climate variability from climate change than simply a matter of time scale, and much more in line with the purpose of the IPCC Assessment Reports than the one presented in this SOD. There could be some improvement with respect to the UNFCCC definition, but the difference presented here is not convenient, since it is not adequate to call the natural internal climate variability as 'variability' or 'change' according to time scale, and in case of interdecadal variability they are exactly the same thing. 'Variability' and 'change' should be distinguished based on more solid and clearer criteria. A first way would be that already presented in UNFCC, but a little more general. In this option, climate change is a change which is attributed directly or indirectly to human activity. This would include not only effects of alteration of the global atmosphere composition, but also of land use. In this way, climate change would have anthropogenic causes, while climate variability would have natural causes. Another possible way of distinguishing 'climate change' from 'climate variability' would be attributing the former to external forcings (natural or anthropogenic) and the latter to natural internal processes. Both ways are better than the nebulous difference based on time scale. In my view (which seems also to be the public general view), the best way of distinguishing 'climate change' from 'climate variability' is the first way described above that attributes climate change to human activities, while climate variability is of natural origin (internal or external). This would not preclude the AR5 of including the analysis of natural variability and its effects, as well as how it could be affected by climate change. If IPCC wants to change the previous definition of climate change, this change should be emphasized and the reason and basis of this change should be made very clear, differently of the rather inconspicuous way it is presented in page 2-6 or in the glossary (page AIII-5). These similar definitions of "climate variability' and 'climate change' are at odds with their use in several places of the manuscript, as mentioned before. A last remark: if the confusing definitions of climate variability and climate change, which make them interchangeable expressions for interdecadal time scales, have been prompted by the difficulty in attributing the detected variations to human activities or to interdecadal natural variability, then it is better to recognize this difficulty than produce a conceptual confusion in the public. It is important to distinguish the concepts of 'climate change' and 'climate variability' in a very clear way. [Alice Grimm, Brazil]	
2-302	2	6	8	6	8	replace "El Nino" with "the El Nino-Southern Oscillation" or "ENSO" [Government of Australia]	accepted
2-303	2	6	8	6	10	This sentence reads as if it is a definition of climate change. The authors should be careful that is what they intend, as it could be quoted for years to come as the "IPCC definition". [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted. Text has changed
2-304	2	6	8	6	10	The definition of "climate change" given here is not exactly the same as the one given in Chapter 1 [Government of Australia]	Noted. Text has changed
2-305	2	6	8	6	10	Climate change describes statistically significant changes in statistics over the time and spatial scales that can be meaningfully associated with them. Daily changes in a 30-year running mean will never be statistically significant. [Government of France]	Noted. Text has changed
2-306	2	6	8			Multi-decadal is worth a mention in this line, but is it clear why "millenial" is mentioned with it? Climate varies on timescales longer than the millenial. [Adrian Simmons, United Kingdom]	Noted. Text has changed
2-307	2	6	9	6	9	… considered to be … --> … considered to exhibit … [Claudio Cassardo, Italy]	Editorial.
2-308	2	6	9			There is a reference here to "mean state" but no mention of the averaging period over which the "mean" is defined. [Adrian Simmons, United Kingdom]	Editorial.
2-309	2	6	10	6	10	"variability…" --> "variability and extremes" [Richard Allan, United Kingdom]	Editorial.
2-310	2	6	11	6	11	Should "observed" be changed to "inferred"? [Dian Seidel, United States of America]	noted
2-311	2	6	12	6	12	Maybe nuance and add "depending on the variable" since not all those trends are assessed for all variables (even when data are available) [Ileana Bladé, Spain]	noted
2-312	2	6	12	6	12	"1998", this does not seem to be consistent with Table 2.7 (year 1998). [Xuebin Zhang, Canada]	corrected
2-313	2	6	12	6	14	Consider shortening "Trends have been assessed …. for 1979-2011 only" to "Trends have been assessed	noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						over a variety of periods depending upon data avilability." [Richard Allan, United Kingdom]	
2-314	2	6	14			you have described about the period of climatological reference period. That is Ok, but please write this in the figures instead of abnormally. Because it may cause some confusion for readers. [Rahimzadeh Fatemeh, Iran]	noted
2-315	2	6	16			See general comment earlier on the use of the word "trend". Here trend is used to designate a "long-term" movement in a time series. Yet trends computed over a ten-year period are presented for some variables. Is ten years really "long term" when one speaks of climate? [Adrian Simmons, United Kingdom]	Noted. Text has changed
2-316	2	6	17			"as generating the observed values" could be reworded. Observed values are generated by making observations and measurements, not by adding trends, oscillations and random components. The latter are things one computes from the observed values. [Adrian Simmons, United Kingdom]	Noted. Text has changed
2-317	2	6	18			It has been referenced box 2.2 at first(it means before box 2.1). Is'nt it better to change theorder of boxes? [Rahimzadeh Fatemeh, Iran]	noted
2-318	2	6	22	6	24	instead of 'measures' maybe better to use 'observations'. [European Union]	noted
2-319	2	6	22			Would read better if a word such as "utilising" were to be inserted between "particular" and "satellites". Satellites per se are not an observing technique [Adrian Simmons, United Kingdom]	Editorial.
2-320	2	6	23	6	24	Delete "together with more traditional measures". [Government of Australia]	Editorial.
2-321	2	6	24	6	24	Need to be clear about what is meant by "Dynamical reanalyses", i.e., that these use dynamical models to support meteorological analyses that are spatial, temporal and physically consistent based on observations. [Government of Australia]	Noted. Text has changed
2-322	2	6	29	2	29	Global mean surface air temperature is not appropriate to use here as what is actually used is a combination of SST and LSAT [Elizabeth Kent, United Kingdom]	it is defined in the text
2-323	2	6	29			Box 2.1 is introduced in the text after Box 2.2 and Box 2.3 (a small problem) [Michael Coffey, United States of America]	editorial
2-324	2	6	35	6	36	This needs rewriting. Reanalysis products are not "additional observations". They are products formed by processing the existing observational record. [Adrian Simmons, United Kingdom]	noted
2-325	2	6	40	6	40	Change 'a' to 'an' [Peter Burt, United Kingdom]	Editorial.
2-326	2	6	43	6	46	This paragraph of 4 lines is wholly inadequate as a summary of the composition changes. Composition change is the main driver for the climate, and (in section 2.2) comes before all the sections on other variables. It should not be dismissed as “also include”, but expanded significantly and moved further forward in this section. [William Collins, United Kingdom of Great Britain & Northern Ireland]	considered
2-327	2	6	45	6	45	Here and in many other places (please revise): there should be no comma before an “and” that connects two not-independent clauses [Ileana Bladé, Spain]	Editorial.
2-328	2	6	51	6	51	I think “such as those due to changes in planetary waves” would be better [Ileana Bladé, Spain]	Editorial.
2-329	2	6	51	6	51	Here, or nearby, cross refer to Chapter 14. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Editorial.
2-330	2	6	51	6	53	Can you give a single example where global warming ives rise to local cooling due to planetary wave action? [Geert Jan van Oldenborgh, Netherlands]	Noted. Text has changed
2-331	2	6	55	6	55	Please define SREX. [Government of Australia]	editorial
2-332	2	6	55	6	55	Explain the acronym SREX [Ingeborg Levin, Germany]	editorial
2-333	2	6	55	6	56	"Seneviratne et al.2012a" here and elsewhere place, there is only obe SREX for the reference 2012a and 2012b [Xuebin Zhang, Canada]	editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-334	2	6	55			Define SREX, first appears on Page 5 line 6 [Michael Coffey, United States of America]	editorial
2-335	2	7	4	7	10	This paragraph indicates that no interpretation of the detected changes will be undertaken (temporal nature or cause) but this is not strictly true throughout the document. [Ileana Bladé, Spain]	noted
2-336	2	7	4	7	16	This is an important paragraph, but its message could be clarified. Subsequent text often refers to "statistically significant changes," with no reference to the assumed statistical model. At the very least, it should be stated here that, when no statistical model is indicated in later text, then the null hypothesis is one that assumes no serial correlation (or simple AR-1 process, etc.), and subsequent text should be consistent with that statement. [Government of United States of America]	Noted. Text has changed
2-337	2	7	5	7	13	line 5 "whether the observed …. Are outside …. And consistent …" and line 13 "how large …" seem to be self-conflicting. Does Ch2 report statistically signiifcant trend? [Xuebin Zhang, Canada]	Noted. Text has changed
2-338	2	7	7	7	13	The statistical problem of handling long term persistence when computing the significance of temperature trends is of enormous importance for the detection and measurement of climate change, so the continued refusal to deal with this topic in IPCC reports is very unfortunate, especially in light of the massive literature now available on the subject. This paragraph sets the topic aside with the insinuation that researchers can get pretty much get any result they want by choosing their statistical model. Of course the first thing that comes to the reader's mind is that this, presumably, applies to your calculations too. Also, your claim is not demonstrated in the 3 papers you cite. The Mann (2011) paper argues that persistence-like behaviour can be generated in a relatively simple model driven by stationary noise, and then recommends stationary AR representations for statistical modeling on the grounds of Occam's razor. This, however, does not address the points made in the other 2 papers (Cohn and Lins 2005 and Mills 2010), which estimate models that can nest simpler AR structures as restricted cases and show they can be rejected. Occam's razor does not justify using a simple model when it can be explicitly rejected against a more general form. Mills 2010 does state that where rival models cannot be nested a choice must be made, but that is not the point at issue here since he is only comparing more complex models than the ones you use, each of which nest your AR1 model and reject it. Since the use of an oversimplified model, such as AR1, exaggerates the significance of the trends, your continued reliance on it even after it has been long superseded in the expert literature risks providing misleading inferences. You need to present a valid statistical modeling framework, defend it based on a reasonable range of time series modeling tests, and then explain why, if your findings differ from those of others, yours should take priority. [Ross McKitrick, Canada]	See Box 2.2
2-339	2	7	7			"many drivers for the observed changes may exist"; bizaare first sentence of para. Simply state that the chapter does not address reasons for observed changes or trends. We all know that many drivers may exist. [Stephen E Schwartz, United States of America]	noted
2-340	2	7	8	7	10	The following text is unclear and could be removed since I doubt that there is "no physical interpretation of trends" in this chapter and even if not I consider it is reasonable to just state that physical interpretation of trends is dealt with in other chapters. "(e.g., as in Wu et al., (2011) has been attempted either, because the results of such analyses depend entirely on the null hypothesis one formulates (Cohn and Lins, 2005; Mann, 2011; Mills, 2010)." [Richard Allan, United Kingdom]	Noted. Text has changed
2-341	2	7	9	7	40	"More…uncertainty" -> "Of the analyses assessed now, more include an estimate of parametric or structural uncertainty than at the time of AR4." [Jan Cermak, Germany]	editorial
2-342	2	7	9		10	Strike the business about the null hypotheses. Just stick to your last. (for non english native speakers, the reference is to the saying "Shoemaker stick to your last."; means stay within your own expertise.) [Stephen E Schwartz, United States of America]	noted
2-343	2	7	10	7	10	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	Noted. For the reasons discussed in Box 2.2 we use linear trends as a means to synthesize the information into a single number. The rationale behind the method is further expanded in the appendix. These numbers are always backed up by figures showing the nature of the series. A discussion of the causes of the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						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 a climategate 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."[end of quote] 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]	timeseries behavior is out-of-scope for our chapter and rather the domain of Chapter 10
2-344	2	7	15	7	15	Change 'are' to 'is' [Peter Burt, United Kingdom]	editorial
2-345	2	7	20	7	25	This section does a fairly good job of explaining the uncertainties and possible sources of error in the recorded observations at individual weather stations. However, it fails to identify two major sources of error that could adversely affect the accuracy of the computed global mean surface air temperature described in Box 2.2 as "key to understanding both the causes of change and the patterns, role and magnitude of natural variabilities" (page 2-6, lines 33-34). [Reynold Stone, Trinidad and Tobago]	Noted. No changes requested or made in response to this comment.
2-346	2	7	20	7	25	The first is the use of incomplete station data (e.g. stations without data for the last few decades) and station data with many missing values for the periods where data do exist. For instance, at the end of July 2011, the Climatic Research Unit (CRU), University of East Anglia, reluctantly released data for over 5,000 stations. A visual inspection of the data for stations in the Caribbean, Central and South American regions revealed the absence of data for many stations indicating poor coverage of these regions. Moreover, for those stations whose data are housed at the CRU, many of the data series ended in the 1970s with a large number of missing values. [Reynold Stone, Trinidad and Tobago]	Rejected. The changing completeness of coverage is already included in the appendix with a figure that summarizes this issue. It is not on topic for this box. No changes made.
2-347	2	7	20	7	25	The second is the possibility of error in the transmission of data from the various National Meteorological Stations to the CRU. The case of Trinidad and Tobago is cited as an example. An examination of the Trinidad and Tobago monthly mean temperature data (collected at the Piarco International Airport for the period 1951-2011) released by the CRU at the end of July 2011 revealed that two different methods were employed to compute the daily mean temperature which were subsequently used to calculate monthly means. [Reynold Stone, Trinidad and Tobago]	Accepted. The transmission and storage issues have been added to the parentheses that were on 2-7 line 30.
2-348	2	7	20	7	25	For the periods, 1951-1979 and 1991-2011, the daily mean temperature was computed using the arithmetic mean of four hourly values taken at 2:00 am, 8:00 am, 2:00 pm and 8:00 pm whereas for the period 1980-1990, the daily mean temperature was computed as the average of minimum and maximum daily temperatures. This introduced an artificial positive jump in the surface air temperature in 1980 resulting in a spurious increase in temperature. Moreover, no data were sent from Trinidad and Tobago to the CRU for 38 months during the 1951-2011 period. [Reynold Stone, Trinidad and Tobago]	Rejected. This is already covered in the parentheses that were on line 2-7 28. It is also discussed in the land surface air temperature section where such topic specific rather than generic box-style discussion should belong. No changes made to this passage of text.
2-349	2	7	20	7	25	These two major sources of error need to be discussed and measures to address them identified. If the number of stations used to compute the global mean surface air temperature varies in time, one could never be sure whether the observed variability and trend are real or are merely artifacts of the variability in the number of stations used to compute the global mean. [Reynold Stone, Trinidad and Tobago]	Rejected. These are discussed in section 4.2.1 and its accompanying appendix section. It is not appropriate or possible to discuss such specifics in the context of a generic cross-chapter box.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-350	2	7	20	7	52	This box 2.1 could be in introduction (Chapter 1), or in an introduction of all chapters dealing with observations. Or at least Chapter 1 should give ref to box 2.1 in Chapter 2. [Government of France]	Noted. Generic efforts have been made to increase the degree to which our chapter and other chapters reference our boxes. No specific changes to the box text were requested or made in response to this comment.
2-351	2	7	20	7	55	(1)It should be mentioned the lack of observed stations before 1950 and decreasing observed data stations after 1990 in the world. (2)The impacts of urbanization on the observed data should be described. See references: Ren, G., Y.H.Ding, Z.C.Zhao, J.Y.Zheng, T.W.Wu, G.L.Tang and Y.Xu, 2012, Recent progress in studies of climate change in China, Advances in Atmospheric Sciences, 29(05), 958-977 (3)It should point out the uncertainty range of the global annual mean surface air temperature and global precipitation at the end of this box. [Zong-Ci Zhao, China]	Rejected. Comment does not pertain to the contents or remit of the box but rather to other sections within the chapter and the appendix where these specific details are already discussed in some detail.
2-352	2	7	20	7	57	I appriciate you to present a very excellent and important problem here. But I suggest you to add the refrences and if it is possible to present some figures for clarification. [Rahimzadeh Fatemeh, Iran]	Noted. Space restrictions preclude such additions.
2-353	2	7	22	7	22	Delete "were not made for climate monitoring". I suspect that the recent advent of extremely large quantities of remotely sensed data means this is not a valid statement, and more generally the issue around "climate" is not that the observations lack sufficient accuracy, but that the networks and methods lack suitable temporal stability. [Government of Australia]	Noted. While true that a small subset of the satellite measurements have been purely for climate most multi-decadal satellite records have not been so. To address reviewer concerns the qualifier explicitly has been added to the text here.
2-354	2	7	22	7	34	General lack of supporting references in this paragraph [Peter Burt, United Kingdom]	Noted. No specific suggestions make this hard to respond to. Most of the material in this paragraph does not really need supporting references.
2-355	2	7	22	7	55	Box 2.1 on uncertainty in observational records - the three paragraphs demonstrate the difficulties and pitfalls in estimating uncertainty in obs records. Perhaps this dwells too much about the problems - it would be good to know more about (1) the efforts made by reserachers to overcome or better describe uncertainty by reconciling different data sets, and (2) techniques for describing uncertainty, for example the latest SST realisation which has an ensemble of observations (the greater the uncertainty the wider the spread in the ensemble). [Paul van der Linden, Great Britain]	Noted. Space restrictions preclude such additions. There already exists text regarding reconciling different datasets within the box. Further the SST aspects are discussed within the main text within the SST section where their discussion is more appropriate.
2-356	2	7	22	7	55	Box 2.1 on uncertainty in observational records - the three paragraphs demonstrate the difficulties and pitfalls in estimating uncertainty in obs records. Perhaps this dwells too much about the problems - it would be good to know more about (1) the efforts made by reserachers to overcome or better describe uncertainty by reconciling different data sets, and (2) techniques for describing uncertainty, for example the latest SST realisation which has an ensemble of observations (the greater the uncertainty the wider the spread in the ensemble). [Paul van der Linden, Great Britain]	Repeat of 2-355. See that response.
2-357	2	7	23	7	23	What does "as data demands" mean? [Government of France]	Noted. Changed to demands on the data.
2-358	2	7	27	7	29	I consider it wrong to charcterise "station relocations or new satellites" as "errors" in the observational record. They are features of the observational record that have to be taken into account in attempting to extract a climate signal from that record. [Adrian Simmons, United Kingdom]	Accepted. The latter two have been changed from errors to effects to address.
2-359	2	7	27	7	34	Should note somewhat that the RSS data does not include the polar regions, and the UAH data are interpolated to the poles. [Government of Australia]	Rejected. Comment appears misplaced as we are not discussing the MSU datasets here in a box which is subject area generic rather than specific.
2-360	2	7	30	7	30	Insert the word “also” before “have” and make it clear that the temperature record is only a best estimate. [John McLean, Australia]	Editorial. The current text in the following sentence makes the reviewer's latter point clearly.
2-361	2	7	32	7	32	changed → has changed [Ileana Bladé, Spain]	Editorial.
2-362	2	7	32	7	34	Why is this an absolute exception? The uncertainties are smaller, but they are still there. I would not mention this in the introduction. [Geert Jan van Oldenborgh, Netherlands]	Noted. It is an exception because unlike other measurements there exists an unbroken chain of metrological traceability as noted in the text. Changes

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							have been made to clarify this. This text is important to retain given that the next section is on composition where many of the measurements and their uncertainties are directly traceable. This then directly affects the confidence assessments in the ES taht pertain to composition changes. It is important to stress this distinction.
2-363	2	7	34	7	34	Reference to Keeling on this line should be Keeling et al., 1976a [Government of Australia]	Editorial.
2-364	2	7	34	7	34	Mauna Loa, not Manua Loa. Also worth nothing that there probably is uncertainty in those measurements albeit it a much reduced level. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-362
2-365	2	7	34	7	34	The name of observatory should read "Mauna Loa" [Andrey Shmakin, Russian Federation]	Editorial. Corrected.
2-366	2	7	34			I hope that we are not suggesting that there are no uncertainties in the MLO CO2 observations. [Michael Coffey, United States of America]	Taken into account. See response to 2-362
2-367	2	7	36	2	38	This sentence is not very clear. It needs some changes to the punctuation at least. [Elizabeth Kent, United Kingdom]	Accepted. Some changes to clarify have been made.
2-368	2	7	36	7	46	While the first paragraph of Box 2.1 is easy to understand, the second one is rather difficult. Perhaps some re-phrasing and some examples could make it easier. Probably not only experts will read it. [Alice Grimm, Brazil]	Accepted. Attempts have been made to clarify this paragraph.
2-369	2	7	36			I consider the Boxes as rather a tutorial, as such, I find this sentence rather unclear. [Michael Coffey, United States of America]	Taken into account. See response to 2-367
2-370	2	7	38	7	39	The sentence that spans these lines is perfectly correct. But it just raises the question as to why the assessement of the upper-air temperature record by one or more of the independent reanalysis groups was suppressed later in section 2.4.4. See later comments on this. [Adrian Simmons, United Kingdom]	Noted. No changes requested to the text here or made. See responses to latter comments.
2-371	2	7	39	7	40	Clarify sentence "More analyses assessed now…." [European Union]	Noted. Some edits have been made to this end but without specific suggestions it is hard to know what changes were required by the reviewer to clarify this sentence.
2-372	2	7	39		40	rewrite to say "…AR4 include published estimates of…" [Karen Rosenlof, United States of America]	Editorial.
2-373	2	7	42	7	42	what is meant with "like-for-like" ? [Ingeborg Levin, Germany]	Noted. By like-for-like we mean two equivalent estimates are being compared. It is not easy to see how to restate this to make it any clearer than is already the case here.
2-374	2	7	48	7	55	Consider adding a remark about how recognition of these data limitations affect the assessment process and changes from on AR to the next. [Dian Seidel, United States of America]	Noted. After considerable thought on this sensible suggestion it is hard to see how to add sucha sentence without breaking the flow or sounding a little self-serving given that we have changed a number oif the AR4 conclusions herein.
2-375	2	7	49	7	49	definitively → perfectly ? [Ileana Bladé, Spain]	Editorial.
2-376	2	7	50	7	50	defined → assessed or established ? [Ileana Bladé, Spain]	Editorial.
2-377	2	7	52	7	52	I am not familiar with the term“product heritage”and I cannot find it easily on the web. Perhaps explain in parenthesis? [Ileana Bladé, Spain]	Accepted. Changed to dataset heritage
2-378	2	7				Several suggestions were made to combine boxes 2.1 and 2.2 and to provide that information early in the chapter as it is relevant throughout. There were also suggestions to discuss how end points in the time series can produce anomalies. [Government of United States of America]	Rejected. This suggestion was discussed at length in the chapter meeting in Hobart and it was decided to retain the current box structure within the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-379	2	8	1	8	1	Is there a brief section in this chapter or elsewhere that says that CO2, CH4 and N20 have all come from fossil-fuel burning? In other words that this unequivocal (or in your silly terminology - vurtually certain). Something that says the increase is approximately the right value knowing how much fossil fuel has been burnt; that the C13/C12 ratio changes as it is fossil carbon and that it agrees with measurements of the O18/O16 ratios that have been measured since the 1980s. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	For CO2, a reference to its predominant fossil fuel related source is given in section 2.2.1.1.1. N2O and CH4 increases are not related predominantly to fossil fuel related sources.
2-380	2	8	1	8	1	The discussion of atmospheric greenhouse gases prior to discussion of temperature can be construed as preparing the audience for subsequent claims that greenhouse gases cause warming. This is unprofessional and leaves the IPCC open to ridicule. [John McLean, Australia]	Rejected - the bulk of the peer-reviewed literature indicates that LLGHGs are the main drivers of increased radiaitve forcing.
2-381	2	8	2	8	2	This is a good section and its good to see it with other observations. Authors have done a good job sythesising across observations [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted.
2-382	2	8	2			Section 2.2: Coordination between ch2 and ch8 is needed to ensure that we give/use the same lifetimes (or adjustment times) [Jan Fuglestvedt, Norway]	Chapter 8 should provide Ch 2 with lifetimes.
2-383	2	8	4			This section regarding the long-lived GHGs is less detailed compared to the AR4. This is because in AR5, items as radiative forcing and the source/sink distribution of GHGs, are treated in separated chapters (see Chapter 6 and 8). However on my opinion the 2.2.1 paragraph of chapter 2 lacks some important information. A description of the observing programs taken into account in this section (UCI, NOAA, AGAGE and SIO) should be included. For each program it is important to specify how many stations they include, which area of the globe they map, and since how long each measurement program starts recording each species. I recommend at least to include in the text some references describing the characteristics mentioned above unless the programs are described elsewhere. Moreover it is worth to mention which kind of measurements are taken into account (i.e. sample flask, continuous etc…) and from which observing station (all sites of each program are considered or a selection between remote marine boundary layer and continental sites has been performed like in AR4?). At the beginning of subsections regarding each gas the number of measurement stations considered should be mentioned and also the starting period of direct observation. [Florinda Artuso, Italy]	Unfortunately, the reader must refer to original literature and supplementary material at the end of the chapter for such details.
2-384	2	8	4			Section 2.2.1. “Well-mixed” should be used throughout instead of “Long-lived”. The defining characteristic of these gases (as described in this section) is that their abundance can be assessed from a few surface measurements because the mixing ratio varies little across the globe. There lifetime is irrelevant from the point of view of observations. This would also be consistent with the terminology in chapter 8. [William Collins, United Kingdom of Great Britain & Northern Ireland]	They are only well-mixed because they are long-lived.
2-385	2	8	6	8	7	mention of "radiative forcing" -- please add reference to Ch8 here; we note that the term radiative forcing has not yet been introduced in the chapter (might consider adding a reference to the Glossary). [Thomas Stocker/ WGI TSU, Switzerland]	This statement is not relevant to chapter 8.
2-386	2	8	6	8	16	I suggest common time periods for the different gasses are used through out this paragraph. [Moira Evelina Doyle, Argentina]	Rejected - the time periods are used to illustrate specific changes in atmospheric composition.
2-387	2	8	6	8	16	Forcing estimates are done on a variety of timescales here. Wouldn't a cumulative forcing estimate for 1998-2011 be appropriate? [Jeffrey Taylor, United States of America]	Rejected - radiative forcing is the most direct way to summarize climate impacts of LLGHGs.
2-388	2	8	7	8	7	Chapter 8 and I think Chapter 1 are using WMGHGs not LLGHGs - need a consistency check and executitive decisison. [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Changed to WMGHGs.
2-389	2	8	7			forcing. I am pleased to see you get right to forcing. we know that second best, after changes in composition. [Stephen E Schwartz, United States of America]	Noted.
2-390	2	8	8	8	8	"increased" change to" have increased" [Government of Australia]	Editorial - editing will occur before publication.
2-391	2	8	8	8	8	Replace 'many' with 'the major' [European Union]	Rejected - change would make statement incorect.
2-392	2	8	9	8	9	"are controlled by the Montreal Protocol" - the production and emissions of many ODS continue to be	Rejected - the period discussed is in the past.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						controlled by the MP [Dale Hurst, United States of America]	
2-393	2	8	10	8	11	not all observations are "in situ" - e.g., flask samples [Dale Hurst, United States of America]	Measurements from discrete samples are also in situ, even if the analyzers are not.
2-394	2	8	10			"hereafter" refers to all subsequent time, especially time after death; you surely mean "hereinafter". [Stephen E Schwartz, United States of America]	Accepted.
2-395	2	8	11	8	12	This comment on radiative forcing is not necessary here in the chapter on observations as it is covered in chapter 8. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected - RF is the most climate-relevant way to describe increases in LLGHGs.
2-396	2	8	12	8	12	CO2 is not a long-lived greenhouse gas. The annual increase in atmospheric CO2 is only about 50% of the estimated emissions, which means that 50% is absorbed within the calendar year, Correct this statement. [John McLean, Australia]	Rejected - a significant fraction of CO2 emitted from fossil fuel combustion remains in the atmosphere for millennia.
2-397	2	8	12			7.5% increase in rad forcing. need to convey which rad forcing. what is included? ghg forcing only? ghg plus aerosol? anthro? total including solar? Contrast line 7 which is specific "their radiative forcing" referring to ghgs. [Stephen E Schwartz, United States of America]	Accepted (parially) - the section is on LLGHGs, and it is clear this is what the change in RF refers to.
2-398	2	8	14	8	16	This sentence is not strictly accurate as surface observations can be combined with inverse models to infer regional scale emissions (see page 11, line 31 of this chapter). [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected - with or without models, current observation networks can not be used to accurately estimate regional emissions.
2-399	2	8	14	8	16	Is it possible to highlight those regional CO2, CH4, etc. observation networks that are sufficient and those that are not? [European Union]	Rejected - beyond the scope of AR5 chapter 2.
2-400	2	8	15	8	15	the constraint of the emission rates is only valid if the sinks are well known or negligible [Ingeborg Levin, Germany]	This detail was in an earlier draft, but removed to save space.
2-401	2	8	16	8	16	For some regions (i.e., some areas in the US), there may be enough observations for calculation RF and constrain emissions. [Qi Tang, United States]	Rejected - not to accurately estimate emissions.
2-402	2	8	16			to which I would add also not sufficient for process studies determining sources and sinks. might mention the need for OCO and the great loss in not having the intended satellite instrum. [Stephen E Schwartz, United States of America]	We are not advocating for more measurements, in situ or remotely sensed, only indicating the current state of the observation networks.
2-403	2	8	18			here and throughout: "mole fraction". I am pleased to see the use of this terminology. However strictly speaking, Mixing ratio is the quantity and mole fraction (dimensionless) the unit. [Stephen E Schwartz, United States of America]	Noted - but to avoid confusion with various definitions of mixing ratio, will maintain mole fraction.
2-404	2	8	19	8	19	radiative forcing (what year - 2011?) [Government of Australia]	Accepted - text modified.
2-405	2	8	19	8	19	Chapter 2 should not discuss radiative forcing, incorrect number given for LLGHG [Gunnar Myhre, Norway]	Rejected - RF is the most climate-relevant way to describe increases in LLGHGs.
2-406	2	8	19		19	on 2.79 W/m2, chapter 8 (p. 3, line 23 in their summary) gives 2.83 W/m2 for WMGHG RF [Stephen Montzka, United States of America]	Will correct using values from Chapter 8.
2-407	2	8	19			...radiative forcing, which totals 2.79 W m–2 since 1750.' Is this the increase in radiative forcing since 1750? [Michael Coffey, United States of America]	Yes.
2-408	2	8	19			2.79 W m-2. Needs uncertainty. I am becoming increasingly less sanguine about present estimates of uncertainty in forcing by llghgs. An argument can be made that the uncertainties indicated for LW forcings by CO2 and by WMGHGs are substantially underestimated. CO2 forcings and climate response of 15 atmosphere-ocean general circulation models (GCMs) that participated in round 5 of the Coupled Model Intercomparison Project (CMIP-5) were compared by Andrews et al (2012). Forcing and temperature response coefficient were inferred from the output of the model runs respectively as intercept and slope of a graph of net top-of-atmosphere energy flux versus global mean temperature anomaly subsequent to a step-function quadrupling of atmospheric CO2. (Because the model experiments examined response to a quadrupling of CO2, rather than a doubling, the intercept had to be divided by 2 to obtain the forcing pertinent to doubled	Uncertainty will be added when received from Chapter *; the remainder of comment is for Ch 8.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						CO2). The forcing is interpreted as an "adjusted forcing" that includes rapid adjustments, mainly of atmospheric structure, that modify the TOA radiative flux on time scales shorter than a year or so. A key finding of Andrews et al. was the spread of values of forcing exhibited by the different GCMs, 16%, 1-sigma. The spread in forcing is a consequence of differing treatments of the radiation transfer in the several models as well as different treatments of clouds that interact with radiation. As the forcing inferred from the analysis of Andrews et al. is an adjusted forcing, it appropriately reflects differences among the models in rapid (<~ 1 yr) response of atmospheric structure to the imposed forcing. This spread in forcings inferred from the climate model runs is substantially greater than the uncertainty specified in the Figure. That there is such a range of forcing as inferred from GCM runs should not come as much of a surprise. For example, although the Radiative Transfer Model Intercomparison Project (Collins et al., 2006) reported a 1-sigma spread in longwave forcing at 200 hPa among the GCMs compared of only 8.5%, that study was restricted to cloud-free atmospheres, with the reason given that "the introduction of clouds would greatly complicate the intercomparison exercise," from which one infers that the spread of forcing in a model with clouds would greatly exceed that in an idealized cloud-free model. Hence the finding of a 1-sigma spread of ± 16% in the forcings (i.e., 5-95% range ± 26%, well greater than the ± 10% shown in the figure) is likely as accurate an assessment of the maximum level of confidence as can be placed at the present time in forcing by LLGHGs. Andrews, T., Gregory, J. M., Webb, M. J. and Taylor, K. E. 2012. Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere-ocean climate models. Geophys. Res. Lett. 39, L09712. Collins, W. D., Ramaswamy, V., Schwarzkopf, M. D., Sun, Y., Portmann, R. W., Fu, Q. et al. 2006. Radiative forcing by well-mixed greenhouse gases: Estimates from climate models in the IPCC AR4. J. Geophys. Res. 111, D14317. [Stephen E Schwartz, United States of America]	
2-409	2	8	20	8	20	"their" has no contextual connection - "and the emissions and removal of LLGHGs" [Dale Hurst, United States of America]	Accepted - text modified.
2-410	2	8	22	8	22	"production" is not a source unless you instead mean "in-situ production in the atmosphere" - some ODS are produced many years before they are emitted [Dale Hurst, United States of America]	Rejected - text clear as is.
2-411	2	8	22	9	22	the two sources and two sinks should each be connected by "and" instead of "or", for some gases (halons) are both destroyed and removed. [Dale Hurst, United States of America]	Rejected - clear as is.
2-412	2	8	27	8	27	What is meant by 'emissions verification'? [European Union]	It means verifying emissions estimates.
2-413	2	8	27	8	27	An appropriate reference with actual measurements would be here Levin et al., 2011 (Levin, I., S. Hammer, E. Eichelmann, F. Vogel, 2011. Verification of greenhouse gas emission reductions: The prospect of atmospheric monitoring in polluted areas. Philosophical Transactions A 369, 1906-1924.) [Ingeborg Levin, Germany]	Added reference.
2-414	2	8	30	8	30	Include 'air archives and' between 'from measurements of air trapped in' and 'polar ice cores' [Government of Australia]	Noted - text modified.
2-415	2	8	35	8	36	"Unless noted otherwise an average of NOAA and AGAGE global annual surface mean mole fractions" [Dale Hurst, United States of America]	Noted - sentence corrected.
2-416	2	8	35	8	49	Unit definition is inconsistent between the text and the table. For example: in the text "ppt = pmol mol–1" - while the table uses parts per trillion (which is much more understandable). It is assumed that pmol is a pico mole, but that is not defined here. [Government of United States of America]	Noted - table caption modified.
2-417	2	8	36		36	point of this sentence is unclear. It is also unclear what "globally" means in this sentence (throughout the chapter or across the globe...?) [Stephen Montzka, United States of America]	Noted - sentence corrected.
2-418	2	8	39	8	39	"summarizes global annual surface mean mole fractions of LLGHGs from" [Dale Hurst, United States of America]	Reworded.
2-419	2	8	39		39	global instead of globally here? Otherwise it confuses the issue. [Stephen Montzka, United States of America]	Reworded - globally was used because it is an adverb

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							modifying averaged.
2-420	2	8	39			mentions 4 programs, but only 3 columns in table. [Karen Rosenlof, United States of America]	Four programs are identified in the table notes.
2-421	2	8	40	8	41	I think it is appropriate to insert a short overview concerning sampling strategies and techniques for estimating global means and uncertainties for each programs. [Florinda Artuso, Italy]	Rejected - this is beyond the scope of Chapter 2. Details are in the appropriate references.
2-422	2	8	46	8	46	I suggest to replace“Global annually averaged surface dry air mole fractions” with “global annual mean mole fractions”. Are the global means referred to the average of all values recorded in all stations of each program? Please specify. [Florinda Artuso, Italy]	Reworded.
2-423	2	8	46	8	49	Table 2.1 caption: the meaning of ppm, ppb and ppt has been already described at pag.8-line 35 so on my opinion it should not be included in the caption. [Florinda Artuso, Italy]	Noted - table caption modified.
2-424	2	8	46	9	1	Editing of table 2.1 should be reviewed in order to better define the content of each column. [Florinda Artuso, Italy]	Noted - columns reorganized and new column headings added to improve clarity.
2-425	2	8	46	9	13	Table 2.1 does not have clearly expressed units for the lesser gas concentrations [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected - units are given in the table caption.
2-426	2	8	46	9	13	Table 2.1: I still think that GWP values should not be given in this table. GWP100 is just one of several possible metrics for comparing gases and several value related choices are implict in this. Using this here would, in my view, be inconsistent with the presentation and assessment in chapter 8. (In the context of table 2.1 which gives abundances, it would be more relevant, I think, to use radiative efficiency, not an emission metric like GWP). [Jan Fuglestvedt, Norway]	Rejected - GWPs and lifetimes were requested in early drafts.
2-427	2	8	46	9	13	Table 2.1 provides GWPs which are stated to be taken from Chapter 8, but the values differ from those provided in Chapter 8 (Table 8.A.1). There is no apparent explanation for the differences between chapters. Chapter 2 values appear to be the same as in AR4. [Government of Australia]	Correct GWPs based on Ch 8 appendix.
2-428	2	8	46	9	13	a citation for the source of the lifetimes reported in the table is lacking [Stephen Montzka, United States of America]	Will be ch 8 appendix.
2-429	2	8	46	9	13	The column labeled SIO/AGAGE in Table 2.1 is very misleading. These networks are not connected at all and the vast majority of the data is from AGAGE not SIO. They should be separated just as NOAA and UCI are separated (and there is room to add another 2 columns to achieve this). This current nomenclature makes AGAGE look minor and anyone not deeply engaged in these measurements will be seriously misled. I am certain that the funding agencies for AGAGE and the leaders of each AGAGE station will be much disturbed at this confusion. The footnotes should state briefly whether the data from each network come from frequent on-site or less frequent flask sample measurements. Also, references to overview papers/websites for each of these networks should appear in the footnotes (e.g. for AGAGE Prinn, Weiss et al, JGR, 115, 17751-17792, 2000; http://agage.gatech.edu). [Ronald Prinn, United States of America]	Rejected - clarification of difference between AGAGE and SIO enhanced.
2-430	2	8	46	9	14	My view is that GWP should not be presented in Table 2.1 [Gunnar Myhre, Norway]	Rejected - It was added in response to reviewer's comments to add context to Table 2.1.
2-431	2	8	46	9	14	It would be nice to have additional columns with AR5 best estimates for the various gases. The estimates may either be averages of the available data or just assessment of best estimates. [Gunnar Myhre, Norway]	These are already given the the appendix to Chapter 2.
2-432	2	8	46		49	Table 2.1. While it may be difficult to fill in uncertainties in all the lifetimes, those for which we have recent estimate of should be included (or revised and included) so that it is a recognized component of uncertainty in projecting abundances, GWPs, etc. For CH4, N2O, and HFC-134a, this is published (see note for p.9/l.10), can leave the other lifetimes as is. [Michael Prather, United States of America]	The lifetimes in Table 2.1 are given for context; uncertainties should be given in Chapter 8.
2-433	2	8	46			When would the new GWP of the studied GHG be incorporated [Government of Chile]	Prior to completion of the final draft.
2-434	2	8	46			Table 2.1. The meaning of the different columns should be better clarified. It is not quite clear which ones refer to absolute concentrations and which ones to changes. Also the columns with the absolute concentrations should probably be grouped together and the same should be done with the changes. [European Union]	Columns reorganized and re-labeled to make more clear.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-435	2	8	49	8	49	in Table 2.1 why is only the CH4 lifetime approximate "~" ? [Dale Hurst, United States of America]	It was a placeholder while waiting for lifetimes from Chapter 8.
2-436	2	8	50	8	50	the correct symbol for approximately is ≈ (not ~) [Ingeborg Levin, Germany]	Rejected - "~" means "about".
2-437	2	8	52	8	52	The estimated lifetime of CO2 should also be given in this table. [Xiaobin Xu, China]	Rejected - there is no lifetime for CO2.
2-438	2	8		20		Is there any scope here for a brief discussion of limitations of the current observing system (for each chemical composition variable/group of variables) pointing out the gaps/major uncertainties? [European Union]	This is outside the scope of Chapter 2. Already it is stated that current networks are sufficient for estimating global parameters, but not regional emissions.
2-439	2	8				Table 2.1: the UCI global methane data are included in this table but as far as I can tell there are no references to the published UCI global CH4 data; if none published in the current review period, then cite earlier work [Government of Australia]	Noted. Documentation, including citations, for observations pushed to on-line supplementary material.
2-440	2	8				Table 2.1 Is there not lifetime for CO2? [Government of Chile]	There is not.
2-441	2	8				Table 2.1: I consider it better to add a results by WMO GHG Bulletin (http://www.wmo.int/pages/prog/arep/gaw/ghg/GHGbulletin.html). [Takashi Maki, Japan]	Rejected - WMO GHG Bulletin results are not sufficiently documented.
2-442	2	8				Section 2.2.1 To support the continuation of surface observation sites, I consider it better to add a figure which shows a map of surface observation network (ex. From WDCGG). http://ds.data.jma.go.jp/gmd/wdcgg/cgi-bin/wdcgg/map_search.cgi [Takashi Maki, Japan]	Rejected - beyond the scope of AR5 chapter 2.
2-443	2	8				Table 2.1: Lifetimes for CFC and HCFCs. There is a growing body of evidence that some of these lifetimes (particularly CFC11) may be different than those listed. This chapter should discuss the latest findings in the new SPARC lifetimes assessment (lead organizers are Malcom Ko and Stefan Reimann). This comprehensive report is in the late draft stages, similar to the IPCC assessment, and it should be finalized in spring 2013. There are also a number of papers in press that discuss new, observationally-based estimates for lifetimes. These should also be referenced in this chapter. [Kenneth Minschwaner, United States of America]	Lifetimes will be updated with those used by Chapter 8, but detailed discussion of the lifetimes is beyond the scope of this chapter.
2-444	2	8				changes in atmos composition. I am pleased to see the discussion starting here. This is the logical flow: changes in composition, measureable; can influence climate. this is change in chemical climate, if you wish. well known. this is the solid foundation on which everything else rests. You might wish to convey that. [Stephen E Schwartz, United States of America]	Noted.
2-445	2	9	8	9	8	Please define what is the meaning of the uncertainties for each programs or insert references. Also define the meaning of the uncertainties regarding the change of the air mole fractions since 2005. [Florinda Artuso, Italy]	All uncertainties are 90% confidence intervals calculated by each program.
2-446	2	9	10			Budget lifetime and perturbation lifetime are precise and correct use of the terminology (i.e., the perturbation lifetime is actually the steady-state lifetime of a perturbation put on top of the current state and abundance levels). Please consider updating both values to recently published Prather, Holmes, Hsu analysis (March 2012 GRL) and include uncertainties. Note that the 2012 GRL paper presents a methodology for including all information and propagating uncertainties in lifetimes and current budgets. The specific numbers can/should be updated based on Chapters 2 & 5 & 6 pieces that go into the model for propagating lifetimes, budgets and uncertainties. Currently the CH4 lifetime is 9.1+-0.9 y with pert. lifetime of 12.4+-1.4 y; for N2O these are 131+-10y and 121 +-10 y [Michael Prather, United States of America]	Noted - CH4 and N2O budget lifetimes are now from Prather et al. (2012). Other lifetimes updated to match those used by Chapter 8.
2-447	2	9	12			Great to have the uncertainty in Pre-I, consider the ranges in chapter 6 also: "Centennial variations of up to 10 ppm CO2, 40 ppb CH4 and 10 ppb N2O occur throughout the late Holocene." [Michael Prather, United States of America]	Noted - modified
2-448	2	9	17	9	21	The caption of Figure 2.1 is not clear: is the red line representing the deseasonalized monthly time series of Mauna Loa and South Pole averaged values measured by SIO? Is the blue line representing the average of the values recorded at the same sites but measured by NOAA? Why the SIO values have been deseasonalized? [Florinda Artuso, Italy]	Yes, red is SIO global mean determined from MLO and SPO. Text for NOAA modified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-449	2	9	17	9	21	As in REFOD, I strongly recommend superposing Fig. 2.1b with monthly global temperature records measured in the troposphere by any method and check that d(CO2)/dt FOLLOWS temperature with a lag of 9-11 months as shown by Humlum et al (2012). Also, the variability of d(CO2)/dt which varies by a factor as large as 5-6 from one year to another, is hardly attributable to anthropogenic emissions which do not vary so much from one year to another. [François Gervais, France]	Rejected - beyond the scope of AR5 chapter 2.
2-450	2	9	17			"mole fraction"; as noted this is the unit, not the quantity; the quantity is mixing ratio; the unit is mole fraction. One doesn't label the mass of an object as kilograms; one labels it as mass and reports it in units of kg. does this matter? Only to someone who cares. But the hallmark of precise work and thinking is care. [Stephen E Schwartz, United States of America]	Noted, but not implemented.
2-451	2	9	24	9	26	The figure 2.2 (a) shows an evident discrepancy between the AGAGE CH4 values recorded during the period 1989-1993 and the CH4 time series recorded by the other two programs. Such discrepancy is amplified in fig.2.2 (b) where the growth rate is reported. The AGAGE CH4 growth rate reported before the 1993 is out of scale and not in phase with UCI and NOAA. I suggest to comment this discrepancy or in case an explanation is not available, to not include AGAGE values before 1993. Why the growth rate values reported in chapter 2 of AR4 (figure 2.4) are so different? Please check it. [Florinda Artuso, Italy]	The growth rates here and in AR4 were calculated with different methods. A reference has been added here.
2-452	2	9	34			Suggest that the authors consider the EPA CASNET results in addition to the IMPROVE data. [Government of United States of America]	pg 19
2-453	2	9	34			Suggest the authors consider nitrate or total organic N in addition to sulfate. Particulate nitrate and ammonium are very important components in fine and coarse PM. They are also routinely measured by the IMPROVE network. [Government of United States of America]	pg 19
2-454	2	9	34			Organic aerosol plays an important role in climate forcing. I could have been better represented in the overview of surface observations sectiond. [Government of United States of America]	pg 19
2-455	2	9	34			The IMPROVE Network should be explained/described when it is introduced. [Government of United States of America]	pg 19
2-456	2	9				Table 2.1: please use IUPAC rules for chemical formulae in the Notes - the rule is that halogen ligands are listed alphabetically - i.e. not CFCl3 but CCl3F, not CF2Cl2 but CCl2F2, not CF2ClCFCl2 but CClF2CCl2F, not CHF2Cl but CHClF2, not CH3CFCl2 but CH3CCl2F, not CH3CF2Cl but CH3CClF2; HFC-143a should be CH3CF3 to be consistent with all other HFCs etc. [Government of Australia]	IUPAC rules applied.
2-457	2	10	1	10	6	Caption of figure 2.4. Why UCI values have not been included? Differences in CCl4 and HCFC-142b seem not to be so high from tab.2.1. Moreover it is quite strange that the same statement is reported in AR4 (chapter2, fig.2.6 pag.145). Are these differences unchanged since AR4? Please check it. HCFC-142b records are mentioned in the caption and also commented in the text and reported in tab.2.1 but the corresponding plot is not reported in figure 2.4. Why? In contrast figure 2.4. reports records of HFC-245fa and HFC-365mfc that are not mentioned in this section and in table 2.1 but only in Appendix 2.A (pag.112). I suggest to report in fig. 2.4 all time series of the species commented in section 2.2.1 (long lived greenhouse gases) and reported in tab.2.1. [Florinda Artuso, Italy]	UCI values were not received in time for inclusion in the figure or discussion. Further details of differences given in table in supplementary material.
2-458	2	10	1			Section 2.2.1.1.1 Carbon dioxide: This section says that the increase in CO2 is due mostly to fossil fuel combustion (a % or amount in Pg C yr-1 would be appropriate to add here) although the section later states that interannual variability in [CO2] is due to terrestrial CO2 sink/source activity. These statements may appear contradictory to a non-specialist. Consider either explaining the difference between variability and mean concentration or rewriting these statements. [Government of United States of America]	Rejected - CO2 budget details are in Chapter 6, and the proposed discussion is beyond the scope of Chapter 2.
2-459	2	10	10	11	17	Paragraphs regarding CO2, CH4 and N2O should report the number of observing stations considered in this work. [Florinda Artuso, Italy]	Rejected - readers will have to consult the literature in the appendix/supplementary material.
2-460	2	10	11	10	11	Insert 'the' before 'South' [Peter Burt, United Kingdom]	Rejected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-461	2	10	11	10	27	This section on CO_2 should include interesting new results on the CO_x budget of the upper atmosphere (see the recent Nature Geoscience paper by Emmert et al) that indicates unusual trends in CO_2 in the mesopause/lower thermospheric region. The implications of the measurements reported in this paper, for CO_2 cooling of the upper atmosphere, are also highly relevant to chapter 2 and should be discussed. [Kenneth Minschwaner, United States of America]	Rejected - This work, while interesting, has more relevance for climate satellites than for climate. It is beyond the scope of AR5, Chapter 2.
2-462	2	10	12	10	12	Author name in capitals [Peter Burt, United Kingdom]	EndNote format issue that authors have no comtrol over.
2-463	2	10	12	10	13	The reference to Keeling in this sentence should be Keeling et al., 1976a,b [Government of Australia]	EndNote format issue that authors have no comtrol over.
2-464	2	10	12			KEELING' is all caps [Michael Coffey, United States of America]	EndNote format issue that authors have no comtrol over.
2-465	2	10	12			KEEIING to be corrected [Umesh Kulshrestha, India]	EndNote format issue that authors have no comtrol over.
2-466	2	10	17	10	17	Spell out abbreviation if mentioned first time (NH) [European Union]	Noted.
2-467	2	10	17	10	18	Is there a reference showing the breakdown of contributing factors to CO2 increase for the globe and by region or is this covered in another Chapter? The GWP/GTP for each atmospheric component and sector (agriculture, aviation etc.) is discussed in Chapter 8 which is perhaps the closest thing. A link might be useful here. Is there any where else that the IPCC could point the reader to find regional information? [European Union]	Chapter 6 covers the CO2 budget.
2-468	2	10	18	10	18	refer to chapter 6 [European Union]	Reference to 6 added.
2-469	2	10	19	10	19	These “Multiple lines of observational evidence” should be listed here, or cross-referenced if they appear elsewhere in the Report. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected - a more thorough description was removed to save space, and it is available in the citation.
2-470	2	10	19	10	20	'Multiple lines of observational evidence …' shouldn't there be an IPCC explicit confidence indication attached to this statement? [Government of Australia]	Yes, in Chapter 6. Here the statement provides context for descriptions of the observations.
2-471	2	10	19	10	20	"most of the CO2 increase is due to human emissions". This is not in accordance with several studies that point out that the oceans preferentially release more CO2 containing C12, which is the same type as released by fossil fuels. [John McLean, Australia]	Rejected - the statement is in complete agreement with the bulf of the peer-reviewed literature.
2-472	2	10	20	10	21	"Since the last year…..in 2011." This sentence is illogical due to the phrase "since the last year". [Xiaobin Xu, China]	Rejected - clear as is.
2-473	2	10	22	10	22	omit "(from 1 Jan in one year to 1 Jan in the next year)" because it is implied by "annual average" [Dale Hurst, United States of America]	Rejected - it is stated to make clear the annual increases are not calculated as the difference between annual means, as is often done.
2-474	2	10	23	10	24	"1.68 ± 0.55 ppm yr-1" - same for "2.0 ± 0.3 ppm". Significant figures for the two mean values are inconsistent. [Dale Hurst, United States of America]	Significant figures now consistent.
2-475	2	10	24			"significantly" has different meaning in tech writing (statistically significant) and common usage (substantial); I expect it is the latter intended here, but potentially confusing because of all the ± uncertainties given here. [Stephen E Schwartz, United States of America]	Noted - deleted "significantly".
2-476	2	10	25	10	25	CH4 growth rate showed in Fig.2.2 during 1998 seems to be higher than 3 ppm yr-1 while the reported value in the text is 2.90 ppm yr-1. Please check it. [Florinda Artuso, Italy]	The instantaneous growth rate in a year can be greater than the annual increase.
2-477	2	10	25			"Most of this interannual variability (IAV) in growth rate is driven by small changes in the balance between photosynthesis and respiration on land, each having global fluxes of ~100 PgC yr–1" This statement may be correct, but it would seem that there would be no way to exclude photosynthesis and respiration that occurs over the ocean, from algea and whales, for example. Further similar processes	Noted - the statement in the text is consistent with the bulk of the peer-reviewed literature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						(photosynthesis and respiration) occuring below the ocean do contribute to the atmospheric budget of greenhouse gases."Most of this interannual variability (IAV) in growth rate is driven by small changes in the balance between photosynthesis and respiration on land, each having global fluxes of ~100 PgC yr–1" [Government of United States of America]	
2-478	2	10	25			"±"; specify what it means: one sd? [Stephen E Schwartz, United States of America]	Added a general statement to the introduction that all uncertainties are 90% c.i., unless noted otherwise.
2-479	2	10	26	10	27	It would be very informative to state the magnitude of the imbalance between the huge (~100 PgC yr-1) source and sink that leads to the annual CO2 growth [Dale Hurst, United States of America]	Text modified.
2-480	2	10	29	10	29	the correct symbol for approximately is ≈ (not ~); shouldn't it be better ≈120 PgC yr-1, as in Figure 6.1? [Ingeborg Levin, Germany]	Noted - value changed to match Chapter 6. "~" means "about".
2-481	2	10	29	10	52	I recommend to report in the text the globally averaged CH4 value recorded in 2011 (see table 2.1 or table 2.A.1.) in order to highlight the difference with the preindustrial value. Also differences with AR4 should be reported. I suggest to calculate the percentage of the increase between the actual value and the one estimated for 1750 from ice cores, as it has been done for N2O at the beginning of the paragraph. [Florinda Artuso, Italy]	Noted - added 2011 global annual mean.
2-482	2	10	30	10	31	For comparison, please quote also the 2011 AGAGE and NOAA CH4 values(and add that the NOAA-2004 scale is very close to the AGAGE(Tohoku University) scale adopted by AGAGE in 2002). [Ronald Prinn, United States of America]	Rejected - this will take more space and not add to the assessment.
2-483	2	10	30	10	34	For consistency with other parameters, suggest quote 2011 concentrations of CH4 [Government of Australia]	Noted - added 2011 global annual mean.
2-484	2	10	30	10	45	It may be helpful to add some explanations of the declined increase rate in the last two decades of last century. [Qi Tang, United States]	Noted - this is done in Chapter 6.
2-485	2	10	30	10	45	There are many papers focusing on this topic, such as Aydin et al., nature 2011; Kai et al., nature 2011. [Qi Tang, United States]	Rejected - these papers are more appropriate for chapter 6.
2-486	2	10	30		32	Section 2.2.1.1.2 Methane There should be an explanation for why increases in methane may have started in pre-industrial times. [Government of United States of America]	Rejected - for chapter 6.
2-487	2	10	31	10	31	Space required between brackets [Peter Burt, United Kingdom]	Noted.
2-488	2	10	35	10	35	The expression "a decreasing rate of increase" is slightly confusing [Birgit Hassler, United States of America]	Noted, but it succinctly describes the data.
2-489	2	10	36	10	37	More recent references are needed. [Xiaobin Xu, China]	We can only reference what is published, what is appropriate, and what is know to the writing team.
2-490	2	10	37	10	37	What does [OH] stand for and why the square brackets? I couldn't find it previously in this chapter or the chapter 1 but I then found it on page 12 (chapter 2) in round brackets? Can this definition be moved to the first instance? I'm still not sure of the significance of the brackets as other things are not bracketed. [Kate Willett, United Kingdom]	Noted - text modified.
2-491	2	10	37		39	sentence is correct, but fails to address the recent increase, which is of greatest concern; i.e., steady state doesn't seem to apply anymore... Perhaps add a sentence to provide context to recent increase (is it the result of some additional, variable emission in years since 2006, or a continually increasing emission since that time?). [Stephen Montzka, United States of America]	The reasons for the increase follow in lines 40-45.
2-492	2	10	39	10	39	IAV: interannual variability? [European Union]	Defined pg 10, L26 of SOD.
2-493	2	10	40	10	42	What sustains the CH4 increase through 2010 after the 2007-2008 temperature and precipitation anomalies? [Dale Hurst, United States of America]	There are no peer-reviewed papers to assess for this period.
2-494	2	10	40	10	45	There is potential for a contradiction about CH4 emissions from the arctic between these two sentences. [Jeffrey Taylor, United States of America]	Made clear that there has not been a permanent measureable increase in Arctic emissions from wetlands and hydrates.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-495	2	10	41		45	These sentences say that drivers for increases in methane are likely higher temperatures in the Arctic but then says that Arctic methane emissions from wetlands have not increased measurably. Can this be clarified? [Government of United States of America]	Reworded.
2-496	2	10	41			What are the primary sources of CH4? How do anomalously high temperatures and greater than average precip produce increased abundances of CH4? Just a couple of sentences on the physical processes involved would be helpful for the lay person. [Government of United States of America]	Such a discussion is beyond the scope of Chapter 2; see chapter 6 or the cited references.
2-497	2	10	42			Consider a brief sentence on the physical significance behind the words, "...greater than average precipitation in the tropics during 2007 and 2008..."? How does increased precipitation in the tropics influence CH4 concentrations? [Government of United States of America]	Reword.
2-498	2	10	43	10	45	The Rigby et al (GRL,35, L22805, 2008) study also supports the conclusion that the increase is not dominated by the Arctic; the increase occurred at almost the same time in both hemispheres. Please reference this. [Ronald Prinn, United States of America]	Reword.
2-499	2	10	44	10	45	This isn't quite right: Dlugokencky et al actually say ' we have not yet activated the strong climate feedbacks from permafrost and CH4 hydrates'. [European Union]	Reword.
2-500	2	10	47			The stratosphere is a strong sink for CH4 and should be mentioned. What do we know about changes in stratospheric OH between 1979 and 2004? [Dietrich Feist, Germany]	Stratospherric sinks of CH4 are described in section 6.1. There are no direct or indirect measures of stratospheric OH.
2-501	2	10	56			This sentence seems to call for a reference to go with the figure of 270 ± 7 ppb estimated from ice cores. [Adrian Simmons, United Kingdom]	Noted.
2-502	2	10	57	10	59	Here a relevant reference would be Röckmann and Levin, 2005, which presents N2O isotope measurements on archived air samples from Neumayer station from 1990 - 2002, coming to the same conclusion (Röckmann, T. and I. Levin, 2005. High-precision determination of the changing isotopic composition of atmospheric N2O from 1990 to 2002. J. Geophys. Res. 110, D21304, doi:10.1029/2005JD006066.) [Ingeborg Levin, Germany]	Added reference.
2-503	2	10	58	10	58	Wording: "emissions from soils treated with nitrogen fertilizer and manure" better "from soils fertilized with synthetic and organic (manure) nitrogen fertilizers" [European Union]	Noted - modified text.
2-504	2	11	2	11	3	This sentence is confusing - is it suppose to be saying that N2O and CFC-12 are approximately equal 3rd in the list of GHGs, listed according to the magnitude of their radiative forcing? [Government of Australia]	Noted - text modified for the meanest understanding.
2-505	2	11	9	11	9	Higher N2O concentrations in the tropics/ subtropics are likely coming from significant soil N2O emissions from tropical forests. I am not sure that upwelling ocean water is indeed significantly contributing here. For the global source strength of tropical forest soils for N2O see Werner et al. 2007 A global inventory of N2O emissions from tropical rainforest soils using a detailed biogeochemical model. Global Biogeochemical Cycles, 21, GB3010, doi:10.1029/2006GB002909 [European Union]	Text modified to accommodate comment.
2-506	2	11	10	11	10	Do you mean peak-to-minimum? [Qi Tang, United States]	No. Peak to peak amplitude is commonly used to describe max to min.
2-507	2	11	20	11	22	The AR5 should stand on it's own. It is irrelevant what's in WMO 2010. The authors should make their own judgment on the length of description needed to convey the information needed to the readers. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected - if there was sufficient space, the budgets would be re-evaluated here, but, since there is not, WMO is referred to.
2-508	2	11	24	11	24	What does “low” mean – compared to other halogens? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted - text modified.
2-509	2	11	24	11	24	"abundance are low (6-63 ppt)" - "low" by itself is not very descriptive [Dale Hurst, United States of America]	Noted - text modified.
2-510	2	11	24	11	27	Can you quantify how small the 'relatively small' contribution of HFCs to radiative forcing is? Relatively small compared to a specific other GHG for example? [European Union]	Noted - text modified.
2-511	2	11	24		24	low...and small relative to what? Perhaps add "abundances are low...and forcing is small relative to the chemicals they are replacing (CFCs and HCFCs). [Stephen Montzka, United States of America]	Noted - text modified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-512	2	11	29	11	32	There is no mention here, or anywhere else in the Chapter, of the likely transition away from HFC-134a in certain applications. Do you want to include some information on the new, very short-lived, HFOs in this section? The HFOs 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 which is the most emissive use of HFCs. GWPs are given for several HFOs in Table 8.A.1 of Chapter 8. If you want to include something you might 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]	Noted - there is insufficient space to describe species with very small RF and future forcing agents.
2-513	2	11	30	11	30	Table 2.1 has 62.4 ppt as the SIO value, 63.0 for NOAA, and the 2005-2011 increase is given as 28.2 ppt, not "22.8 ppt" [Dale Hurst, United States of America]	Noted - text corrected.
2-514	2	11	31	11	31	Replace "occur" with "can be attributed to" [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected - prefer current wording.
2-515	2	11	34	11	34	"ambient air from 5 sites" or "ambient air at 5 sites" ? "from" implies flask sampled, "at" implies in situ measurements [Dale Hurst, United States of America]	Noted - modified.
2-516	2	11	35	11	35	sounds like 5.2 ppt is 2005 global mean instead of 2005-2011 increase. "used to calculate the 2005-2011 increase of 5.2 ppt" [Dale Hurst, United States of America]	Noted - table 2.1 makes this clear.
2-517	2	11	37	11	37	What is the value of the growth rate peak? Is this peak in 2006 calculated taking into account the data of the archive from Cape Grim as stated at pag.11-line 36? When does the archive start? In the text is stated that "Direct measurements of HFC-123 began in 2007 (line 34-35) However time series in fig. 2.4 starts in 1980. [Florinda Artuso, Italy]	It was 1.19 ppt/yr, but such details are not important in this discussion. The extended time series is based on the air archive from Cape Grim.
2-518	2	11	40	11	42	Sentence beginning "Currently, ": Are these references to bottom-up studies or observations, or a mixture of the two? [William Collins, United Kingdom of Great Britain & Northern Ireland]	These are top-down studies based on observations and inverse models.
2-519	2	11	40	11	42	The original text says: "Currently, the largest emitter of HFC-23 is China (Kim et al., 2010; Stohl et al., 2010; Yokouchi et al., 2006); developed countries emit <20% of the global total." It is inappropriate to select one country as a case. It's recommended to take out "the largest emitter of HFC-23 is China (Kim et al., 2010; Stohl et al., 2010; Yokouchi et al., 2006)." [Government of China]	Noted - text changed to state largest emissions from east Asia.
2-520	2	11	43	11	43	"but their global contribution remains small." [Dale Hurst, United States of America]	Rejected - clear as is.
2-521	2	11	44		44	citation of (WMO, 2011)--probably should be consistent on citing Chapter authors of these reports, or like this (Montzka et al., 2011b is used on line 21 of this page), unless you are purposefully citing the entire report. [Stephen Montzka, United States of America]	Noted - changed citation.
2-522	2	11	48	11	49	HFC-32 is missing in Table 2.1 [Dale Hurst, United States of America]	Noted - this table is not meant to be all-inclusive.
2-523	2	11	52	11	53	"as components" is redundant - "are mainly used in refrigerant blends" [Dale Hurst, United States of America]	Noted - text changed.
2-524	2	11	54	11	55	"of emissions reported to the UNFCCC, after adding estimates of unreported emissions in east Asia." [Dale Hurst, United States of America]	Noted - modified.
2-525	2	11	55	11	55	need a reference for 'emissions from E Asia' [Government of Australia]	Noted - reference was given above when HFC-125 first introduced, but now repeated for clarity..

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-526	2	11	55		55	Unclear. So emissions from East Asia of what magnitude were added before assessing with consistency with UNFCCC (and from what study)? [Stephen Montzka, United States of America]	Noted - modified and added reference.
2-527	2	12	1	12	9	Brief mention of newly quantified heavier PFCs and their very high GWPs (Ivy et al, ACP,12, 4313-4325, 2012, and Ivy et al, ACPD, 12, 12987-13014, 2012) should be added here. [Ronald Prinn, United States of America]	These are discussed in the chapter's appendix or supplementary material.
2-528	2	12	3	12	3	(Deeds et al, 2008 and references therein) - Deeds et al. were not the first to discover this. The authors are careful to acknowledge the earliest work on measurement and calibration of CO2 (Keetling et al., 1976a,b) and yet do not afford this acknowledgement to all the initial work on measurement and calibration of the non-CO2 GHGs - CH4, N2O, CFCs etc. Why this bias toward CO2 and the early researchers in the CO2 field alone? [Government of Australia]	Noted - added "and references therein".
2-529	2	12	3	12	4	"with a pre-industrial (1750) level of 34.7 ± 0.2 ppt determined from Greenland and Antarctic firn air" - 1750 could be mistaken for the preindustrial level [Dale Hurst, United States of America]	Noted - deleted 1750.
2-530	2	12	16	12	17	This paragraph is confusing. Where does the last sentence leave us now - so the 2011 annual mean of 7.28ppt is correct but previously we thought that developed countries were contributing less? Perhaps moving this last sentence to earlier in the paragraph would help and stating the 'actual SF6 emissions from developed countries' in ppt. [European Union]	Rejected - text clear as is.
2-531	2	12	19	12	19	Halons are not described in this section but only in the Appendix 2.A. Moreover from line 38 to 49 only Montreal Protocol chlorinated solvents (CCl4 and CH3CCl3) are commented instead of all chlorocarbons. So it is more appropriate change the title of this section to “Montreal protocol gases (CFCs, chlorinated solvents and HCFCs). [Florinda Artuso, Italy]	Noted - section title corrected.
2-532	2	12	19	13	4	Section 2.2.1.2: There are some new papers in ACPD on lifetimes of various halocarbons which could be relevant (Rigby et al., Laube et al., Minschwaner et al.) [Jan Fuglestvedt, Norway]	Noted - detailed discussions of lifetimes is more suitable for Chapter 8.
2-533	2	12	21	12	57	See comment above for CFC and HCFC lifetimes and budget reanalyses. [Kenneth Minschwaner, United States of America]	Lifetimes will be updated with those used by Chapter 8, but detailed discussion of the lifetimes is beyond the scope of this chapter.
2-534	2	12	22			Suggest the authors define the unit: PgCO2-eq/yr [Government of United States of America]	Rejected - equivalent CO2 emissions defined in glossary.
2-535	2	12	23	12	23	"after reductions for stratospheric O3 depletion and use of HFCs" is a confusing statement. [Dale Hurst, United States of America]	Noted - text modified.
2-536	2	12	23	12	23	the meaning of the text in brackets is unclear to me [Ingeborg Levin, Germany]	Noted - text modified.
2-537	2	12	23		23	unclear what "after reductions for stratospheric O3 depletion and use of HFCs" refers to specifically [Stephen Montzka, United States of America]	Noted - text modified.
2-538	2	12	27	12	27	I suggest you make it clear that you mean GWP for 100 years time horizon. [Jan Fuglestvedt, Norway]	Noted - text modified.
2-539	2	12	28	12	28	why are you completely ignoring UCI numbers here? [Dale Hurst, United States of America]	UCI values were not received in time for inclusion.
2-540	2	12	34	12	34	"in a study of the CFC-11 lifetime" is not needed. "estimated by Douglass et al. (2008) using models" [Dale Hurst, United States of America]	Noted - text deleted.
2-541	2	12	36	12	36	"rather than current and future production." [Dale Hurst, United States of America]	Rejected - clear as is.
2-542	2	12	38	12	38	again, why are you completely ignoring UCI numbers here? [Dale Hurst, United States of America]	See above.
2-543	2	12	39	12	39	replace word 'lab' with 'laboratory' [Government of Australia]	Text modified.
2-544	2	12	44	12	44	add: "because little if any of this compound was banked" [Dale Hurst, United States of America]	Rejected.
2-545	2	12	44	12	44	again, why are you completely ignoring UCI numbers here? [Dale Hurst, United States of America]	See above.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-546	2	12	44		49	some of this is redundant with text on p. 10, line 50-52. Some text could be saved if these points were made there... [Stephen Montzka, United States of America]	Noted - this text merged with previous text and presented with paragraph on OH.
2-547	2	12	45	12	45	add: "and recent emissions are estimated to be very small" [Dale Hurst, United States of America]	Rejected - text deleted.
2-548	2	12	51	12	54	run-on sentence should be broken in two. [Dale Hurst, United States of America]	This is not a "run on" sentence, but it was broken in two for clarity.
2-549	2	12	52			For easier reading break the sentence like this "... currently capped. Based on changes ..." [Dietrich Feist, Germany]	Noted - sentence broken into 2 sentences.
2-550	2	12	54	12	54	"As a result" of what? That "their global production is not currently capped" or that "there has likely been a shift in emissions within the NH" ? [Dale Hurst, United States of America]	Rejected - clear as is.
2-551	2	12	55	13	2	again, why are you completely ignoring UCI numbers here? [Dale Hurst, United States of America]	See above.
2-552	2	13	3	13	4	what "approximately doubled from 2004 to 2007"? The change in the growth rate? The growth rate itself? [Dale Hurst, United States of America]	Rejected - clear as is.
2-553	2	13	6	13	8	The section title is inaccurate, because it mentions only "Gases" whereas the section also contains a discussion of aerosols - which are not gases. I suggest changing the title to "Short-Lived Greenhouse Gases, Particles, and Other Climate Relevant Gases" [John Ogren, United States of America]	Noted - title changed to near-term climate4 forcers.
2-554	2	13	6	13	9	Elsewhere AR5 describes the role of water vapor as the leading greenhouse gas. But here in Section 2.2 that role is missing. Instead, stratospheric water vapor is discussed, and tropospheric water vapor is relegated to Section 2.5 near the end of the chapter. Yet tropospheric water vapor has far more influence over temperature and weather than stratospheric water vapor. I suggest that words to this effect be included in Section 2.2 with a mention that tropospheric water vapor is discussed later in Section 2.5.6. It is essential for policy makers to understand the dominant role of tropospheric water vapor in regulating temperature, and I suggest that one way to acquaint them is to employ the famous quotation from John Tyndall, who wrote in 1863 that water vapor "is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapour from the air. . . and the sun would rise upon an island held fast in the iron grip of frost." Citation: John Tyndall. On radiation through the Earth's atmosphere. Philosophical Mag. 1863, 4 (25), pp. 200–206. [Forrest Mims, United States of America]	Noted.
2-555	2	13	6			Section 2.2.2: Ch8 has introduced the name Near Term Climate Forcers (NTCF) instead of short-lived greenhouse gases, which may also be used here for consistency. [Jan Fuglestvedt, Norway]	Noted.
2-556	2	13	6			Sec 2.2.2 There seems to be no section on tropospheric water vapour, the earth's main greenhouse gas! [Paul Matthews, United Kingdom]	Tropospheric H2O is in section 2.5.
2-557	2	13	14	13	14	Should the Randel "drop" in concentration after 2000 be explicitly referenced? [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	We feel the current references are appropriate.
2-558	2	13	14	14	17	Srction 2.2.2.1, should mention somewhere current estimates of total water vapour concentration. Only the changes are currently discussed. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	H2O mixing ratio range over Boulder added.
2-559	2	13	14			For all of Section 2.2.2.1, I would write either "stratospheric water vapour" or "stratospheric H2O". [Dietrich Feist, Germany]	Rejected - we want to distinguish vapour from ice crystals.
2-560	2	13	18	13	20	Revision and clarification is needed for the sentence "Stratospheric water vapour mainly enters …". What is the significance of "cold" in "cold tropical tropopause" (is "cold" superfluous here?) and why does the entry of water vapour INTO the stratosphere cause extreme dryness? [Government of Australia]	The 'cold' is significant in that it is the cause of the extreme dryness
2-561	2	13	18			Please rewrite the sentence: "Water vapour mainly enters the stratosphere through the cold tropical tropopause which acts as a cold trap that removes most of the water vapour from the air. As a result, the stratosphere is extremely dry and the annual cycle of stratospheric H2O is large." [Dietrich Feist, Germany]	This sentence has been rewritten.
2-562	2	13	18			That water vapor warms the troposphere and cools the stratosphere is known long before Solomon et al. (2010). Manabe and Strickler (1964 Journal of the Atmospheric science pp361-385) pointed this out in 1964.	Reference added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Government of United States of America]	
2-563	2	13	19	13	20	"enters across the cold but seasonally varying tropical tropopause, causing extreme dryness and a large annual cycle in lower stratospheric H2O" [Dale Hurst, United States of America]	This sentence has been rewritten.
2-564	2	13	19	13	21	The sentence that spans these lines would benefit from a rewrite. Firstly, it would be best to start "Water vapour enters the stratosphere across the cold …" as the water vapour is not strictly "stratospheric" until it has actually crossed the tropopause. Secondly, annual cycles abound in water vapour in the stratosphere. One reason concerns annual variations in the temperature and location of where air enters via the tropical tropopause. But there are others due to the annual cycle of dehydration over the Antarctic, the annual cycle of descent of air moistened by methane oxidation in the polar winter vortex, the descent of dryer air from the mesosphere at higher levels in the winter vortex and perhaps more. As what goes in has to come out eventually, the interannual variability in how air leaves the stratosphere and re-enters the troposphere might also be discussed. [Adrian Simmons, United Kingdom]	This sentence has been rewritten.
2-565	2	13	19			mention annual cycle is due to the annual cycle in cp temperature. [Karen Rosenlof, United States of America]	This has been noted.
2-566	2	13	23	8	25	This preview of AR5 results seems incomplete since the real conclusion is that there has been no net change since 1992. Perhaps add “and, overall, no net increase since 1992”. [Ileana Bladé, Spain]	This sentence has been rewritten.
2-567	2	13	24	8	24	The 2000-2001 was already noted in AR4 (line 41) but here this result is stated as a new development. [Ileana Bladé, Spain]	This sentence has been rewritten.
2-568	2	13	24	13	24	"and a subsequent net increase since 2005 that has been observed by" [Dale Hurst, United States of America]	This sentence has been rewritten.
2-569	2	13	27			There are also long-term records of stratospheric H2O from ground-based microwave radiometers at Table Mountain, Mauna Loa and Lauder, NZ that cover more than one solar cycle. Check Nedoluha, G. E., R. M. Gomez, B. C. Hicks, J. E. Wrotny, C. Boone, and A. Lambert (2009), Water vapor measurements in the mesosphere from Mauna Loa over solar cycle 23, J. Geophys. Res., 114, D23303, doi:10.1029/2009JD012504. [Dietrich Feist, Germany]	These measurements primarily focus on mesospheric water vapour, not stratospheric water vapour (the focus of this section).
2-570	2	13	28	13	28	also include: Hurst et al. (2011) and Oltmans, S.J., Vömel, H., Hofmann, D.J., Rosenlof, K.H. and Kley, D., 2000. The increase in stratospheric water vapor from balloonborne, frostpoint hygrometer measurements at Washington, D.C., and Boulder, Colorado. Geophysical Research Letters, 27(21). [Dale Hurst, United States of America]	Rejected - Hurst referenced below and Oltmans not used in assessment.
2-571	2	13	31	13	31	Literature reference "Russell et al., 1993" is in capital letters [Birgit Hassler, United States of America]	Editorial - editing will occur before publication.
2-572	2	13	31			'RUSSELL' is all caps [Michael Coffey, United States of America]	Editorial - editing will occur before publication.
2-573	2	13	32	13	32	"MIPAS (2002-2012: Michelson Interferometer for Passive Atmospheric Sounding" - conform to the style used for SAGE, HALOE and MLS [Dale Hurst, United States of America]	Noted - text modified.
2-574	2	13	34	13	34	is there a reference for the attribution of differences between the satellite data sets to the issue of vertical resolution? [Rolf Müller, Germany]	No, only unpublished comparison results.
2-575	2	13	35			diffs in vertical resolution do not cause the offsets between haloe and mls..this is not clear in the text. [Karen Rosenlof, United States of America]	Text modified.
2-576	2	13	36	13	36	current version of HALOE is V19 and not V20 as stated here [Rolf Müller, Germany]	Version 20 available and used.
2-577	2	13	37	13	37	Change "adjusted" to "removed". The language around adjustments needs to be clear that this is about the "removal" of artificial signatures and changes, rather than a simply "adjustment" which carries particular implications and interpretations. [Government of Australia]	Text modified.
2-578	2	13	37			change can to may. [Karen Rosenlof, United States of America]	Rejected.
2-579	2	13	39	13	52	the discussion here emphasises the importance of tropical tropopuase temperatures; I do not think this discussion is entirely consistent with chapter 8, p. 24, sec. 8.3.3.3. where oly changes in 'dynamics' are	The Chapter 8 discussion focuses on causes of tropical tropopause temperature changes, while the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						mentioned. [Rolf Müller, Germany]	discussion here focuses on the coupling of temperatures to water vapour.
2-580	2	13	40	13	40	What are these effects linked to the QBO? Can you expand on this here or point to other chapters/sections that describe this in more detail? [Kate Willett, United Kingdom]	The QBO influences tropical tropopause temperatures. Text modified to clarify this point.
2-581	2	13	41	13	42	"yet there was only a small net change from 1992 to 2011." [Dale Hurst, United States of America]	Noted - text modified.
2-582	2	13	41	13	42	Is the sentence "Variability during 2001–2011 was large yet there were small changes from 1992 through 2011."? Or should the last phrase read "from 1992 through 2001"?. [David Parrish, United States of America]	Correct as is.
2-583	2	13	41	13	42	Variability and changes are normally interchangable words, so they can't be both large and small. [Dian Seidel, United States of America]	Noted - modified to net change.
2-584	2	13	44	13	52	In line 44/45 it is stated that there is 'reasonable understanding' in lines 51/52 it is stated 'cannot be explained' both statements refer to tropical tropopuase temperatures – I think these two statements are somewhat contradictory [Rolf Müller, Germany]	L44/45 describe short-term changes and L51/52 long-term changes.
2-585	2	13	45	13	45	"Boulder balloon measurements (since 1980) - because (1980-2011) infers the measurements have stopped [Dale Hurst, United States of America]	Rejected - this only refers to the period we are discussing in AR5.
2-586	2	13	46	13	46	"updated and reanalyzed (Scherer et al., 2008; Hurst et al., 2011)," [Dale Hurst, United States of America]	Noted - text changed.
2-587	2	13	48	13	48	"is good for the period since 1988" - this is what's shown in Figure 2.5 (bottom) [Dale Hurst, United States of America]	Text changed to 1998.
2-588	2	13	49	13	52	If only 30% of the trend is understood (and 70% is not), what is the reader to make of the statement that the 70% is not explained by one potential factor? Should other possible factors be listed? Or should this material be removed, because it's not very informative?4 [Dian Seidel, United States of America]	The cause of the long-term (1980-2010) changes inferred from the Boulder data are unknown. Text has been modified to indicate this.
2-589	2	13	50	13	50	(Hurst et al., 2011; [Dale Hurst, United States of America]	EN error.
2-590	2	13	51	8	51	There seems to be an apparent contradiction between this statement and the earlier statement (line 43) that the observed interannual water vapor variations are closely linked to variations in tropical temperatures. Perhaps add “despite the strong link on interannual timescales” (in parenthesis). [Ileana Bladé, Spain]	Rejected - this statement refers to the changes during 1980-2010 inferred from the Boulder balloon data. The previous statement refers to the satellite data for 1992-2011.
2-591	2	13	54	13	54	in situ is (correctly) italicised here, but not elsewhere in the chapter. Please check for consistency [Peter Burt, United Kingdom]	Editorial - editing will occur before publication.
2-592	2	13	54	13	57	The point of this isolated paragraph is not clear. If this is a technical comment, should it not go next to earlier references about instrument discrepancies (line 33). If this caveat is important for the interpretation of results (and thus follow results), this should be explicitly stated and explained. [Ileana Bladé, Spain]	This paragraph moved to paragraph above.
2-593	2	13	54		57	Concerning the discussion of in situ water vapor measurements, the authors should consider acknowledging earlier findings such as those at JPL by C. Webster et al., R. May et al., especially for high altitude H2O mixing ratio measurements using IR spectroscopy. [Government of United States of America]	Rejected - this assessment highlights new work since the last assessment.
2-594	2	13	55	13	55	Reference should be made to Vomel et al 2007 b as well as a (i.e. Vomel et al., 2007 a,b). [Government of Australia]	Both are referenced.
2-595	2	14	2			What is the variability of H2O in terms of the standard deviation of de-seasonalized anomalies? [Government of United States of America]	Rejected - shown graphically in Figure 2.5.
2-596	2	14	3	14	3	"and a net increase since 2005," [Dale Hurst, United States of America]	Rejected - clear as is.
2-597	2	14	3	14	3	The step like change in 2000 and increase from 2005 is stated three times in this section (1st paragraph, 3rd and last). I like the overall summary at the end but perhaps it doesn't need to be repeated so many times? [Kate Willett, United Kingdom]	First statement has been removed.
2-598	2	14	5			annual and qbo variations well understood, long term trend is not. [Karen Rosenlof, United States of America]	This statement refers to the satellite record (1992-

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							2010), when there are no long-term trends.
2-599	2	14	7	14	7	It is not immediately clear that these are the same balloon measurements referred to in the previous sentence. Suggest changing to "The balloon measurements indicate …" [Ileana Bladé, Spain]	Rejected - clear as is.
2-600	2	14	7	14	7	a discrepancy exists -> discrepancies exist [Ileana Bladé, Spain]	Noted - text modified.
2-601	2	14	7	14	7	Please give dates for 'long-term' [Peter Burt, United Kingdom]	Rejected - dates are given.
2-602	2	14	7	14	7	"from 1998 to present, but a discrepancy exists for changes during 1992-1998." [Dale Hurst, United States of America]	Text modified.
2-603	2	14	8	14	10	It would be good to mention a more complete range of factors affecting stratospheric humidity such as noted above in view of the comment that the long-term increases over Boulder cannot be explained by tropical tropopause temperatures and methane oxidation. [Adrian Simmons, United Kingdom]	Changes inferred from the Boulder measurements during 1980-2010 cannot be explained by any known factors. Text has been modified.
2-604	2	14	9	14	9	change 'although' to 'but' [Rolf Müller, Germany]	Noted- -text changed.
2-605	2	14	14	14	14	"(updated from Randel (2010))." - fix parenthesis [Dale Hurst, United States of America]	Noted - fixed.
2-606	2	14	21	14	21	The earlier section on stratospheric H20 included an explanation of why changes in stratospheric H20 are important for temperature and climate. An equivalent sentence here for ozone would be welcome. [Ileana Bladé, Spain]	considered. Text changed
2-607	2	14	21	14	22	The description of radiative forcing in AR4 doesn't fit in to this observations chapter, particularly as it was a model-based calculation. [William Collins, United Kingdom of Great Britain & Northern Ireland]	The RF in AR4 was given for context, it is to give some context of the following discussion. The RF given now point to Chapter 8 for further discussion.
2-608	2	14	21	14	22	"radiative forcing of stratospheric ozone changes between" [Dale Hurst, United States of America]	Corrected as suggested
2-609	2	14	21	14	49	Section 2.2.2.2 Stratospheric ozone. The authors do not cite any of the several papers that claim (perhaps controversially) to detect significant ozone recovery: for example Yang et al., J. Geophys. Res., 2008; Salby et al., Geophys. Res. Letts., 2011. [Government of Australia]	There are different stages of recovery. We tried to avoid discussion about these stages to keep the section short. It is a controversial issue as you can see even from some comments below. In any case there is no recovery in a sense of return to pre-1980 levels.
2-610	2	14	21	14	49	Is this a good place for discussion of implications for the ozone layer in the future or reference to other sections that deal with this (Chapters 8, 11 and 12)? How long would it take to recover at the current rate for the Antarctic and Arctic? [European Union]	Unfortunately this measurement section can not discuss implications for future ozone. Some foreward referencing is now included.
2-611	2	14	21			Section 2.2.2.2 - Suggest a brief analysis of the response in ozone that was predicted based on the reductions set forth in the Montreal Protocol. [Government of United States of America]	Unfortunately this measurement section can not discuss implications for future ozone. Some foreward referencing is now included.
2-612	2	14	26	14	30	It would be more clear to condense this information by saying "…. and 2.5% for 60ºS-60ºN, with changes occurring mostly outside the tropics, particularly the SH where the current extratropical (30ºS-60ºS) mean values are 6% below the 1964-1980 average, compared to 3.5% for the NH extratropics". [Ileana Bladé, Spain]	Corrected as suggested
2-613	2	14	26	14	32	The results quoted in this paragraph are confusing. Figure 2.6 begins in 1970 but the reference period begins in 1964. The text says that there are no statistically significant trends in the tropics but is not explicitly stated whether the trends in the other regions or for the global mean are significant. In panel (a) "global" denotes the region 60ºN-60ºS but the text makes a distinction between those two regions (line 28). [Ileana Bladé, Spain]	The presented results are from two sources: plots are from Weber et al., and % deviation from the 1964-1980 means are from Douglass et al. We added reference to Douglass et al. to avoid the confusion.
2-614	2	14	27	14	28	"about 3.5 and 2.5% below the 1964–1980 average for the entire globe and 60°S–60°N, respectively." [Dale Hurst, United States of America]	Corrected as suggested
2-615	2	14	28	14	28	Perhaps no need to mention "statistically significant," as such a test is NOT applied to trends in other latitude bands. [Government of Australia]	Corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-616	2	14	30	14	31	"In the NH, the 1993 minimum of about 5.5% below the 1964–1980 average was primarily caused" [Dale Hurst, United States of America]	Corrected as suggested
2-617	2	14	34	14	35	mention the sensitivity of (in particular upper) stratospheric ozone to stratospheric temperature change and this constitutes an important feedback mechanism (super recovery). [Rolf Müller, Germany]	Unfortunately such discussionis beyond the scope of Chapter 2, but very interesting.
2-618	2	14	34	14	39	Again, the information in this paragraph could be condensed since the variations appear to follow the same behavior in both upper and lower stratosphere so there is no need to repeat that there was a strong decline followed by stabilization. [Ileana Bladé, Spain]	It is better to specify particular regions since there are no significant trends near 30 km, i.e. between them.
2-619	2	14	35	14	35	I don't believe the acronym ODS is defined in this chapter. [Ileana Bladé, Spain]	"ODS" is not used in the revised text
2-620	2	14	35	14	35	change 'ODSs' to 'ozone depleting substances' [Rolf Müller, Germany]	"ODS" is not used in the revised text
2-621	2	14	36	14	36	For completeness, can an explanation be ventured for the ozone loss in the lower stratosphere (an explanation is given for the loss in the upper stratosphere). [Ileana Bladé, Spain]	There is no such simple explanation for the ozone loss in the lower stratosphere because several factors contribute to it.
2-622	2	14	41	14	44	Why aren't the polar trends in Fig. 2.6e discussed ? From line 27 it is implied that the polar trends make a large contribution but it is not explicitly discussed. [Ileana Bladé, Spain]	Changes in the polar regions are evident from Figure 2.6e. No additional discussion is included to keep the section short.
2-623	2	14	41	14	44	It should be stated here that the by far strongest ozone loss in the stratosphere occurs in austral spring over Antarctica (ozone hole) and that there are no signs of a significant recovery here. I would also be good to point to chapters 11, 12, and 14 where the consequences of the ozone hole for southern hemisphere climate are discussed. [Rolf Müller, Germany]	Edited as suggested
2-624	2	14	44	14	44	Can anymore be said about the very low levels of Arctic and Antarctic ozone in the respective springtimes of 2011? Any expectation of the low ozone persisting over the Arctic? Is there a good discussion anywhere else in WG1 of the combined factors that lead to such low values occurring? [Kate Willett, United Kingdom]	We added the key message that the amount of ozone destroyed in the Arctic in 2011 is comparable to that in the Antarctic
2-625	2	14	46	14	48	Which latitude ranges do these statements pertain to? Certainly there was no "partial recovery to ~2000" in the SH (Fig. 2.6 panel d) [Dale Hurst, United States of America]	This sentence is about global ozone. The text was edited to avoid confusion
2-626	2	14	46			Is this a global trend? [Government of United States of America]	The number was corrected to 3.5% to reflect the global values
2-627	2	14	47	14	47	The 2.5% value quoted is for 60ºS-60ºN only. [Ileana Bladé, Spain]	The number was corrected to 3.5% to reflect the global values
2-628	2	15	1			Section 2.2.2.3: Perhaps some mention should be made that the vertical distribution of O3 in the troposphere is important for radiative forcing (with a nod to Chapter 8). [Government of United States of America]	This is a very reasonable suggestion but given the very limted space radiative forcing issues are deferred to Chapter 8.
2-629	2	15	3	15	3	Again, I note some inconsistency in the fact that a little background information is provided here for tropospheric ozone but the same was not done for the stratospheric ozone section which starts off quite abruptly. [Ileana Bladé, Spain]	Due space restrictions we have condensed somewhat the introductory sentence to tropospheric ozone.
2-630	2	15	3	15	4	This sentence isn't quite right. How about "In the troposphere, ozone is short-lived. Its main sources are input from the stratosphere or in situ production from precursors gases and sunlight." [William Collins, United Kingdom of Great Britain & Northern Ireland]	While ozone is relatively long-lived in the stratosphere, the qualifying term "troposphierc ozone' is consistent with being short-lived.
2-631	2	15	3	15	31	In this section, generally, I am concerned about the lack of supporting references: lots of statements but no sources! [Peter Burt, United Kingdom]	To save space references were originally limited to the supporting material in Table 2.A.2. Some references are now included in the main text, and Table 2.A.2 introduced
2-632	2	15	3	15	31	Somewhere in this section there needs to be a discussion about trends in background/baseline ozone which is more relevant to climate than the individual regional changes discussed later. It is alluded to obliquely in this paragraph, but there needs to be a stronger statement: "We assess an overall increase/decrease/can't-tell in	Table 2.A.2. now states: "To understand ozone trends in air masses that are representative of regional or baseline conditions, measurements are

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the northern/southern hemisphere background ozone observations". [William Collins, United Kingdom of Great Britain & Northern Ireland]	from rural sites. However in East Asia data are so limited that trends are also assessed in urban areas." The section's summary statement now indicates the overall global ozone trend since the 1970s: "measurements since the 1970s indicate surface and free tropospheric ozone has generally increased around the world, however the magnitudes of the trends vary (medium evidence, medium agreement: Medium Confidence). "
2-633	2	15	4	15	4	The term "climate forcer" seems new. We had "forcing" before, and in AR5 the term "driver" is introduced, which seems like a nice term. Why force another term on the reader? [Dian Seidel, United States of America]	Climate forcer has been replaced with greenhouse gas.
2-634	2	15	5	15	6	"its" has no contextual connection - "and the few-week lifetime of tropospheric ozone make global trend assessments challenging." [Dale Hurst, United States of America]	The sentence has been revised to remove this ambiguity.
2-635	2	15	7	15	7	It is not clear exactly what "regional and seasonal long-term ozone trends" refers to here [Dale Hurst, United States of America]	This sentence has been deleted.
2-636	2	15	9	15	10	The sentence "New time-series for Eastern Asia are now available" should be omitted in this paragraph or further developed including if available other new time series [Moira Evelina Doyle, Argentina]	This statement has been deleted.
2-637	2	15	10	15	10	Please give reference(s) [Peter Burt, United Kingdom]	References have been added.
2-638	2	15	12		21	Regarding columnar ozone over India, Tandon and Attri (2011) have reported the trends which need to be included. Ref:Tandon A and Attri A K. 2011. Trends in total ozone column over India: 1979–2008. Atmospheric Environment, 45, 648–1654 [Umesh Kulshrestha, India]	We thank the referee for pointing out this reference. The Tandon and Attri study focuses only on total column ozone, 90% of which is stratospheric. The dominance of stratospheric ozone in total column ozone prevents any conclusions from being drawn regarding trends or variability in tropospheric column ozone. Because this section deals only with tropospheric ozone we have not included the reference to Tandon and Attri.
2-639	2	15	12			The observations mentioned in these first two sentences would benefit from references. . [Government of United States of America]	References have been added.
2-640	2	15	12			The acronym TCO for "tropospheric colomn ozone" should be avoided, to prevent confusion with "total column ozone" for which the abbreviation may also be used. [Adrian Simmons, United Kingdom]	TCO has been removed
2-641	2	15	16	15	16	of → over [Ileana Bladé, Spain]	fixed
2-642	2	15	21	15	21	After “China”, append “Using Satellite data, Reveadekar and Patil (2009) indicate that the total column ozone over north India during 1997-2005 has recovered as compared to the 1979-88, that is, the period prior to implementation of montreal protocol (1979-88) [Government of India]	The Revadekar and Patil study focuses only on total column ozone, 90% of which is stratospheric. The dominance of stratospheric ozone in total column ozone prevents any conclusions from being drawn regarding trends or variability in tropospheric column ozone. Because this section deals only with tropospheric ozone we have not included the reference to Revadekar and Patil.
2-643	2	15	23			The document expresses the ozone amount in two ways: concentration and column amount. Does this discussion of tropospheric ozone refer to concentration, column amount or both? [Government of United States of America]	The units are ppb (volumetric mixing ratio) and this statemetn has been clarifed.
2-644	2	15	28	15	31	This section doesn't specify what sort of emissions are at issue, so the reader might misinterpret it to mean emissions of ozone. [Dian Seidel, United States of America]	This emissions statement has been deleted.
2-645	2	15	30	15	45	This is one example of seemingly lack of order in presenting results and where connecting words would really	This sentence has been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						help the flow of ideas. For instance the sentence that begins with “In east Asia ... ” and refers to a location where large trends follow large emissions seems wrongly placed after a sentence stating that the two do not always go together. [Ileana Bladé, Spain]	
2-646	2	15	33			Figure 2.7 should be replaced with updated figure sent to Frank Dentener, Ed Dlugokencky and Owen Cooper [David Parrish, United States of America]	Updated.
2-647	2	15	39	15	2	This paragraph needs to be clearer when talking about emissions what these are emissions of. It reads as if they are emissions of ozone. [William Collins, United Kingdom of Great Britain & Northern Ireland]	This sentence has been deleted.
2-648	2	15	39	15	39	It would be very informative to identify which sites in Figure 2.7 have no statistically significant trends [Dale Hurst, United States of America]	The sites with statistically significant trends are now identifed in the figue caption.
2-649	2	15	39	15	41	The first 2 sentences are inconsistent with the rest of the paragraph. Which sites are these and where are they located in general? What is meant by "significantly increasing ozone are not always associated with regional increases in anthropogenic emissions"? Presumably this alludes to inter-contintental transport of ozone and should be discussed as such with refererence to, for example, the HTAP (2010) report or Parrish et al. (2012). [Katharine Law, France]	These 2 sentences have b een deleted.
2-650	2	15	41	15	42	"In East Asia, …… show increasing ozone." References should be cited to support this statement. [Xiaobin Xu, China]	References have been added.
2-651	2	15	42	15	45	This sentence is unclear to me. [Ingeborg Levin, Germany]	This sentence has been deleted.
2-652	2	15	46	15	46	"Ozone increased in Europe from the 1950s and 1970s until approximately 2000." This sentence needs clarification because "from the 1950s and 1970s" is very vague. [John McLean, Australia]	This sentence has been clarified to indicate that long term records that began in the 1950s or the 1970s show increasing ozone though the end of the 20th century.
2-653	2	15	46	15	47	This sentence is also unclear, as the increase starts from two different dates. [Ingeborg Levin, Germany]	This sentence has been clarified to indicate that long term records that began in the 1950s or the 1970s show increasing ozone though the end of the 20th century.
2-654	2	15	46	15	47	The sentence "Ozone increased in Europe from the 1950s and 1970s until approximately 2000" is difficult to interpret. Does it mean simply that ozone inceased from the 1950s to about 2000? Or that ozone increased from the 1950s to some unspecified date and then increased again from the 1970s until about 2000? [Adrian Simmons, United Kingdom]	This sentence has been clarified to indicate that long term records that began in the 1950s or the 1970s show increasing ozone though the end of the 20th century.
2-655	2	15	46	15	49	These two sentences seem a little conflicting - perhaps just switching them round would make more sense. I think you mean observed ozone abundance in the first instance and then emission records in the second instance and so despite emissions tailing off, tropospheric ozone continued to increase? [Kate Willett, United Kingdom]	This section has been completely re-written and the confusing statements have been fixed.
2-656	2	15	54	15	55	The decreases in surface ozone in Europe are first described as inconsistent with the decreased emissions (after 1990 – line 48) and then as consistent with decreased emissions (after 2000). Perhaps this could be referred to only once and described as an unexplained lag/delay between decreased emissions and decreased concentrations ? [Ileana Bladé, Spain]	This section has been completely re-written and the confusing statements have been fixed.
2-657	2	15	54	15	55	The first 2 sentences are misleading - depends on location in Europe and also season - refer to Colette et al. (2011) and Wilson et al. (2012) for example. [Katharine Law, France]	This section has been completely re-written and the confusing statements have been fixed.
2-658	2	16	5			Section 2.2.2.4: I suggest merging this section with 2.2.2.3. It doesn't make sense to discuss the ozone precursors on their own without tying it in to the changes in ozone (or methane lifetime). If it is to be kept separate then at the very least the consequent implications for ozone and methane need to be discussed here. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Rejected
2-659	2	16	7	16	7	Change "(NOx = NO + NO2)" to "NOx (NO + NO2)" [Government of Brazil]	done

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-660	2	16	7	17	7	Misplaced bracket, should be after '=' [Peter Burt, United Kingdom]	done
2-661	2	16	7			The appearance of the acronym VOC here seems to be the first occurrence in the chapter, so it should be spelt out what VOCs are here. [Adrian Simmons, United Kingdom]	VOC is now explained in the text
2-662	2	16	8	16	9	Space should not be used as an excuse for leaving something out. The authors should make a judgement as to what is important to include and what is not. [William Collins, United Kingdom of Great Britain & Northern Ireland]	There is an small additional section summarizing some key publications on VOC trends
2-663	2	16	8	16	9	Are Volatile Organic Compounds (VOCs) important for policy? If so perhaps query the bit that says 'Due to space limitations, trends in VOCs have not been assessed in this section.' It might be good to link here to a section that does cover VOCs or if its very important request that it is included here. [European Union]	There is an small additional section summarizing some key publications on VOC trends
2-664	2	16	11	16	20	It would be informative to refer to the work of Petrenko et al (2012) on CO trends from firn air in Greenland. That study also suggests that historical CO emissions used by models are too low and that they peaked earlier in 1970-80s. [Katharine Law, France]	Unfortunately at the time of writing the paper was not yet available in the peer reviewed literature.
2-665	2	16	13	16	17	Please explain acronyms: MOPITT, AIRS, TES, IASI [Ingeborg Levin, Germany]	There will be a table with explanations of ACRONYMS
2-666	2	16	16	16	18	citations for Worden et al., 2012 a & b have the same reference. (remove a & b [Helen Worden, United States of America]	Corrected
2-667	2	16	18	16	18	“magnitude of trends remains uncertain due to the presence of instrument drifts” [Helen Worden, United States of America]	This suggestion is accepted.
2-668	2	16	18	16	18	Worden et al., 2012 show that the most recent version of MOPITT data (V5) accounts for MOPITT instrument drift, but this could say: [Helen Worden, United States of America]	This suggestion is accepted.
2-669	2	16	18	16	18	“magnitude of trends remains uncertain due to measurement bias between MOPITT and AIRS” [Helen Worden, United States of America]	This suggestion is accepted.
2-670	2	16	18	16	19	'Small CO decreases observed in the NOAA and AGAGE networks' - this important statement needs appropriate reference cited. [Government of Australia]	The appropriate references were erroneously not included in the Appendix, but will be in the final draft
2-671	2	16	18	16	20	I could not find this CO discussion in Appendix 2.A. Can you refer to a Table or Figure? [Ronald Prinn, United States of America]	The appropriate references were erroneously not included in the Appendix, but will be in the final draft
2-672	2	16	19	16	19	Needs a paragraph discussing observationally determined scattering efficiencies per the method of Warren White:http://vista.cira.colostate.edu/improve/publications/Principle/NAPAP_SOS/Low%20Res/Chapter4.pdf. This is particularly important for the main anthropogenic aerosol substances, e.g., sulfates. This will provide an important set of data to compare to models. A table of data is needed also. [Robert Charlson, United States of America]	Chapter 2 is mainly about trends and does not discuss methodological issues. In Ch. 7 optical properties are further discussed optics but do not go into mass scattering efficiency (was covered already in TAR).
2-673	2	16	22	16	22	Replace "NOx" with "NO2" [William Collins, United Kingdom of Great Britain & Northern Ireland]	Reject: the statement is correct (although measurements pertain to NO2)
2-674	2	16	22	16	26	Sources of NOx are not mentioned. It should be made clear that tropospheric NOx not only originates from combustion processes but that a significant source for atmospheric Nox are emissions from N fertilized soils (or from soils naturally rich in N or secondarily rich in N due to atmospheric N deposition) [European Union]	Chapter 2 focusses on the measured trends, detailed discussion on the contributing sources to NOx will be given in Chapter 6.
2-675	2	16	22			What is the source of NOx over China during these periods? [Government of United States of America]	Chapter 2 focusses on the measured trends, detailed discussion on the contributing sources will be given in Chapter 6. Obviously a large part of the Nox is related to combustion processes, although there have been reports on large contributions from agricultural fields as well, but can not be discussed in Chapter 2.
2-676	2	16	29			What is the key driver behind the, "...strong upward trend" in NO2 over Central Eastern China? [Government of United States of America]	Chapter 2 focusses on the measured trends, detailed discussion on the contributing sources to NOx will be given in Chapter 6.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-677	2	16	30			Fine to mention "Western Europe and Poland", but this begs the question as to what the situation was for the rest of Eastern Europe? Can anything be said on this to complement the reference to Poland. [Adrian Simmons, United Kingdom]	Figure 2.8 shows the changes relative to 1996 in satellite derived tropospheric NO2 columns, with a strong upward trend over Central Eastern China and an overall downward trend in Japan, Europe and the US. All decreases are variable between years and seem to have stabilized since 2009. NO2 reductions in the US are very pronounced after 2004, related to differences in effectiveness of NOx emission abatements in the US and Europe and also to changes in atmospheric chemistry of NOx (Russell et al., 2010)variable between years and seem to have stabilized since 2009. NO2 reductions in the US are very pronounced after 2004, related to differences in effectiveness of NOx emission abatements in the US and Europe.
2-678	2	16	32	16	33	Consider including an additional reference where satellite data is used to observe trends in NOx decreases: A.R. Russell, L.C. Valin, E.J. Buscela, M.O. Wenig and R.C. Cohen, Space-based Constraints on Spatial and Temporal Patterns of NOx Emissions in California, 2005-2008, Env. Sci. & Tech. 44, 3608-3615, 2010.Consider including an additional reference where satellite data is used to observe trends in NOx decreases: A.R. Russell, L.C. Valin, E.J. Buscela, M.O. Wenig and R.C. Cohen, Space-based Constraints on Spatial and Temporal Patterns of NOx Emissions in California, 2005-2008, Env. Sci. & Tech. 44, 3608-3615, 2010. [Government of United States of America]	We now include reference to this paper.
2-679	2	16	34	16	34	Stop the sentence after 29% yr–1 (1996–2006). Then, Replace the remaining part of the sentence i.e. ",while moderate decreases up 35 to 7% yr–1 (1996–2006) are reported for North America and Europe" by "However, it is to be noted that over South Asia, the increasing trend for the same period, based on a satellite data analysis, has been documented by Ghude et al. (2008) to be only about 1.74%. Further, Ghude et al (2009), by analyzing satellite data, find that absolute concentration of the NO2 over south Asia is significantly lower than developed countries like Europe and North America. Moderate decreases up to 7% yr–1 (1996–2006) are reported for North America and Europe." [Government of India]	This is a good suggestion, but due to space restricitions we could not consider the suggested phrase.
2-680	2	16	40	16	40	Citation style wrong [Peter Burt, United Kingdom]	The citation style is due to issues with EndNote which wil be solved in final version.
2-681	2	16	42	16	42	What are the global averaged trends in optical depth, perhaps a time series plot would be useful. [Government of Australia]	Overviews of global trends are given in Hsu et al; however given our sceptical approach to the validity of global trends, we refrain from showing this plot
2-682	2	16	42			Ch 2 Section 2.2.3 (Aerosols) nowhere seems to mention aerosol data acquired from snow fields, ice cores, sediments etc. that extend the record back in time. Those data (e.g. Windom's early work) are an important part of the observational record that provide insight into aerosol composition and abundance from the pre-satellite era. [Government of United States of America]	We refer to Chapter 8 which will include some evaluation of icecores.
2-683	2	16	42			In order to reduce Ch2 in length you might want to consider moving parts of this section on aerosols as well as of section 2.5.7 on clouds to Chapter 7. Adding more cross-Chapter references might also help shortening parts of Ch2. [Thomas Stocker/ WGI TSU, Switzerland]	This was discussed with CLA's of Chapter 7, but it turned out to be better to leave it in Chapter 2. This seems to be an issue that was somewhat misconcepted during the scoping of the report
2-684	2	16	44	16	45	Chapter 7 also does forcing, chapter 8 does tome evolution of the forcing and future forcing [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	We have corrected this.
2-685	2	16	44	16	51	Partitioning of natural aerosols from anthropogenic ones is highly essential for meaningful study of aerosol impacts on climate and climate change. Antropogenic aerosols are mainly confined to polluted regions.	We agree with the reviewer comments. These issues are discussed in depth in Chapter 7. Chapter 7 also

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Observations over clean, high-altitude stations during calm or weak-wind conditions represent reasonably natural aerosols which can be regarded as base-line data for seggregating time-series anthropogenic 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. Discriminating natural aerosols from anthropogenic origin, based on satellite-derived Angstrom coefficients, needs comparison with in-situ measurements of size seggregated aerosols over locations of known environments coupled with meteorology. [Panuganti, C.S. Devara, India]	emphasises more that we need to know what the pre-industrial aerosol is.
2-686	2	16	44	16	51	Is it strictly appropriate to talk about anthropogenic and natural aerosols in the way that is done here? If land-use change causes a change in levels of dust in the atmosphere, is this dust really a "natural" aerosol? Likewise smoke from fires that may be exacerbated by anthropogenic changes in land management and (indirectly) temperature, or ignited by humans. [Adrian Simmons, United Kingdom]	This is correct- and there are even more issues than mentioned by the reviewer. In Chapter 2 there is no space for such discussion, howver Chapter 7 is going in length into these issues.
2-687	2	16	44			Section focuses too much on trend of AOD and not on actual values and not on anthropogenic contribution to AOD and not on forcing. How well is anthro contrib to AOD known? How well does it have to be known? For a sensitivity of TOA forcing to AOD of roughly 30 W m-2 per tau (24 hr avg, equinox, low and mid latitudes) in cloud free conditions (McComiskey et al 08); derate by a factor of 2 for cloud cover to get 15 W m-2 per tau. So the error in tau of ~0.05 indicated in the figure corresponds to 0.75 W m-2. This is substantial in terms of forcing over the industrial period. So the real question is how accurate are satellite products in this context? How accurate is attribution to anthro and inferred forcing? This needs to be explicitly addressed. Quote from Kahn, 2012, explicitly addresses that question and provides an answer and important context (Note that Kahn refers to instantaneous cloud-free forcing; derate by factor of 4 for 24-hr average and 50% cloud cover): Calculations suggest that instantaneous, mid-visible AOD measurement accuracy of about 0.02 is typically required under cloud-free conditions to constrain DARF to approximately 1 Wm-2 (McComiskey et al. 2008; CCSP 2009), whereas the corresponding uncertainties in the current global AOD products from MISR and MODIS are 0.03 or larger over dark water, and 0.05 or larger over land (Kahn et al. 2010; Levy et al. 2010; Remer et al. 2005). Theoretical DARF sensitivity analysis identified particle single-scattering albedo (SSA) as the other leading factor in most situations, especially important for determining radiative forcing over land surfaces, and requiring an instantaneous constraint of about 0.02, though varying with other factors, particularly AOD and surface albedo (McComiskey et al. 2008). Kahn RA. (2012) Reducing the Uncertainties in Direct Aerosol Radiative Forcing. Surveys in Geophysics 33:3-4, 701-721. DOI 10.1007/s10712-011-9153-z Kahn RA, Gaitley BJ, Garay MJ, Diner DJ, Eck T, Smirnov A, Holben BN (2010) Multiangle Imaging SpectroRadiometer global aerosol product assessment by comparison with the Aerosol Robotic Network. J Geophys Res 115:D23209. doi:10.1029/2010JD014601 Levy RC, Remer LA, Kleidman RG, Mattoo S, Ichoku C, Kahn R, Eck TF (2010) Global evaluation of the Collection 5 MODIS dark-target aerosol products over land. Atmos Chem Phys 10:10399–10420. doi:10.5194/acp-10-10399-2010 Remer LA, Kaufman YJ, Tanre D, Mattoo S, Chu DA, Martins JV, Li R–R, Ichoku C, Levy RC, Kleidman RG, Eck TF, Vermote E, Holben BN (2005) The MODIS aerosol algorithm, products, and validation. J Atmos Sci 62:947–973 McComiskey, A., S. E. Schwartz, B. Schmid, H. Guan, E. R. Lewis, P. Ricchiazzi, and J. A. Ogren (2008), Direct aerosol forcing: Calculation from observables and sensitivities to inputs, J. Geophys. Res., 113, D09202, doi:10.1029/2007JD009170 [Stephen E Schwartz, United States of America]	Due to the division of themes- the issues mentioned by the reviewer are more thoroughly discussed in Chapter 7. We agree that the division is rather artificial.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-688	2	16	46	16	47	'Short life time' should probably refer to "tropospheric aerosols" rather than "aerosols". [Government of Australia]	Corrected as suggested
2-689	2	16	49	16	49	Reference to "natural" is confusing. Most aerosols are naturally occurring, but what is unnatural in many cases is their elevated concentrations and production due to human activities. [Government of Australia]	We have rephrased the sentence into: aerosol from natural and anthropogenic sources.
2-690	2	16	53	16	53	Dan Murphy has a MISR AAOD analysis in its 2nd review in Natuture geosci - worth considering a cite maybe [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	The references is more about Radiative forcing, and relevant to Chapter 7.
2-691	2	16		16		Section 2.2.3.1: Are trends of 10-15 years of relevance to climate change? In terms of global radiation budget they probably are since this relates to the fundamental heating rate of Earth. But in terms of aerosol which are spatially and temporally hetrogeneous they are probably not. I would suggest that at present the satellite record of AOD at the largest scales may be useful in confirming and contextualizing the longer-term in situ trends and as such Section 2.2.3.1 could be reduced or incorporated into the following section. [Richard Allan, United Kingdom]	A more extended discussion on how to interpret trends for short records is given in a separate trend box. We have not integrated the sections.
2-692	2	17	1		2	AOD trends can not only be shown for the last 15 years but have been shown from the last 25 years. (see below) [Rolf Philipona, Switzerland]	Thank you for the references which has been taken into account.
2-693	2	17	4	17	4	In the following sentence, "relatively accurately" is both grammatically awkward and imprecise: "AOD can be relatively accurately determined with sun-photometers…." Suggested revision: ""AOD can be accurately determined (uncertainty of 0.01 to 0.02) with sun-photometers…." The best reference (there are many) is: T. Eck , et al. 1999. Wavelength dependence of the optical depth of biomass burning, urban and desert dust aerosols J. Geophys. Res. 104 31333–50. [Forrest Mims, United States of America]	We have followed this suggestion to address the uncertainty of measurements for each type of observation.
2-694	2	17	4	17	17	Developments in data retrieval schemes involving cloud-screening algorithms have improved the availability of data sets for the study of long-term changes and trends. In this context, besides the technological improvements in instrumentation for capturing more finer details of aerosols , networking-cum-intercomparison and accurate ground-truth for satellite retrievals are highly essential. [Panuganti, C.S. Devara, India]	we have added
2-695	2	17	4		10	In this paragraph the authors speak explicitly about the AERONET network which uses sun-photometers at many sites. They are speaking of trends in various parts in the world but they do not say in which time period. Please specify the time period of the AERONET measurements. [Rolf Philipona, Switzerland]	We have in the table caption included some statements on the time period of the measurements- which sometimes included datagaps.
2-696	2	17	4		17	However, nothing is said about AOD measurements that were made before. At the Lindenberg Observatory continuous AOD measurements started in 1986 and several other stations in Germany and in the Alps in the early 1990s. These world longest continuous AOD measurement series show that over Europe AOD decreased by 60% since the mid 1980s. From 1995 to 2005 AOD still declined by 25%. Hence, the measurements show that Europe had a strong decline of AOD already before the year 2000, which goes well together with reports showing the SO2 reduction over Europe. This is important, since this is likely the reason why the AOD decline after 2000 was lower in Europe than over North America. [Rolf Philipona, Switzerland]	We have now made reference to this important dataset.
2-697	2	17	4		17	The results of the AOD trends in Europe were published in a paper by Ruckstuhl et al., in2008. This paper and a corresponding text needs to be added in 2.2.3. Also, I suggest that Figure 1 of the Ruckstuhl paper is shown in the report, because this is a key issue to understand climate change in Europe, with respect to solar brightening (also shown in this paper) and the strongly related rapid increase of temperature in Europe since the 1980s. [Rolf Philipona, Switzerland]	We have now made reference to this important dataset.
2-698	2	17	4		17	Ref: Ruckstuhl, C., Philipona, R., Behrens, K., Collaud Coen, M., Dürr, B., heimo, A., Mätzler, C., Nyeki, S., Ohmura, A., Vuilleumier, L., Weller, M., Wehrli, C., and Zelenka, A., 2008. Aerosol and cloud effects on solar brightening and the recent rapid warming. Geophysical Research Letters, 35, L12708. [Rolf Philipona, Switzerland]	We have now made reference to this important dataset.
2-699	2	17	4			“AOD can be relatively accurate”. What does this statement mean? An alternative approach would be to cite the uncertainty of AOD derived from sun-photometers. [Government of United States of America]	sentence now includes a uncertainty qualifier
2-700	2	17	5			Needn't be cloud free; sufficient to have cloud free path to sun. [Stephen E Schwartz, United States of America]	sentence corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-701	2	17	9			How can a station be "regionally representative"? Please explain criteria [Jan Cermak, Germany]	The meaning is not influenced by local sources, but the sentences has been modified.
2-702	2	17	9			Are you really referring to "increasing" trends (as in a change of rate), or did you mean "positive" trends (referring to an upwards tendency in the parameter)? [Jan Cermak, Germany]	Sentences have been corrected.
2-703	2	17	9			With reference to table 2.2 it must be noted that only six of the trends shown are statistically significant at the 95% level, and several time series are 6 years or shorter. [Jan Cermak, Germany]	This is now mentioned
2-704	2	17	10	17	11	There is a factual error in the statement. It should be some thing like "Time series of spectral AOD measured using a network of solar radiomenters (ARFINET) confirm increasing trends in short-wave AODs over South Asian region over the last two decades (Moorthy et al. 2012)". [K KRISHNA MOORTHY, INDIA]	We have condensed the suggestion of the reviewer in a rephrased sentence
2-705	2	17	11	17	11	The reference of Dani et al (2012) may be included in addition to Krishna Moorthy et al (2012) [Government of India]	We have considered this reference, but we can not have too extensive referencing.
2-706	2	17	12	17	14	It is worth mentioning here the work by "Papadimas, C. D., N. Hatzianastassiou, N. Mihalopoulos, X. Querol, and I. Vardavas (2008), Spatial and temporal variability in aerosol properties over the Mediterranean basin based on 6-year (2000–2006) MODIS data, J. Geophys. Res., 113, D11205, doi:10.1029/2007JD009189." that analyses 6-years of MODIS and AERONET AOD observations over the Mediterranean and attributes AOD trends during summer to anthropogenic emissions changes and during winter to the NAO and its impact on precipitation. [MARIA KANAKIDOU, GREECE]	thank you for the reference. Due to space restrictions and exhaustive literature on the subject it was not necessary to discuss this publicaiton.
2-707	2	17	12			Might add at end of sentence: A detailed study of aerosol optical depth at North Central Oklahoma showed no significant trend over 1993-2007 (0.0097 ± 0.0136 per decade, 1 s.d.), albeit significant year-to-year differences (medians of daily means differing by as much as 50%, 0.10 to 0.15). 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. [Stephen E Schwartz, United States of America]	thank you for the reference. Due to space restrictions and exhaustive literature on the subject it was not necessary to discuss this publicaiton.
2-708	2	17	16	17	17	It is stated that large AOD "trends" are associated with stong "inter-annual variability" related to wild fires and dust emissions. In this case the word "trends" should not be used. The appropriate statement is that there is strong inter-annual variability in AOD associated with inter-annual variability in wildfires and dust emissions. There may also of course be trends in AOD associated with trends in wildfires and dust emissions. See general comment 37 above. [Adrian Simmons, United Kingdom]	A separate box will explain our use of the word trend
2-709	2	17	16			Why would inter-annual variability register as large trends? Please clarify and provide reference. [Jan Cermak, Germany]	A separate box will explain our use of the word trend
2-710	2	17	19	17	20	The following sentence is awkward: "Cloud screened, ground-based solar broadband radiometer measurements provide longer time-records than sub-spectral sun-photometer data, but they are less accurate." First, the phrase, "sub-spectral sun-photometer" is confusing when "spectrally selective" is what is meant. Second, comparing the "accuracy" of sun photometers with that of broadband radiometers is inappropriate, since the two classes of instruments play very different roles. A broadband instrument can very accurately measure the full sky (plus sun) irradiance while a sun photometer cannot. Here is a suggested revision: "Cloud screened, ground-based solar broadband radiometer measurements provide longer time-records than spectrally selective sun-photometer data, but broadband measurements do not permit the aerosol size retrievals available from sun photometer derived AOD at two or more discrete wavelengths." [Forrest Mims, United States of America]	We have utilized part of the suggested sentences. Thank you.
2-711	2	17	26	17	54	Satllites provide data on global scale and over sufficiently longer periods, hence they are very valuable in the assessment of aerosol-climate interaction programs. But in the light of discripancies between the satellite products and ground-based/multi-platform real-time observations, the sensor technology and data retrieval algorithms need to be improved for obtaining reasonable coherence between them. In these exercises, observational methods also need to be supported by laboratory experiments. Accurate measurements of aerosol absorption, surface solar reflection and evaluation of weighting functions with respect to atmospheric	We agree with this statement, which are reflected to a large extent in our revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						pressure and temperature variations will improve the data quality. [Panuganti, C.S. Devara, India]	
2-712	2	17	26		38	can you state whether you feel the trends are reliable. [Karen Rosenlof, United States of America]	We give more more information on certain situation were despite statistically significant trends we still think that these trends are less reliable than expected.
2-713	2	17	26			Discussion of Fig.2.9: Suggest that the explanation for the anomalous negative trend values off Mexico provided in the caption be repeated in the text. This illustrates how end points of time series can drive the trends to unexpected values. The authors might consider a discussion of this process. [Government of United States of America]	We agree with the reviewer- it is very much connected to the use of short timeseries of parameters driven by variability; we have some more discussion on this phenomenon in the text.
2-714	2	17	26			2 sections about trends from satellite observations: the section starts well by explaining advantage (global coverage) and main uncertainty sources (cloud screening and background signal); probably there should be some crosslink to chapter 7. Regional results of analyses using four different satellite products on AOD are described.Two figures (2.9 and 2.10) show maps of 'linear trends', both not very concluding in my opinion, because the treatment of retrieval uncertainties is not clear. Actually, the 3 references of multi-satellite analyses show that it seems too early to come to trend conclusions: from Zhang and Reid 2010 we learn that the Terra MODIS DA product has an AOD which is about 25% smaller than AOD of the other products, due to a better cloud clearring. From their Figs 4 to 6 I would conclude that a trend analysis is really very difficult with these datasets, especially one obeserves a trend between MODIS DA and AERONET AOD in Fig. 4. Regional trends between ATSR and AVHRR (Thomas et al 2010) do not seem to agree in sign (Table 4). The article of Hsu et al 2012 adds data from SeaWiFS in comparison with MODIS DA and AERONET. They also explore the change in retrieved fraction over the years which also should be included in the uncertainties. [CLAUDIA STUBENRAUCH, France]	We agree with the reviewer- for this reason we have refrained from being to detailed in giving numbers for the trends- in a number of regions trend signs and magnitude are in agreement.
2-715	2	17	28	17	29	Rephrase sentence starting with "The accuracy of retireved…". [Birgit Hassler, United States of America]	This erroneous statement has been revised.
2-716	2	17	28	17	29	The following sentence is garbled: "The accuracy of retrieved AOD over oceans is usually better over ocean." Suggested revision: "The accuracy of AOD retrieved over oceans is usually better than AOD retrieved over land." [Forrest Mims, United States of America]	This erroneous statement has been revised.
2-717	2	17	28	17	29	About the expression: "The accuracy of retrieved AOD over oceans is ussually better over ocean". Please explain better this sentence. [Rubén D Piacentini, Argentina]	This erroneous statement has been revised.
2-718	2	17	28	17	29	I don't think this sentence makes sense: 'The accuracy of retrieved AOD over oceans is usually better over ocean.' [Kate Willett, United Kingdom]	This erroneous statement has been revised.
2-719	2	17	28	17	29	Change "The accuracy of retrieved AOD over oceans is usually better over ocean." to "The accuracy of retrieved AOD over oceans is usually better than over land." [Xuepeng (Tom) Zhao, United States of America]	This erroneous statement has been revised.
2-720	2	17	28			Reword this: The accuracy of retrieved AOD over oceans is usually better over ocean. [Michael Coffey, United States of America]	This erroneous statement has been revised.
2-721	2	17	28			Do the authors mean that the accuracy of retrieved AOD over ocean is usually better than ADO over land? This could be clarified by including the uncertainty of the retrieved AOD over land and ocean. [Government of United States of America]	This erroneous statement has been revised.
2-722	2	17	28			Please include the statistical significance of the trends. [Government of United States of America]	At some places we have included significance levels.
2-723	2	17	29	17	29	repeated twice "over oceans" [Claudio Cassardo, Italy]	This erroneous statement has been revised.
2-724	2	17	29	17	29	Typo -- Should be "… AOD is usually better over ocean." [Ralph Kahn, United States of America]	This erroneous statement has been revised.
2-725	2	17	29	17	29	typo: take out 'over oceans' after AOD [CLAUDIA STUBENRAUCH, France]	This erroneous statement has been revised.
2-726	2	17	32	17	33	From Figure 2.9 it appears that the most prominent area of negative AOD trends is off the west coast of Central America, with no sign of negative trends "near the US east coast". [Dale Hurst, United States of America]	We have improved the description of the text and and Figure to show this better.
2-727	2	17	35	17	35	remove redundant "Mishchenko et al" [Dale Hurst, United States of America]	This is an endnote problem to be solved in final draft

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-728	2	17	35	17	36	Double citation (just give date in brackets) [Peter Burt, United Kingdom]	This is an endnote problem to be solved in final draft
2-729	2	17	35	17	36	Correct style of reference "Mishchenko et al.". [Birgit Hassler, United States of America]	This is an endnote problem to be solved in final draft
2-730	2	17	35	17	36	Suggest adding a summary similar to this: "Two long-term (1980s-2000s) AVHRR AOD products over global oceans (Mishchenko et al., 2007a; Zhao et al., 2008) based on independent calibration, cloud screening, and aerosol retrieval algorithm indicate a negative trend for monthly and globally (over ocean) averaged AOD for the past three decades. The negative trend in NH is more evident than in SH but their significance generally falls below the 95% confidence level. However, the negative linear trend of zonal mean AOD in some latitude belts (e.g., 30oN-50oN) for the individual Atlantic Ocean and East Pacific Ocean can easily pass 95% confidence level (Zhao et al., 2011)." New Reference: Zhao, T. X.-P., A. Heidinger, and K. P. Knapp, Long-term Trends of Zonally Averaged Aerosol Optical Thickness Observed from Operational Satellite AVHRR Instrument, Meteoro. Appl., 18, 440-445, doi: 10.1002/met.235, 2011. [Xuepeng (Tom) Zhao, United States of America]	We have added the reference.
2-731	2	17	35			Define AVHRR [Michael Coffey, United States of America]	all abbreviation in appendix/table
2-732	2	17	37	17	37	ATSRR-2' should be 'ATSR-2' [Government of United Kingdom of Great Britain & Northern Ireland]	Corrected
2-733	2	17	37			Define ATSRR-2 [Michael Coffey, United States of America]	all abbreviation in appendix/table
2-734	2	17	38	17	38	Suggest adding the following sentence at the end of this paragraph: "and for AVHRR data (Zhao et al., 2011), such as latitude belt of 5oN-25oN of Indian Ocean and 32oN-40oN of West Pacific Ocean." [Xuepeng (Tom) Zhao, United States of America]	We have mentioned the general consistency except for the southern ocean.
2-735	2	17	41	17	44	A similar comment applies to the caption of Figure 2.9. The negative "AOD trends" off Mexico are ascribed to enhanced volcanic activity earlier in the period. In the context of an assessment of climate change, it would be better not to call such an AOD variation a trend. It is really low frequency variability associated with intermittent volcanic activity. Of course, there may be a true trend in volcanic activity that imparts a trend in aerosols. [Adrian Simmons, United Kingdom]	We will explain the use of the word trends or change in Box 1. The reviewer is correctly remarking that this trend is caused by a singular event, which is not sustained over a longer time.
2-736	2	17	46	17	46	Explain the abbreviation "SeaWiF". [Birgit Hassler, United States of America]	Table with Abbrevatiations will be provided.
2-737	2	17	46			SeaWiFS AOD shown in Figure 2.10 appears to be inconsistent with the AOD trend derived from MODIS shown in Figure 2.9. For example, a large positive trend appears on the east coast of China in MODIS AOD is not apparent in SeaWiFS AOD. A large positive trend over Red Sea in SeaWiFS is completely missing from MODIS AOD. However, there is no explanation of such discrepancies. [Government of United States of America]	Trends at the East Coast of China are visible in both retrieval but the magnitude differs due to differences in retrieval method. The large trend in spring and summer in SeaWifs is indeed much less visible in MODIS, possible due to screening out scenes with high dust loads.
2-738	2	17	53	17	53	The word "regional" here is confusing since the variability refers to seasonal variability. [Ileana Bladé, Spain]	AOD trends over the Saharan outflow region off western Africa display the strongest variability, with AOD increasing only in spring but strongly decreasing during the other seasons.
2-739	2	17	53		54	Consider rewording as follows: with AOD increasing only in Spring but otherwise AOD strongly decreasing during other seasons. [Government of United States of America]	Rephrased
2-740	2	17		20		Whether the data series correspond to surface-level or height-dependent or column-integrated, least squares linear trend and/or multi-regression statistical analyses are commonly used to delineate the long-term changes and trends. But these methods are sensitive to beginning and ending data points, so biases due to such effects need to be removed through the alternative methods such as piecewise liner 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 instrument performance , periodic calibration with standard sources is needed. Other 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. In order to estimate more realistic treds, improvements should take place in both	Good comment, but this detailed discussion is beyond the scope of this section. In Chapter 2 generally linear trend methods have choosen, indeed with the disadvantage discussed by the reviewer,but with the advantage that numbers throughout the chapter become better comparible.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						directions of normalization with instrument performance from time to time, and removal of natural variability induced by solar, meteroolgical and geographical parameters. [Panuganti, C.S. Devara, India]	
2-741	2	17				I would replace Figs 2.9 and 2.10 by a figure like Fig 12 or 13 of Hsu et al 2012, using several datasets; at least with this kind of figure one can see the behaviour over the whole period. A second figure could be like Fig. 9 of Hsu et al, again results from several datasets would be better (or explain why the dataset chosen is the best). [CLAUDIA STUBENRAUCH, France]	Thank you for the suggestion- we have choosen to highlight the seasonal aspect of aerosol trends.
2-742	2	18	3	18	5	For clarity I suggest: "The analysis of …. do not confirm THE continuation of decreasing AOF trends …. SUGGESTED by earlier analyses". [Ileana Bladé, Spain]	We have rephrased to: . These regional changes are consistent with independent analysis of AVHRR trends for the 1981-2005 period (updated from Mishchenko et al., 2007) (Cermak et al., 2010; Zhao et al, 2010) ; except over the Southern Ocean (45°S–60°S) where negative AOD trends of AVHRR retrievals are neither confirmed by MODIS data (Zhang and Reid 2010) nor by ATSR-2-data for the period 1995-2001 (Thomas et al., 2010). The latter analysis also shows that owing to strong interannual variability, AOD trends over most larger ocean regions are only weakly or not significant with the exception of tropical Pacific and the Indian Ocean, the latter in agreement with positive continental scale AOD trends over southern Asia.
2-743	2	18	3	18	6	The cited references do not explicitly consider post-2000 trends. A more accurate wording of this sentence would be: "Analyses of more recent satellite and AERONET trends after 2000 do not register a continuation of negative AOD trends as seen in analyses of AVHRR sensor product time series (refs)." [Jan Cermak, Germany]	Accept.
2-744	2	18	3	18	7	Suggest that the authors consider replacing: "The analyses of more recent satellite and AERONET trends do not confirm a continuation of decreasing AOD trends over the oceans after 2000 (see Section 2.3 for a discussion on 'global brightening'), as suggested by earlier analyses of AVHRR sensor products (Cermak et al., 2010; Mishchenko et al., 2007a). However, analysis of longer over-lapping multi-annual time series is needed to corroborate this finding." with "The analyses of more recent satellite and AERONET trends after 2000 do not show a continuation of decreasing AOD trends over the global oceans (see Section 2.3 for a discussion on 'global brightening') revealed by earlier analyses of AVHRR sensor products for the period from the late 1980s to the early 2000s (Cermak et al., 2010; Mishchenko et al., 2007b). A more recent segment of AVHRR data also reveals no significant AOD tendencies (Mishchenko et al. 2012). Analysis of longer overlapping multi-annual time series is needed to obtain a reliable profile of AOD for the full period of available satellite data." [Government of United States of America]	Thank you for the suggestion: the ocean trends have been de-emphasized in the text and we do not extensively discuss this.
2-745	2	18	3			AERONET cannot provide a complete and/or representative picture of changes over the oceans. Source for this statement? [Jan Cermak, Germany]	Most Aeronet stations are based over land [Holben et al.] and this is discussed.
2-746	2	18	4			"negative AOD trends" instead of "decreasing AOD trends" [Jan Cermak, Germany]	Corrected
2-747	2	18	9	18	13	Please indicate this paragraph is a summary of the whole section. [Ileana Bladé, Spain]	We make a summary at the end of the aerosol section
2-748	2	18	9	18	13	The whole paragraph seems like a repetition of things said in the earlier paragraphs of Section 2.2.3.1. Consider combining this paragraoh with the previous ones. [Birgit Hassler, United States of America]	This paragraph has been better integrated
2-749	2	18	12		13	"Vast regions of the world do not display significant aerosol trends over the last decades, and consequently no	We have rephrased the discussion on global trends.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						global tropospheric AOD trend can be detected." What is the technical basis for this conclusion? Do the authors mean that a global tropo AOD trend cannot be inferred from the existing data because regional trends have not been observed in the data. "Detected" gives a very different message from "inferred." This statement should be reconsidered to be sure that the meaning intended is the one conveyed. (represents comments from 3 reviewers) [Government of United States of America]	
2-750	2	18	22			Please include the statistical significance of the trends. [Government of United States of America]	Included were meaningful.
2-751	2	18				Table 2.2: again to compare results from different sources would be more conclusive. [CLAUDIA STUBENRAUCH, France]	Thank you this has increasingly be done
2-752	2	19	1	19	3	Widowed 'notes', please check page layout in final version [Peter Burt, United Kingdom]	Will be done
2-753	2	19	6	19	6	Why are qualitative estimates of aerosol concentrations from before 1990 not discussed? These are crucial later on in the report, and there estimates from horizontal visibility (eg Wang et al, 2009, doi:10.1126/science.1167549, van Oldenborgh et al, 2010, doi:10.5194/acp-10-4597-2010). [Geert Jan van Oldenborgh, Netherlands]	We have include some disscusion on the basis of Zhu 2009.
2-754	2	19	8	19	8	Why didn't AR4 report trends in particulate but AR5 does? Is it because there is new work to report, or because it is a newly recognized issue? [Dian Seidel, United States of America]	The issue of linkage between air pollution and climate is indeed receiving more attention, and enhanced measurement activities were started in late 1990's and early 2000. More data and longer time series is now available necessary for reliable trend studies.
2-755	2	19	10			The definition of black carbon, elemental carbon and light absorbing carbon, was not given before the discussion. A good description is given in Appendix 2.A.2.3. Please consider providing the definition proir to the discusssion. [Government of United States of America]	Due to space considerations the definitions were moved to th appendix.
2-756	2	19	16	19	16	Actually, other periods are also discussed, not just the two mentioned. See, for example, line 54. [Dian Seidel, United States of America]	This is correct, the periods mentioned refer only to Figure 2.11, as stated. No correctionsmade
2-757	2	19	24	19	31	I am confused by the time periods in this paragraph. According to Fig. 2.11 PM2.5 there are no trend measurements in Europe for the period 1990-2009 and PM10 trends are not shown for this period either, so the first two sentences of the paragraph really only applies to the recent 2000-2009 period. This needs to be stated explicitly. In addition, the following sentence on sulphates applies to the period 1990-2009. For clarity it would make more sense to say that increasing trends are also seen in sulphate for the same 2000-2009 period and that the trends are even stronger when referred back to 1990. [Ileana Bladé, Spain]	This is changed, and periods are now stated explicitly. The sentence is rephrased, but note that the reviewer is mixing up increase/decrease in the sulfate trend.
2-758	2	19	24			“strong significant downward trend”. Does this mean a downward trend with a strong statistical significant level or simply mean a strong downward trend? In any case, the statistical significance of the trend should be given here. [Government of United States of America]	The sentence is rephrased.
2-759	2	19	34			Define IMPROVE [Michael Coffey, United States of America]	Frank/Cathrine: defintion in the Appendix
2-760	2	19	36	19	39	Structure of the sentence starting with "In Canada, …" is confucing. Consider rephrasing. [Birgit Hassler, United States of America]	The sentence is rephrased:
2-761	2	19	38	19	39	"remarkable agreement between PM2.5 and SO42– declines in Canada, pointing to common emission sources." [Dale Hurst, United States of America]	Suggestion adopted
2-762	2	19	46	19	47	This attribution statement is in apparent contradiction with what is stated at the beginning of the chapter (Page 7). If there was an actual criterion for when to allow attribution statements then it needs to be stated, otherwise it gives the impression of lack of consistency/uniformity throughout the chapter. [Ileana Bladé, Spain]	Sentence has been adopted
2-763	2	19	47	19	48	Either "more and longer Asian time series" exist but haven't been analyzed (so why not?) or they don't exist (so there is no need to mention them). [Dian Seidel, United States of America]	Agree, the sentence is rephrased:
2-764	2	19	48			"Elsewhere trends in scattering and absorption are mostly insignificant." Please specify what is meant by "elsewhere.""Elsewhere trends in scattering and absorption are mostly	The paragraph and sentence is rephrased.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						insignificant." [Government of United States of America]	
2-765	2	19	50	19	50	"America, the Pacific and the Caribbean, but are increasing…" [Dale Hurst, United States of America]	Corrected.
2-766	2	19	50			'Caribic' is not a common geographic term (no relevant hits on Google, for example). Clarify [Government of Australia]	Corrected.
2-767	2	19	52			It should be elemental carbon not light absorbing carbon. Some organic carbon is light absorbing. [European Union]	This is related to the way the US and IMPROVE is representing their data and trends. They present trends in "Total carbon" as a sum of light absorbing carbon (LAC) and organic carbon. Their "LAC" is similar to European EC, but not measured in same way, and thus not exactly the same. When summarizing trends in AR5 we choose to use the same terminology as in the original literature when it comes to terminology of carbonaceous aerosols to avoid introducing misinterpretations in the trends of the variables. A description is given in Appendix 2.A.2.3
2-768	2	19	52			total carbon = elemental carbon + organic carbon. Light absorbing carbon was not defined before it was used liberally. Elemental carbon, black carbon, or light absorbing carbon, are used seemingly interchangeably, which will likely confuse readers who are unfamiliar with the subject area. [Government of United States of America]	We agree with review, the definitions as used in this report are given in the Appendix
2-769	2	19	53	19	53	remove "of total carbon" because it is redundant [Dale Hurst, United States of America]	Corrected.
2-770	2	19	55	19	55	Correct style of reference "Torseth et al.". [Birgit Hassler, United States of America]	Corrected.
2-771	2	19	55	19	55	Just (2012) needed rather than (Torseth et al. 2012) within the brackets here? [Kate Willett, United Kingdom]	Corrected.
2-772	2	20	4	20	4	Does "two out of three" mean 2/3 of a lot of stations, or 2 of a total of 3 stations? [Dian Seidel, United States of America]	Two out of a total of three. Corrected.
2-773	2	20	6	20	6	How many is "a host"? [Dian Seidel, United States of America]	The sentence is rephrased. It is difficult to specify a precise number. Almost 800 combinations of sites and variables have reported trends in various aerosol variables in the studies included here. Some sites report several variables, approximately 200 different sites are included.
2-774	2	20	6	20	12	Add "In summary", for clarity. Yes, it will feel repetitive to have "In summary" at the end of every page but that's what is needed if a summary is offered. [Ileana Bladé, Spain]	We now include a summary statement for both the remote sensing and surface aerosolwill need to see if the remote sensing and surface aerosol can be combined. Your opinion? A reduced form can then propagate to the ES.
2-775	2	20	9	20	10	Generalizing the results from two sites on the big island of Hawaii to the Pacific as a whole is unwarranted. Furthermore, one of the sites is located at Hawaii Volcanoes National Park, and is clearly influenced by a nearby volcano. [John Ogren, United States of America]	The sentence is rephrased.
2-776	2	20	12	20	12	One sentence should be added to the end of this paragraph: However, constrasting trends of mass and optical concentrations of aerosols from 1992 to 2010 over Europe, America, Canada, and China (Wang et al., 2012). Reference: Wang, K.C., Dickinson, R.E., Su, L. and Trenberth, K.E., 2012. Contrasting trends of mass and optical properties of aerosols over the Northern Hemisphere from 1992 to 2011. Atmospheric Chemistry and Physics, 12(19): 9387-9398. [Kaicun Wang, China]	The sites included in this study are mainly urban or suburban sites (more than 80%) largely influenced by local source distributions and changes. These results are not comparable to regional background sites with large representativity as included in the other studies.
2-777	2	20	15	20	18	Caption for Table 2.3 is different than the caption on page 134, line 3-5. [Birgit Hassler, United States of America]	The correct one is the one at page 20 in section 2.2.3.2 (SOD)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-778	2	20	20	21	23	I think that, at the end of the box 2.2, it should be stated more clearly which is the message of the box. [Claudio Cassardo, Italy]	Accepted
2-779	2	20	20	21	24	I like Box 2.2 in that it acknowledges some of the issues around calculating trends and states clearly that there is no actual physical reason for using a linear trend. Yet, in the end, linear trends are used "for simplicity"; perhaps this choice could be elaborated better and acknowlegged as sub-optimal (rather than giving the impression that it is fully satisfactory as it does now with thebox ending in Table which is used as an illustration of consistency between the linear and non-linear methods). Also, maybe this table should include the period 1979-2011 which is used throughout the chapter. [Ileana Bladé, Spain]	Accepted - to clarify meaning of box. shorter record will not be included here, but is included in Table 2.4, where it is calculated using the same method.
2-780	2	20	22	20	24	Missing closing bracket [Elizabeth Kent, United Kingdom]	corrected
2-781	2	20	22	21	14	Same as comment number 14 [Rahimzadeh Fatemeh, Iran]	unclear to what this refers
2-782	2	20	22	21	21	Same comment for Box 2.2 [Government of France]	unclear to what this refers
2-783	2	20	22	21	21	Box 2.2. The title is ”quantifying changes in the mean: trend models and estimation”. Thus if the trend in year t is denoted by mu(t), we are interested in any trend difference [mu(t) - mu(s)], with t and s within the sample period. There is only one method which generates uncertainties around such differences: trends from the class of structural time series models. See Visser (2004): Estimation and detection of flexible trends, Atm. Environment 38, p. 4135-4145, or Chandler and Scott (2011, Section 5.5). It is important to note that the well-known OLS linear trend is a special case of these models. If trend differences are so important than it is of equal importance to mention these stochastic trend models and their capability of estimating uncertainty bands around trend differences! See Visser and Petersen, 2012, Climate of the Past 8, p. 265-286. This remark is also important for Section 2.A.3, pages 2-120 through 2-2-124. [Hans Visser, The Netherlands]	Here is a quote from the abstract to Visser(2004) "Once flexibility is introduced the problem of detecting statistically significant increase/decrease becomes much more complicated: the trend may be alternating between being constant, decreasing or increasing. As a consequence, the problem of detecting significant increases or decreases cannot be summarised in one single figure or statistical test, as in the case of linear trend detection." In order to provide approximate but understandable information, we have used the linear trend.
2-784	2	20	22	21	21	Box 2.2. An important aspect of choosing a stochastic trend model, is that one can compute the probability of crossing a pre-defined threshold. This aspect is omitted here completely. Please see Visser and Petersen (2009), The likelihood of holding outdoor skating marathons in the Netherlands as a policy-relevant indicator of climate change, Climatic Change 93, p. 39-54, and Visser and Petersen, Climate of the Past 8, p. 265-286, 2012, Figure 1. [Hans Visser, The Netherlands]	Rejected - this consideration is beyond the scope of this chapter
2-785	2	20	22			When reading this box, we are left at the end wondering - so why do they use linear trends and not non-linear trends in this chapter? All that is needed is a firm summary statement at the end of this box outlining the reasoning for why linear trends are favoured in the chapter. Otherwise, the only reasoning given to the reader is "for simplicity" as given on line 28, and this seems a somewhat weak justification. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - such a statement has been attempted. Another way of phrasing simplicity is transparency. The more people can understand and criticize the method, the more transparent it is. Much more complicated and "sophisticated" methods could be used, but the quantitative answer would not be different within the uncertainty bars. So why not
2-786	2	20	24	20	33	Statistical methods usually assume that all samples have been obtained under identical circumstances. For time series of climate observations this condition may apply to the measurements of trace gases, but it certainly does not apply to the "Annual mean temperature anpmaly or to the average sea level, This means that plausible "trends" of temperature or sea level and most of the other climate features presented in this Chapter cannot be obtained by any of the standard statistical methods. This particularly applies to long term series. Some short term series may be used where conditions of measurement are known to have been similar. For these reasons I regard most of the "trends" shown in this Chapter, and particularly thw temperature time series in this Chapter and the sea level series in Chapter 13 as unreliable. [Vincent Gray, New Zealand]	Rejected - If the data are accepted as given, then the linear trend gives an objective estimate of change with uncertainty that is transparent.
2-787	2	20	24	21	23	Please state that the projection of trends must be used with extreme caution because the underlying assumption is that the causes of those trends will continue in regular fashion and behave in consistent fashion. This is patently untrue when temperature trends for periods that include the late 1990s are strongly influenced	Accepted - we are not attempting to attribute the trends or speculate whether they will continue.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						by the extreme El Nino in 1997-8. [John McLean, Australia]	
2-788	2	20	27	20	27	It has obviously been decided not to use a form of linear regression that includes the published uncertainties in annual values of HadCRUT4. But the statement placed here is just not true that uncertainties in trend are not affected much by different methods. Thus taking account of published uncertainties in HadCRUT4 annual mean anomaly values would a substantially increase trend uncertainty - as can be seen in the corresponding chapter in the AR3 which used a modified REML method to account for this factor in temperature trends. Accounting for uncertainties in HadCRUT4 data has a larger effect than that related to serial correlation of its trend residuals. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Yes it is true that we are not taking into account individual groups estimates of uncertainties in annual means as a function of time. Although this gives a trend uncertainty that is a slight underestimate, it allows us to use the same method for all the data sets for which we compute trends. Not all data sets have provided uncertainties for annual means, and for those that do, a wide variety of methods have been applied and not all sources of uncertainty are included in each.
2-789	2	20	28	20	32	You say that the confidence interval for your temperature trends is solely that arising from uncertainty in the trend parameter, but you also say that you use the Santer et al. (2008) AR1 method. In light of ample literature in long term persistence that shows that AR1 trend models likely underestimate the uncertainty of trend parameters, you should include a caution to readers to this effect. Or better yet you should use a more accurate method. [Ross McKitrick, Canada]	Accepted, We have tried to more clearly express the remaining uncertainties that are left out of our analysis.
2-790	2	20	28	20	32	Since I already criticized the use of the AR1 model for estimating trend significance in my comments on the First Order Draft (apparently to no avail), I will just restate the main points here. Obviously the AR1 method presented here is invalid and the treatment needs to be brought up to date. Surely you can't propose to use the "effective dof" method employed in Santer et al. (2008). It is an approximation first 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. For a recent survey of modern univariate trend modeling issue see Mills (2009) 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 and would at least bring the IPCC up to a 1970s level of statistical sophistication. And you could easily use Newey-West standard errors which are robust to any form of autocorrelation, and which are also available in most modern stats packages now. [Ross McKitrick, Canada]	Thank you for these suggestions, but we have chosen not to change our method. We are not convinced that using more complex methods, of which there are a great many candidates, none of which are perfect, provide any additional information to scientists or policy makers.
2-791	2	20	30	20	31	"5 to 95% confidence interval" is mis-worded. I wouldn't believe anything with 5% confidence. Do you mean the 95% confidence interval or the inner 90th percentile (5-95%)? [Dale Hurst, United States of America]	What it means is that within the 5 to 95% intervals is 90% of the probability, with only 5% chance of the trends being larger or smaller, respectively. This can be reworded to 90%, if that helps.
2-792	2	20	32	20	34	It seems a mistake to exclude parametric uncertainty here and elsewhere. From 1979 to present, parametric uncertainty as represented by HadCRUT4 is comparable to structural uncertainty as estimated from the spread of available global temperature series used in AR5 (e.g. Fig 2.21). Neglecting parametric uncertainty means neglecting an important slice of the uncertainty pie. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted - we can state that parametric uncertainty is ignored in these uncertainty limits and refer to sources.
2-793	2	20	36	20	36	see my earlier comment about Cohn and Lins 2005 at page 7 line 10. This would be the place to discuss trend significance and long term persistence/autocorrelation [Marcel Crok, The Netherlands]	Accepted - that's precisely what this box is about.
2-794	2	20	41	20	44	A linear trend map also assumes that the trend pattern is fixed and only its intensity increases, which is not necessarily true (see Cai, W., and Whetton, P. H. (2001). A time-varying greenhouse warming pattern and the tropical-extratropical circulation linkage in the Pacific Ocean. Journal of Climate, 14 (16): 3337-3355.) [Government of Australia]	TRUE
2-795	2	20	42			change "bell shaped" to "Gaussian" or "normally distributed" [Government of Australia]	Accepted
2-796	2	20	43			Missing end bracket [Richard Allan, United Kingdom]	Thank you.
2-797	2	20	46	20	53	Perhaps add that a common source of non-linearity in climate data is from oscillatory behaviour of various kinds [Government of Australia]	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-798	2	20	46	20	53	What is the message here? Should one use or not use linear trend? [Xuebin Zhang, Canada]	Yes we are using it, but with certain warnings about what is left out. The alternatives do not yet seem to offer sufficient advantages to overcome the greater complexity and free parameters choices associated with smoothing procedures.
2-799	2	20	55	21	3	Flexible regression methods can also be used for nonlinear trend estimation (see, e.g., Bates, B.C., Chandler, R.E. and Bowman, A.W. (2012) Trend estimation and change point detection in individual climatic series using flexible regression methods. J. Geophys. Res., 117, D16106, doi:10.1029/2011JD017077). [Government of Australia]	Yes, a virtually unlimited menu of options exists for looking at changes in data series. Each would give a somewhat different interpretation, but it would be hopelessly confusing to readers. Here we attempt to assess whether trends are outside the statistical noise on certaint intervals and use a simple, transparent method to do so.
2-800	2	20	55	21	17	Section on Non-linear Trends: it is not made clear why this method (smoothing spline) is included. Should it be considered as more appropriate to use than a linear method for calculating temperature trends? Also, the trend values calculated using this method are not mentioned anywhere else in the report – should they be included in the SPM or TS. Finally, at 2,21,17, the table caption should mention that the linear trend values were calculated from the HadCRUT4 dataset. [Government of United Kingdom of Great Britain & Northern Ireland]	The point here is that the same bottom line answer is obtained with two very different methods out of a virtually limitless palette of possible methods of varying complexity.
2-801	2	20	56	20	56	Fawcett et al 2012 examined the use of different types of trend models for Australian mean temperature data. [Government of Australia]	Thank you.
2-802	2	20		21		Box 2.2 - this box is weak - simply comparing one linear technique with one non-linear technique does not add much value. Also, suggest possible reference to Fawcett 2012 (http://www.bom.gov.au/climate/change/acorn-sat/documents/ACORN-SAT_Report_No_3b_WEB.pdf). [Government of Australia]	That is not a peer-reviewed reference. We have referred to many studies of various methods.
2-803	2	21	3	21	3	Add: Another broadly used smoothing technique is (Gaussian) low-pass filltering (used e.g. in Fig. 2.35 and box 2.5, Fig. 1). [Christian-D. Schoenwiese, Germany]	It's more the estimation of the uncertainty of the smoothed curve that is critical.
2-804	2	21	6	21	9	Thank you for changing this since the first draft; this shows much clearer that trends in the first and second half of the 20th century were similar; the real enigma is, i.e. what caused the early 20th century warming? [Marcel Crok, The Netherlands]	Thank you.
2-805	2	21	6	21	21	Table 1 is misleading by giving the appearance of an increase in the smoothing spline trend for 1951-2011 compared to 1901-1950 by artifact of using differing fractions: 50years vs 60 years. It is important to correct this to show half of the period for each. ie 55 years each so calculate for the two 55 year periods 1901-1956 and 1956-2011. [David L. Hagen, United States of America]	rejected - There is a history of computing the 1901-1950 trend and then 1950 to present. The 1901-present trend is the most robust and meaningful.
2-806	2	21	6	21	21	To show anthropogenic contribution, the null hypothesis would be natural variations. Recommend showing the 1500 year underlying natural oscillation as documented Loehle & Singer and adding: "Nine temperature reconstructions show ~1500 year (or 1200) climate cyles, possibly corresponding to the Pleistocene Dansgaard-Oeschger (DO) oscillations (Loehle & Singer, 2010). Anthropogenic contributions need to be distinguished from the underlying rising portion of this cycle since the Little Ice Age." Reference: "Craig Loehle and S. Fred Singer, Holocene temperature records show millenial-scale periodicity. Canadian Journal Earth Science Vol. 47 pp 1327-1336 (2010)." [David L. Hagen, United States of America]	Rejected - this type of data is discussed in the paleo chapter.
2-807	2	21	17	21	19	Need to say whether the calculation is done on annual data or monthly data, since this affects the compensation for autocorrelation of the residuals [Government of Australia]	Accepted - annual data has been used throughout and this has been indicated.
2-808	2	21	17	21	20	Box 2.2 Table 1. No units are given for the trends. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-809	2	21	17	21	21	Box 2.2, Table 1 - to include the period 1979-2011 as in the other tables. [Government of Brazil]	Rejected - the numbers for the 1979-present period are presented elsewhere in the chapter
2-810	2	21	17	21	28	Box 2.2 Table 1 Add the temperature and time units. This appears to be deg C/decade but that is nowhere	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						stated. [David L. Hagen, United States of America]	
2-811	2	21	25	21	25	Great section - it's good to see this handled so comprehensively here, could refer to Chapter 13 [Piers Forster, United Kingdom of Great Britain & Northern Ireland]	Noted. Thanks for the positive feedback.
2-812	2	21	25			Changes in Radiation Budgets: Irradiance measurement is a key parameter in climate studies. Yet no mention is found in the document of “pyranometer”, the instrument used to measure irradiance. Should a description of this measurement technology be included? [Government of United States of America]	Noted, although we would love to include this, due to space restriction we have no possibility to elaborate on the measurement devices and technique in this section.
2-813	2	21	27	21	27	Change "component" to "driver". [Government of Australia]	Taken into account - however, while a forcing is a driver, a budget is rather a "component" or "element" of the climate system. We changed "component" to "element".
2-814	2	21	27	21	29	It would be preferable, in my opinion, to talk not of the radiation budget as requiring something of the hydrological cycle and sensible heating. Rather I would say that a balanced thermal or energy budget requires that imbalance in the radiation budget be compensated by fluxes of moisture and heat. Also, perhaps this introductory paragraph could talk about tropical/polar differences in the radiation budget. Overall balance in the thermal or energy budget requires dynamical atmospheric and oceanic transfers of heat from the tropics to the polar regions. [Adrian Simmons, United Kingdom]	Taken into account - we revised the sentence accordingly to emphasize the balance rather than a requirement. Although we would love to, we do not have the space to expand this paragraph further to discuss tropical/polar differences and associated consequences.
2-815	2	21	36		36	Please do not use the term “upwelling and downwelling” but use “upward and downward” throughout the chapter. Because: 1) these are not English words. 2) Electromagnetic radiation is not “welling” if this stands for cascading. 3) One usually refers to an irradiance at a particular horizontal location either upward or downward. [Rolf Philipona, Switzerland]	Accepted, changed as suggested throughout the chapter.
2-816	2	21	36			"thermal" radiation - it may be worth noting that this is commonly interchangable with "longwave", "long wave-length" and "infra-red" radiative fluxes. [Richard Allan, United Kingdom]	Taken into account - we added a related comment at the beginning of Section 2.3.3.2
2-817	2	21	39	21	40	I suggest removing these lines since they are superfluous. [Richard Allan, United Kingdom]	Noted - we still believe the sentence is useful to guide the reader through the section
2-818	2	21	42	26	56	This "Energy Budget" is implausible as it ignores all the components of the climate which have been established by meteorology.The Climate is a heat engine where the energy comes from the sun';s radiation. and after its utilization, the residue is radiated to space, which is the exhaust. All energy exchange takes place by conduction, convection and latent heat transfer. Forecasting is inhibited because of the prime importance of fluid flow, which is subjecct to :chaos" The IPCC model replaces all this with internal energy exchange entirely by radiation, and it also assumes that the earth does not rotate, that the sun shines with constant intensity, both day and night and that the "chaos"resulting from the unpredictable behaviour of the atmosphere and the oceans is conveniently eliminated [Vincent Gray, New Zealand]	Noted - The fact that the energy for the climate system comes from the sun and is released in form of thermal radiation back to space is accounted for in the global mean energy budget, Also the major energy exchanges between the atmosphere and surface on a global mean basis, not only in form of exchanges of radiation, but also in form of the non-radiative fluxes of sensible and latent heating, as well as the net energy flux into the ocean are considered. The IPCC models do not only take into account the radiation, but also the 3 dimensional dynamics of the climate system, and do account for the earth rotation as well as the diurnal cycle with changing solar zenith angle and associated insolation over the course of a day. See also response to comment 2-2755.
2-819	2	21	44	22	2	In fact recent knowledge has gone backwards as papers such as Stephens et al 2012 are badly in error. There remain many issues with aspects of the energy budget not properly reflected in this section [Kevin Trenberth, United States of America]	Taken into account - The underlying problem is that uncertainties in the different space- and surface based measurement systems are still considerable when it comes to global mean energy balance values, particularly in the surface components. Therefore different views exist on the closure of the surface energy budget, and the observational basis is often insufficient to judge which of the different

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							assessments are "badly in error". Recent progress helped to better constrain some of the surface radiative components. The latest surface- and satellite-based estimates agree now within 2 Wm-2 in both their global mean downward solar and thermal components, and the related references have been added. The controversial discussion on the consistency between energy and water cycle is also included. Remaining gaps and uncertainties in our knowledge are acknowledged. We address this issue by including not only one single value, but in addition a range of plausible values of the magnitude of the major energy balance components.
2-820	2	21	44			Section 2.3.1 - Assessment of the intra-atmospheric radiation budget is absent from this section. Suggest that the following text and references be added: Radiative fluxes within the atmosphere have been measured by balloon and aircraft, for example, but these data sets are sparse. The CloudSat and CALIPSO satellites, launched in 2006, have provided new estimates of in-atmosphere radiative fluxes and heating rates. For example, L'Ecuyer, T. S., N. B. Wood, T. Haladay, G. L. Stephens, and P. W. Stackhouse (2008), Impact of clouds on atmospheric heating based on the R04 CloudSat fluxes and heating rates data set, J. Geophys. Res., 113(D8), D00A15, doi:10.1029/2008JD009951. Henderson, D., T. L'Ecuyer, G. Stephens, P. Partain, and M. Sekiguchi (2012), A multi-sensor perspective on the radiative impacts of clouds and aerosols, J. Appl. Meteorol. Clim., in review. Haynes, J. M., T. H. Vonder Haar, T. L'Ecuyer, and D. Henderson (20XX), Radiative heating characteristics of earth's cloudy atmosphere from vertically resolved active sensors, J. Geophys. Res., in review. (available at http://www.engr.colostate.edu/~jhaynes/papers/haynes_in_review_oct12.pdf) [Government of United States of America]	Noted - due to severe space limitation we cannot further elaborate on the intra atmospheric radiation budget. The text suggested to be included does not provide immediate additional information to the reader.
2-821	2	21	46			"unprecedented accuracy" or "unprecedented stability"? [Government of United States of America]	Taken into account - "are measured with unprecedented accuracy from" has been replaced with "are observed from".
2-822	2	21	48	21	48	Replace "near the turn of the millennium" with something more specific (e.g., a year or set of years) [Government of Australia]	Accepted - "which began operation near the turn of the millennium" has been replaced with "which began data collection in 2000 and 2003, respectively".
2-823	2	21	53	21	53	Add "and Fig. 1". [Ileana Bladé, Spain]	Noted - we do not see what Figure 1 should stand for.
2-824	2	21	53	21	53	SORCE still operating -> SORCE and still operating [Ileana Bladé, Spain]	Accepted - changed as suggested.
2-825	2	21				I suggest using the phrase "radiative energy budget" which is more informative. [Richard Allan, United Kingdom]	Noted - it is not entirely clear where this change should apply.
2-826	2	22	1	22	2	The solar irradiance is not constant and the observed variations with the sunspot cycle must be included here. It makes a difference of up to 0.15 W m-2 in the solar irradiance forcing. [Kevin Trenberth, United States of America]	Noted - This is true, however the variability in solar radiation with sunspot cycle is discussed in detail in Section 8.4.1, (Chapter 8 discusses the forcings). For the purpose of this section the given accuracy is adequate and higher than most other flux components. A reference to Section 8.4.1 is made at the beginning of Section 2.3.2
2-827	2	22	1			What effect will the revised estimate of 340 W/m2 for the solar constant as measured by a cavity (?)	Noted - Because there is overlap between solar

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						radiometer have on the flux values prior to SORCE/TIM? [Government of United States of America]	irradiance measurements from SORCE and its predecessors, the prior incoming solar irradiance values will have to be adjusted by approximately 1 Wm-2 to be consistent with SORCE-TIM.
2-828	2	22	5	22	8	This Figure must be revised to exclude values known to be incorrect. [Kevin Trenberth, United States of America]	Taken into account - The comment primarily refers to a specific component in this diagram, namely the latent heat flux/evaporation, whose magnitude has led to recent controversial debates. We revised the range of values of the latent heat flux given on Figure 2.12, to take into account constraints from global mean precipitation estimates and their bias errors. Figure 2.12 now includes 70 Wm-2 as lower bound for the global mean latent heat flux to reflect the lower limit of global mean precipitation estimates based on GPCP and their stated maximal bias errors (Adler et al. 2012). As upper bound we use 85 Wm-2, which is considered as the upper limit of current uncertainties in precipitation retrieval following Trenberth and Fasullo (2012, Surv. Geophys.). The sensible heat flux is highly uncertain, as no direct overvational constraints are available and to a considerable extent has to rely on modeling and reanalyses results. We use the Stephens et al. (2012) best estimate of 24 Wm-2 as upper limit and an uncertainty range of 10 Wm-2 which encompasses also recent estimates from reanlyses (Trenberth et al. 2009, Berrisford et al. 2011) and CMIP5 climate models (Wild et al. 2013). The existance of a considerable uncertainty range in the global mean sensible heat flux estimate that does not rule out the possibility of values exceeding 20 Wm-2, has been reinforced by recent results from SeaFlux. They give a best estimate of 17 Wm-2 over oceans only, thus leading to a global mean sensible heat flux clearly exceeding 20 Wm-2 (C.A. Clayson, personal communication, paper submitted). All other estimates shown on Figure 2.12 are current best estimates based on recent studies incorporating both surface and space born observations. There is no study taking into account observations that would demonstrate that the other values in Figure 2.12 are biased or being incorrect within their uncertainty ranges. Thus, none of these other values in Figure 2.12 are known to be incorrect (see also response to 2-829).
2-829	2	22	10	22	53	There are major issues with this section. The raw CERES values are out of balance by over 6 W m-2. The ocean heat storge value quoted here is easily disputed and latest estimates remain at 0.9 W m-2 as in Trenberth et al (2009) from ORAS4 ECMWF reanalysis (paper submitted). In any case the value varies from early in the 2000s to late in the 2000s because of the sunspot cycle, so 0.7 is more rerasonable at end. The issues with this section and the Figure 2.12 can be highlighted by examining global precipitation and associated latent heating. The GPCP value is estimated as 76 W m-2. This has been carefully examined for all possible biases and assessed by the GEWEX Global Data and Assessments Panel (GDAP) chaired by Chris Kummerow. GDAP comments on global precipitation (Oct 2012) are as follows: a. Estimated GPCP global climatology bias error is < 8%	Taken into account. The CERES dataset used is EBAF, which uses Argo-based ocean heating rate to determine the planetary imbalance and associated uncertainty. The data and methodology have been published and are cited and summarized in some detail in this section. The Trenberth et al. (2009) value is from a climate model, not from observations. According to Trenberth et al. (2009): "The TOA energy imbalance can probably be most accurately

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						b. GPCC gauge analysis is adjusted upward for wind loss c. Snow is included over land (adjusted GPCC) d. Snow and light rain included over mid-high latitude ocean by use of TOVS/AIRS (Susskind) empirical relation—blend from passive microwave in low latitudes to TOVS/AIRS at higher latitudes—GPCP in middle/high latitude oceans tends to be higher than PMW estimates e. TRMM Composite Climatology (TCC) use TRMM V6 products approx match GPCP totals in deep tropics over ocean f. TRMM PR/Cloudsat comparisons by others indicate Cloudsat higher by ~5%, but were done with post-boost PR data. g. TCC and Japan colleagues use ~6% to adjust post-boost PR data to match means of pre-boost PR data. The LH flux is not 85 to 90 as implied in Fig 2.12. Accordingly, the estimated value is thought to be not possible to be more than 84 W m-2 and the estimate right now is that GPCP is too high because of an over-correction for snow. The best estimate remains 76 W m-2. This is in complete contrast to arguments put forward in Stephens et al 2012, for example, which have no credibility at all. The radiation budgets as computed by Kato et al (2011) (CERES) have already been revised in major ways owing to better treatment of the diurnal cycle, so the published values are not correct. These errors should be recognized and discussed. Also the error bars for the downwelling longwave are large and should be discussed. The implied net radiation flux at the surface requires an extra 11 W m-2 for sensible plus latent heat which is simply not viable; see Trenberth et al 2009. [Kevin Trenberth, United States of America]	determined from climate models and Fasullo and Trenberth (2008a) reduced the imbalance to be 0.9 W m−2, where the error bars are ±0.15 W m−2." The value Fasullo and Trenberth (2008a) use is from the Hansen et al (2005) Science paper. Recently, Hansen revised this value using actual Argo observations with an independent analysis (Von Schukmann et al, 2010) and obtained 0.6 Wm-2, consistent with the value cited here. The Hansen et al. (2011) reference is also cited in this section. The reviewer is correct in pointing out that there is considerably more uncertainty in the surface energy budget, as we emphasized in the SOD on lines 23-24 of p. 2-22. We discuss how the latest satellite-based estimates of surface radiation compare with surface measurements, and also discuss the apparent discrepancy between global mean surface radiation budget estimates and independent estimates of the global mean surface turbulent heat fluxes. We note that the magnitude of the global mean latent heat flux is currently disputed. The range of the global mean latent heat flux estimates in Figure 2.12 in the SOD is now revised to 70-85 Wm-2. The 70 Wm-2 at the lower bound reflect the lower limit of global mean precipitation estimates based on GPCP and stated maximal bias errors (Adler et al. 2012). As upper bound we use 85 Wm-2, which is considered as the upper limit of current uncertainties in precipitation retrieval following Trenberth and Fasullo (2012, Surv. Geophys.) (see also response to comment 2-828). Kato et al. have a paper in press that indeed corrects their 2011 paper; we have changed the reference accordingly. They reduce their best estimate of global mean surface absorbed solar radiation to 163 Wm-2, and now are only 2 Wm-2 higher than the latest estimate inferred from surface observations (Wild et al. 2013) and the estimate given by Trenberth et al. (2009) (both 161 Wm-2). We further discuss the much disputed downward thermal radiation. The satellite-based estimates of downward thermal radiation are in line with surface-based measurements both over land and ocean (Kato et al, 2013). Their global mean estimate is now within 2 Wm-2 of the best estimate given in Wild et al. (2013), who take into account the latest information from the BSRN network. These global mean estimates are also in line with the downward thermal radiation determined in the ERA Interim Reanalysis, whose radiation code compares favorably with surface observations with respect to downward thermal radiation. There is no modeling or observational evidence that would favor lower values than given in Figure 2.12 for the global mean downward thermal radiation, while there is an expanding literature in support of a value near the best estimate presented in Figure 2.12, based on

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							various independent approaches (satellite-based, surface observation based, empirical and modeling). Lower downward thermal radiation (as given in previous IPCC reports) is only supported from residual considerations, when taking the GPCP precipitation estimate equivalent to 76 Wm-2 latent heat flux for granted, and allowing no adjustments for a lower surface solar radiation or a higher sensible heat flux. The fluxes in Figure 2.12, within their uncertainty ranges, now still allow to accommodate a lower latent heat flux, covering the range as given by the GPCP precipitation estimates, primarily through a reduced surface solar absorption towards the low end of the given uncertainty range (below 160 Wm-2). Studies taking into account the information contained in surface observations do not completely rule out the possibility of such a lower surface solar radiation (in contrast to a lower downward thermal radiation where there is no observational evidence). A lower surface solar radiaton obtains also support from a recent paper of Kim ad Ramanathan (2012, GRL). Note that while the surface net radiation estimated in Trenberth et al. is lower than in Figure 2.12, the GEWEX Data and Assessments Panel (GDAP) surface radiation datasets (GEWEX-SRB, ISCCP) on the other hand yield a substantially higher surface net radiation compared to the one shown in Figure 2.12. Thus we believe that this section and Figure 2.12 provide a balanced assessment of the issue in view of the large discrepancies between the Trenberth et al. and the GDAP surface radiation estimates, and in view of the latest estimates from surface- and satellite-based studies.
2-830	2	22	12	22	12	Space missing in "thermalTOA" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-831	2	22	13	22	13	Space missing in "ratebased" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-832	2	22	14	22	21	The use of these numbers is questioned by the error bar as large as 17 W/m2 reported by Stephens et al (2010). [François Gervais, France]	Noted - the uncertainties associated with some of the fluxes is still considerable, particularly at the surface. This is acknowledged in Figure 2.12 by using not only a single value per energy flux to describe its magnitude, but in addition also a range of plausible values. However, for surface radiation, recent studies show improved consistency between satellite-derived and surface observation-based approaches, which strenghten our confidence in the estimates of some of these components (e.g. Kato et al. J. Climate in press, Wild et al. 2013 Climate Dynamics published online)
2-833	2	22	16	22	16	It seems a bit odd that the independently estimated global heat storage term is quoted as 0.6 Wm-2 with no uncertainties. [Ileana Bladé, Spain]	Accepted - Uncertainty range has been added.
2-834	2	22	18	22	19	The sentence that spans these lines might be improved if the words "due to calibration" were moved forward to appear after the word "uncertainty". [Adrian Simmons, United Kingdom]	Noted - we feel this change would not facilitate the reading of this section

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-835	2	22	21			"adds up to another 1 W m–2 or more" is ambiguous. Would it be improved to say “less than X W m-2”. [Government of United States of America]	Accepted - We've deleted "or more".
2-836	2	22	23	22	23	uncertainthan -> uncertain than [Mihai Dima, Romania]	Accepted, changed as suggested
2-837	2	22	23	22	23	Space missing in "uncertainthan" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-838	2	22	23	22	23	Please separates the two words "uncertainthat".The same type of error is present in different other texts. [Rubén D Piacentini, Argentina]	Accepted, changed as suggested
2-839	2	22	23	22	33	A reference to Berrisford et al (2011, Quart. J. Roy. Meteor. Soc., 137, 1381-1399) could be included in this paragraph. This paper discusses the budget components from ERA-Interim and other reanalyses in comparison with the values published in the cited article by Trenberth et al. (2009). The downward thermal radiation from ERA-Interim, for example, is 341 W m–2, a value that seems to fit in with the discussion presented in the paragraph. [Adrian Simmons, United Kingdom]	Accepted, a related statement and the Reference to Berrisford et al. (2011) has been added.
2-840	2	22	23	22	53	To continue. This is in complete contrast to arguments put forward in Stephens et al 2012, for example, which have no credibility at all. The radiation budgets as computed by Kato et al (2011) (CERES) have already been revised in major ways owing to better treatment of the diurnal cycle, so the published values are not correct. This should be discussed. Also the error bars for the downwelling longwave are large and should be discussed. The implied net radiation flux at the surface requires an extra 11 W m-2 for sensible plus latent heat which is simply not viable; see Trenberth et al 2009. There is an inadequate discussion of the major issues and uncertainties here. The global sensible heat flux by T et al (2009) is 17 with a possible 10% error. It might be as high as 19. It is not 20 to 25 as implied in Fig 2.12. [Kevin Trenberth, United States of America]	Taken into account (see also response to comment 2-829) - Kato et al indeed revised their values in a more recent publication accepted in J. Climate which is now included in this Section and provides additional support for the magnitude of the surface radiative fluxes presented here. They revised their best estimates down to 163 and 344 for the surface absorbed solar and downward thermal radiation, respectively. These values now come very close to completely independently estimated values by Wild et al. (2013), taking into account direct surface observations. These latter values are both 2 Wm-2 smaller than Kato et al (2013), resulting in surface absorbed solar and downward thermal radiation of 161 and 342 Wm-2. This leaves an atmospheric solar absorption of 79 Wm-2, which is identical to the ones independently derived by Trenberth et al. (2009) and Kim and Ramanathan (2009). Also, the atmospheric solar absorption in ERA interim and ERA40 are at 80 and 82 Wm-2 very close (Berrisford et al. 2011). The downward thermal radiation in ERA40 and ERA Interim (see comment 2-839), are with 344 and 341 Wm-2 also very close to the 344 and 342 Wm-2 of Kato et al. (2013) and Wild et al. (2013), respectively. An unpublished study of Wang et al. (submitted) also derives a best estimate of 342 Wm-2. Therefore we assess that a considerable degree of consensus has been reached in these recent, completely independently derived approaches, with a best estimate of surface absorbed solar and downward thermal radiation slightly above 160 and 340 Wm-2, respectively. This indeed requires a latent heat flux estimate at the high end of the current uncertainty estimates in global mean precipitation based on GPCP, should not the sensible heat flux be considerably higher than presently assumed, or the surface absorbed solar radiation lower (note that Figure 2.12 can accommodate now 76 Wm-2 or even less for the global mean latent heat flux within within its uncertainty range, which may be achieved primarily

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							through a lower surface solar absorption. Lowering the surface solar absorption would be more compatible with direct observations than lowering the downward thermal radiation).We can also not rule out the possibility of significant biases in our current best estimate of the sensible heat flux, as it is largely model based due to the lack of direct observations of this quantity. The existance of a considerable uncertainty range in the sensible heat flux, and the possibility of a global mean sensible heat flux exceeding 20 Wm-2 cannot be ruled out. This has been reinforced by recent results from SeaFlux, which give a best estimate of 17 Wm-2 over oceans only, thus leading to a global mean flux clearly exceeding 20 Wm-2 (C.A. Clayson, personal communication, paper submitted). Uncertainty ranges remain considerable in all these estimates (see also response to comment 2-828).
2-841	2	22	23	24	16	There are some formatting errors - i.e. missing spaces between words -- for example but not only on page 22, line 25, and probably in all of this chapter. [Robert Kandel, France]	Accepted, changed as suggested
2-842	2	22	23			uncertain: there is a typo [Alexander Loew, Germany]	Accepted, changed as suggested
2-843	2	22	25	22	25	criticalinformation -> critical information [Mihai Dima, Romania]	Accepted, changed as suggested
2-844	2	22	25	22	25	Change "criticalinformation" to "critical information" [Government of Brazil]	Accepted, changed as suggested
2-845	2	22	25	22	25	Space missing in "criticalinformation" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-846	2	22	27	22	27	In line with earlier studies -> In line with other studies (otherwise there is a confusing between "ealier studies" and "previous IPCC assessments"). [Ileana Bladé, Spain]	Taken into account - we skipped the word "earlier", since this includes now also recent studies.
2-847	2	22	30	22	30	Insert "However" prior to sentence "Estimates of … are lower ..." to help the reader anticipate the meaning of the sentence. [Ileana Bladé, Spain]	Noted - we think there is no necessity to add this word.
2-848	2	22	30	22	30	It would be appropriate to note that satellite-measurement-based estimates of surface radiation fluxes have not been validated by comparison with surface measurements on the open ocean (for example, there are only a very few island stations in the BSRN). If there are (hearsay at the Paris EarthCARE workshop earlier this fall) data from buoys or ships that now or soon will change this, they might well be mentioned [Robert Kandel, France]	Taken into Account, Kato et al. (2013) J. Climate also use buoy data over oceans in addition to land-based observations to validate their estimates of satellite-derived surface radiative fluxes. These validations give additional support for the best estimates of surface absorbed solar radiation near 160 Wm-2 and downward thermal radiation exceeding 340 Wm-2 supported in this section and in Figure 2.12.
2-849	2	22	35	22	41	Likewise, a reference to Berrisford et al (2011) could be included in this paragraph. [Adrian Simmons, United Kingdom]	Accepted, Berrisford et al. (2011) is now referred to in this section.
2-850	2	22	37	22	37	The text quotes a range of "below 160 to above 170 Wm-2" but figure 2.12 shows a range of 154-166 Wm-2. [Ileana Bladé, Spain]	Taken into account - The range quoted in the text refers to the published values from various sources, such as climate models (irrespective of known biases), while the range on the figure has additional observational constraints imposed. We revised the related sentence to make this point clearer.
2-851	2	22	38	22	38	Change 'vapor' to 'vapour' for consistency elsewhere in text. [Peter Burt, United Kingdom]	Accepted, changed as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-852	2	22	40	22	40	Wild et al. Submitted, update or skip [European Union]	Accepted - Wild et al. (2013) has been updated (published online in Climate Dynamics 13 November 2012).
2-853	2	22	41	22	41	Change "asomewhat" to "a somewhat" [Government of Brazil]	Accepted - but sentence removed
2-854	2	22	41	22	41	update Stephens et al., in press [European Union]	Accepted - Stephens et al has been updated
2-855	2	22	41	22	41	Space missing in "asomewhat" [Birgit Hassler, United States of America]	Accepted - but sentence removed
2-856	2	22	43	22	43	Change "toexceed" to "to exceed" [Government of Brazil]	Accepted, changed as suggested
2-857	2	22	43	22	43	Space missing in "toexceed" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-858	2	22	43	22	43	"toexceed" should read "to exceed" [Jeffrey Taylor, United States of America]	Accepted, changed as suggested
2-859	2	22	43			another typo [Alexander Loew, Germany]	Accepted, changed as suggested
2-860	2	22	46	22	47	Evaporation can be derived from reanalyses as well as from precipitation estimates. For ERA-Interim the latent heat flux is estimated to be 84W m-2, a value that fits in with the discussion given in this paragraph. Sorry to be repetitive (and yes, I am a co-author), but again this is discussed in Berrisford et al.(2011). See Figure 8 of this paper for a summary. [Adrian Simmons, United Kingdom]	Noted - This further supports the global mean energy balance estimates in Figure 12.1. Yet we have no space to explicitly mention ERA-Interim here once more, as we now already explicitly mention ERA interim with respect to its shortwave and longwave surface fluxes in the revised version.
2-861	2	22	48	22	48	. The magnitude -> but the magnitude [Ileana Bladé, Spain]	Accepted - but sentence removed
2-862	2	22	50	22	50	Space missing in "istowards" [Birgit Hassler, United States of America]	Accepted - but sentence reformulated
2-863	2	22	51	22	51	Wild et al. Submitted, update or skip [European Union]	Wild et al. (2013) has been updated (published online in Climate Dynamics 13 November 2012).
2-864	2	22	52	22	52	Please quantify the uncertainty in SH: 25% [Ileana Bladé, Spain]	Noted - we prefer to leave it here as a qualitative statement, as due to the lack of an adequate observational system, even the uncertainties on the uncertainty ranges are considerable
2-865	2	22	53	22	53	constains -> constraints [Mihai Dima, Romania]	Accepted, changed as suggested
2-866	2	22	55	23	49	There is no good evidence at all, as claimed lines 6-7 p 23, that net TOA radiation increased by 1.4 W m-2: that change is easily explained by a discontinuity in the record when the battery was changed on the instrument as discussed in AR4. The discussions of changes here have no basis. I do not know why the ISSCCP data are discussed here at all as they are known to contain major errors; see for instance Mayer, M., and L. Haimberger, 2011: Poleward atmospheric energy transports and their variability as evaluated from ECMWF Reanalysis data. J. Climate, 25, 734-752. [Kevin Trenberth, United States of America]	Taken into account - we skipped the related discussion as it has been extensively covered in AR4. 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 Trenberth's 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 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

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							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 are incorrect.
2-867	2	22		26		A number of words run together, eg: uncertainthan criticalinformation asomewhat toexceed istoward etc [Michael Coffey, United States of America]	Accepted, corrected as suggested
2-868	2	23	1	23	1	forcus --> focus [Claudio Cassardo, Italy]	Accepted, corrected as suggested
2-869	2	23	1	23	1	forcus -> focus [Mihai Dima, Romania]	Accepted, changed as suggested
2-870	2	23	1	23	1	Typo in "forcus" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-871	2	23	1	23	24	Acronym HIRS is spelt out the second time it appears in this paragraph, not the first. [Adrian Simmons, United Kingdom]	Accepted, changed as suggested
2-872	2	23	1		24	All of the changes in radiative flux quantities reported here need to have uncertainties associated with them. My guess is that a full uncertainty analysis will show uncertainties greater than the reported changes. [Stephen E Schwartz, United States of America]	Taken into account - Wong et al (2006) (Appendix B) is the only reference with uncertainties. They claim an uncertainty of 0.4 Wm-2 in 15-year tropical mean radiation changes. However, we skipped the discussion of the Wong et al. (2006) results, which the comment refers to, since it has been already extensively covered in AR4.
2-873	2	23	1			"forcus" --> "focus" [Richard Allan, United Kingdom]	Accepted, corrected as suggested
2-874	2	23	1			replace forcus=focus [Michael Coffey, United States of America]	Accepted, corrected as suggested
2-875	2	23	3	8	3	TSI is now discussed in section 8.4.1 [Gunnar Myhre, Norway]	Accepted, corrected as suggested
2-876	2	23	4	23	4	Space missing in "and1990s" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-877	2	23	8	23	16	Given that the ISCCP and ERBS-CERES time-series is not homogenous I question the amount of text devoted to discussion of these trends and would suggest a modification of "Since AR4...thermal emission" to the following: "Extending these datasets to the present suggests a continuation of these trends (e.g. Andronova et al. 2009; Harries and Belotti 2010 [http://dx.doi.org/10.1175/2009JCLI2797.1]) although the use of ISCCP and combined ERBS-CERES data is subject to inhomogeneity." [Richard Allan, United Kingdom]	Taken into account - this text was significantly revised and reads now "The extended records from CERES suggest no noticeable trends in either the tropical or global radiation budget during the first decade of the 21st Century (e.g. Andronova et al. 2009; Harries and Belotti 2010; Loeb et al., 2012a,b)."
2-878	2	23	12	23	12	Make it clear that this 6 Wm-2 estimate is also for the tropics. [Ileana Bladé, Spain]	Accepted -, but we've removed this in response to other reviewer comments.
2-879	2	23	13			Does 'full solar and thermal spectrum' mean visible and infrared. What is full solar? [Michael Coffey, United States of America]	Noted, solar and thermal spectrum is synonym to shortwave and longwave. It is, however not synonym to visible and infrared, as solar covers the UV, visible and near-infrared range of the spectrum, while thermal refers to the far-infrared part of the spectrum. The related sentence has, however, been removed.
2-880	2	23	15	23	16	Shouldn´t this sentence breaking down the two contributions (3 and 1 Wm-2) go after the earlier sentence detailing the change in net TOA radiation (line 10) ? [Ileana Bladé, Spain]	Noted - The sentence was removed in response to another reviewer's comment
2-881	2	23	17	23	17	Why is a longer description of the HIRS data given here rather than above (line 11) when these data are first mentioned ? [Ileana Bladé, Spain]	Accepted - however, in the revised text, the earlier reference to HIRS has been removed.
2-882	2	23	19	2	19	I may be reading this wrong, but if the HIRS thermal fluxes are higher than in ERBS/CERES after 1998	Noted - HIRS does not provide net radiation as there

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						shouldn´t the HIRS net radiation be smaller instead of larger? [Ileana Bladé, Spain]	is no information in the shortwave
2-883	2	23	26	22	26	Change "suggestthat" to "suggest that" [Government of Brazil]	Accepted, changed as suggested
2-884	2	23	26	23	26	suggestthat -> suggest that [Mihai Dima, Romania]	Accepted, changed as suggested
2-885	2	23	26	23	26	Space missing in "suggestthat" [Birgit Hassler, United States of America]	Accepted, changed as suggested
2-886	2	23	26	23	28	It seems odd to have a single sentence paragraph. Shouldn´t it be tied to the earlier discussion of CERES results? [Ileana Bladé, Spain]	Accepted - We've moved the single sentence paragraph to the preceding paragraph, but revised that single sentence.
2-887	2	23	26	23	28	In addition to citing the (Loeb et al 2012) results based on 21st-century CERES data, and in line with my comment above regarding page 5, line 40, one might again note that, apart from the period following the Pinatubo eruption, and contrary to claims based on earthlight data, no significant global mean albedo variations have been shown to exist (Wielicki et al. 2005, Kandel & Viollier 2010), . This is consistent with earlier ERBE and ScaRaB data. [Robert Kandel, France]	Taken into account - The reviewers' comment is consistent with the material presented in this revised paragraph. The Earthshine issue was discussed in detail in AR4. No further discussion on this has appeared in the literature since. Therefore, there is nothing more to report on this topic.
2-888	2	23	27	23	28	Loeb et al (2012) does not explicitly show the exchange in Planetary albedo - in fact figure 2a does suggest a trend is absorbed solar radiation (ASR). [European Union]	Noted - There are no statistically significant trends in either absorbed solar radiation or planetary albedo.
2-889	2	23	31			We suggest to replace "larger heat capacity" by "larger effective heat capacity" since it does not target the sole capacity of the different materials these surfaces are made of but it concerns the different depth of propagation of the warming as well. [Government of France]	Accepted- changed as suggested.
2-890	2	23	33	23	49	Trenberth and Fasullo (2010) showed there was missing eneregy in 2008-09 in the sense that the inventory of energy in the climate system did not match the TOA variations from CERES. Loeb et al (2012) published an alternative view which was flawed in the error estimates by including a systematic component in the error bars of interannual changes. The energy was in fact still missing. That paper was also duplicitous in that it used the ERA-interim TOA values but with a large bias adjustment since the actual values average about -2 Wm-2. Moreover none of the reanalyses include aerosol effects such as from Pinatubo. The Figure 2.13 is NOT from Loeb et al (2012) as the CERES curve there is not what was published. There is no statement about how this figure was adjusted to appear to make good agreement when it is in fact poor at times. I will give a paper on this at AGU and we are preparing a paper on this topic. In fact there is already a paper by Loeb et al 2012 in a volume published by Springer from an ISSI workshop that shows how in La Nina conditions, being colder, there is less OLR, and more net radiation into the system. So the radiation is responding to the temperatures not a cause. Rather the extra energy goes elsewhere, and increasing evidence shows it is going into the deeper ocean: it is not missing in the ECMWF ORAS4 ocean reanalysis. While IPCC may not have all the references in time, they should anticipate forthcoming science and not be shown to be immediately wrong! [Kevin Trenberth, United States of America]	Taken into account - Figure 2.13 has been revised. The Trenberth and Fasullo (2010) paper claim that prior to 2004, variations in satellite net radiation (e.g., CERES) were consistent with variations in in-situ derived upper-ocean heating rate, but after 2004, the two diverged from one another. They use the difference after 2004 to argue that there is "missing energy" in the system. That is, after 2004, more energy was being stored deeper in the ocean. The Loeb et al (2012) paper pointed out the weaknesses in the TF10 paper. Firstly, TF10 did not provide any error bars or assessment of the uncertainties in either the satellite or ocean data. Second, they ignored the fact that there was a very fundamental change in the ocean in-situ observing system, from XBT prior to 2004 to ARGO after 2004, so they were not using a homogeneous record (they made the same mistake with the satellite observations too). The Loeb et al paper show that when deriving ocean heating rate over short time scales, the errors are huge. In fact, Fig. 1b in Loeb et al (2012) shows that there is no significant difference between the 1993-2003 and 2004-2008 ocean heating rates when a proper error analysis is performed. Because these are different observing systems, there is no cancellation of systematic error, as implied by the reviewer with respect to the CERES data. The Loeb et al (2012) paper does not claim there is no missing energy, it merely points out that the observational uncertainties are too large to make such a claim. The figure 2.13 is revised in the new version to more clearly show the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							differences between CERES and 3 independent analysis of OHRs.
2-891	2	23	34			From Figure 2.12 the net TOA radiation absorbed by the climate system (atmosphere and surface) is 340-100=240 W m-2. [Michael Coffey, United States of America]	This comment belongs together with comment 2-892 and the response is given there.
2-892	2	23	34			In AR4 (Figure 1 of FAQ 1.1) the net TOA is 342-107=235 W m-2. A 5 W m-2 change. Do satellite observations show a 1 W m-2 increase from 2003 to 2008? [Michael Coffey, United States of America]	Noted - The new global means in Figure 2.12 are based upon the latest satellite observations. The 107 Wm-2 value is inconsistent with any satellite-based observation.
2-893	2	23	36	23	36	. Loeb et al point out that the apparent decline -> , although Loeb et al point out that this apparent decline [Ileana Bladé, Spain]	Taken into account - this part was reformulated.
2-894	2	23	37	23	37	Differences -> Additionally, differences [Ileana Bladé, Spain]	Taken into account - this part was reformulated.
2-895	2	23	38	23	38	The variability -> The interannual variability [Ileana Bladé, Spain]	Accepted -changed as suggested.
2-896	2	23	38	23	41	It seems Figure 2.13 is a complicated figure that deserves more than one sentence …. Because of this lack of detail, it is not readily clear to me how the figure undermines the point made by Trenberth and Fasullo (2010). The sentence is directly extracted from the abstract of the Loeb et al (2012) paper and the crucial point that the analysis suggests that energy storage is ocean heat content has continued to increase is not explicitly made. [Ileana Bladé, Spain]	Taken into account - We have revised Fig. 2.13 and expanded the discussion surrounding it. If changes in net radiation and OHC are consistent within error bars then there is no evidence for missing energy.
2-897	2	23	38	23	41	Figure 2.13 Can you improve the figure caption here and also the text that references the figure? The CERES TOA time series appears in both a and b - I'm assuming the two panels are separated so that the lines are clearer? Do you mean that the figure shows the variability in the energy imbalance which is due to ENSO or that in general, the variability in the energy imbalance is due to ENSO? There is no mention of ENSO in the figure caption if the former is the case. [Kate Willett, United Kingdom]	Taken into account - We have revised Fig. 2.13 and expanded the discussion surrounding it. It is now mentioned in the text that extra energy is received by the climate system in La Nina (e.g. 2009) and less in El Nino (e.g. 2010). When it is cold in La Nina, less thermal radiation is emitted and the climate system gains heat.
2-898	2	23	40			What is meant by "a reanalysis model simulation"? Does it mean a reanalysis (which is not a model simulation because observational values are assimilated to adjust the model-simulated values) or does it mean an AMIP or CMIP type of simulation run with a model that is also used for reanalysis. AMIP-type runs with reanalysis models are useful in that they say what the model would produce in the absence of assimilation of atmospheric observations (but with implicit assimilation of SST observations). The sentence should be clarified. [Adrian Simmons, United Kingdom]	Taken into account - We've removed the reanalysis part of the figure and the associated text.
2-899	2	23	43			Figure 2.13 This needs more description of what is being displayed. It is not actually net TOA radiation. What is ERA Iterim reanalysis. [Michael Coffey, United States of America]	Accepted - We have revised Fig. 2.13 and expanded the discussion surrounding it.
2-900	2	23	55			This is for land only. [Kevin Trenberth, United States of America]	Accepted - This is true, as long term records are only available over land. We revised any wording that could be ambiguous in this respect.
2-901	2	23	56	24	2	Clarification: Please replace 'Monitoring of radiative fluxes from surface stations' with 'Monitoring of radiative fluxes from land surface stations' to make it clear that such stations are not avilable for the ocean. [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	Accepted - we modified the sentence as suggested.
2-902	2	23	56	25	2	As noted above for line 30, virtually no open ocean long-term surface radiation budget measurements exist. [Robert Kandel, France]	Accepted - we revised this section to avoid any misinterpretation in this respect.
2-903	2	24	1	24	2	'also known as global radiation or surface solar radiation, hereafter referred to as SSR'. This terminology is confusing as surface stations are not available for the ocean. It would be better to use the term 'land surface solar radiation' and the acronym LSSR instead of SSR. [Simon Josey, United Kingdom of Great Britain &	Taken into account - we revised the section to make sure there is no misunderstanding that records are from land-based stations only. We did not expand the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Northern Ireland]	acronym further as it should be as concise as possible, and it would no longer be equivalent with "global radiation", the synonymous technical term, in that sentence.
2-904	2	24	4	24	23	There are numerous spelling and syntax mistakes in these two paragraphs -- please proof read. [Jeffrey Taylor, United States of America]	Taken into account, the missing blanks between some words seem to have been the result from a conversion of docx files to doc files and was not in the responsibility of the LAs.
2-905	2	24	4	24	55	This section emphasisizes "global dimming' and 'global brightening" but in neither case was it global. At best it was on parts of land. The rhetoric here is excessive. There is nothing here on ocean areas in spite of a number of ocean buoys measuring radiation. The whole section should be written much more carefully and refer to the recent literature. It should relate to changes in cloud and aerosols and also DTR [Kevin Trenberth, United States of America]	Taken into account - We do not make a statement on the global mean change in surface solar radiation here, as ocean areas are not covered by direct observations, as we explicitly stated in the summary section 2.3.4 in SOD: "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". We document changes as seen in records obtained from the radiation networks in place over land. We revised any wording that might be ambiguous in this respect. Buoy data do not have the required quality nor record length to allow for trend estimates, The relation to changes in clouds, aerosol and DTR is covered in Section 2.3.3.3.
2-906	2	24	4			It appears that some of surface sites show downward trend of solar radiation (SSR) before 1980s and increasing trend after 1980s. This section reads as if the trends appear in all surface observations. How well does the SSR record observed at Potsdam shown in Figure 2.14 represent all other sites? Are there any surface sites that do not show such trends? If so, these decreasing and increasing trends are regional. In addition, these SSR trends derived from satellite over land do not seem to agree with surface observations (page 2-25 line 8) despite the fact that all surface sites are over land. If the authors agree, please also state that these trends are regional in the executive summary (page 2-3 line 47). [Government of United States of America]	Taken into account - We state that "a decline of SSR .... has been observed at many land-based sites", thus we do not claim that all sites show exactly this behavior. Therefore, also the variations seen in Potsdamm do not represent all other sited, but again are observed in a similar way at many of them. There are also sites that show a different behaviors, such as sites in India, which show a continuous dimming with no trend reversal. This is also accounted for in the executive summary, where we mention that these variations are seen at many observation sites, and thus do not claim that this applies to every single record, or that it is a global trend. We explicitly state in the summary section 2.3.4. "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". Satellite derived trends of surface solar radiation are very inconsistent in different products, due to the problems of a proper representation of the spatio-temporal variations in aerosol.
2-907	2	24	6	24	10	Correct text in parentheses in sentence "Specifically, a decline…" [Birgit Hassler, United States of America]	Accepted, duplication removed
2-908	2	24	6	24	55	Surface downward solar radiation is closely coupled with cloudiness. The so called "global dimming" was based on fewer than 100 stations with SSR data since the 1960s primarily in Europe and the U.S., while there are much more cloud observations. Anyone studied cloudiness spatial variations would agree that data from 100 stations from Europe and the U.S. would be insufficient to any inference about global cloudiness changes and thus global SSR changes. The IPCC report should not contribute to this misleading concept of "global dimming" and should discuss SSR changes in connection with cloudiness trends discussed in the same chapter. People do not claim that there is a global trend in cloudiness even though many land areas show	Taken into account - we do not claim that the trends observed at the surface solar radiation stations are of global nature, as too many areas are not covered by direct observations. We explicitly stated in the summary section 2.3.4 of the SOD "Over some remote land areas and over the oceans, confidence is low due to the lack of direct observations, which

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						increaed cloud cover since 1950, because 2/3 of the global surface (oceans) have no data. This should apply to the SSR trends as well. [Aiguo Dai, United States of America]	hamper a true global assessment". The trends found at the land-based radiation sites are, however, in line with related other quantities that have a better coverage over land surfaces, such as sunshine duration, DTR and pan evaporation. The connection with these variables is discussed at the end of Section 2.3.3.1, and the connection with aerosol and cloudiness in Section 2.3.3.3
2-909	2	24	7	24	7	many -> many (but not all) [Ileana Bladé, Spain]	Noted - "Many" implies that these are not all, otherwise we would put "all" in the first place. In fact it is not easy to find regions with sufficient station coverage that do not follow this tendency of decline between the 1950s and 1980s (see e.g. Gilgen et al. 1998, J. Climate).
2-910	2	24	7	24	7	repeated twice "(popularly known as 'global dimming'" [Claudio Cassardo, Italy]	Accepted, duplication removed
2-911	2	24	7	24	9	Editorial correction: same phrases repeated twice (popularly know as 'global dimming') - is this necessary? [Government of Australia]	Accepted, duplication removed
2-912	2	24	7	24	9	Please verify the sentence: "…1980s has been observed at many land-based sites (popularly known as 'global dimming' (popularly known as "global dimming", Liepert, 2002; Stanhill and Cohen, 2001), as well as a partial recovery from the 1980s onward ('brightening' ("brightening", Wild et al., 2005)", since there are repetitions of several words. [Rubén D Piacentini, Argentina]	Accepted, duplication removed
2-913	2	24	7		7	popularly known as global dimming' is repeated [Anne Verhoef, United Kingdom]	Accepted, duplication removed
2-914	2	24	7			Remove "(popularly known as 'global dimming'" - duplication. [Richard Allan, United Kingdom]	Accepted, duplication removed
2-915	2	24	7			Remove redundant "global dimming". [Michael Coffey, United States of America]	Accepted, duplication removed
2-916	2	24	9	24	9	repeated twice "brightening" [Claudio Cassardo, Italy]	Accepted, duplication removed
2-917	2	24	9	24	9	2x "brightening" [European Union]	Accepted, duplication removed
2-918	2	24	9		9	brightening' is repeated [Anne Verhoef, United Kingdom]	Accepted, duplication removed
2-919	2	24	9			Remove "('brightening'" - duplication (unless it is my old PDF reader!) [Richard Allan, United Kingdom]	Accepted, duplication removed
2-920	2	24	14	24	14	SRRbrightening -> SRR brithening [Mihai Dima, Romania]	Accepted, corrected as suggested
2-921	2	24	14	24	14	Space missing in "SSRbrightening" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-922	2	24	16	24	16	Split 'decade' and 'and' [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-923	2	24	16	24	16	decade and (space between words missing). [Christian-D. Schoenwiese, Germany]	Accepted, corrected as suggested
2-924	2	24	23	24	23	Split 'noted' and 'a' [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-925	2	24	23	24	23	(2009)noteda -> (2009) noted a [Mihai Dima, Romania]	Accepted, corrected as suggested
2-926	2	24	23	24	23	Spaces missing in "(2009)noteda" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-927	2	24	23	24	30	Can´t all these studies which all reveal a pattern of significant brightening until the 80s followed by dimming at different locations be grouped together in one sentence? [Ileana Bladé, Spain]	Taken into account: - once sentence only to summarize this paragraph is not feasible, but we revised this paragraph to make it more concise.
2-928	2	24	23			space needed between noted and a [Karen Rosenlof, United States of America]	Accepted, corrected as suggested
2-929	2	24	31	24	31	Reference of recent paper by Soni et al (2012) may be included while citing solar dimming over Indian region	Accepted- reference Soni et al. (2012) has been

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						along with Kumari and Goswami (2010). Soni et al (2012) also found that the trend analysis of bright sunshine duration suggests long-term decrease at all the stations. Reduction in bright sunshine hours varied from 0.0 to 0.5 h per decade. [Government of India]	added.
2-930	2	24	34			How do changing calibration processes and instruments affect the trend over such a long time period? What is the uncertainty in SSR observed by pyranometers in 1950s? [Government of United States of America]	Taken into account - A paragraph is devoted to measurement and data quality issues and the related reference list has been further complemented.
2-931	2	24	39	24	43	Can current dimming trends be linked back to aerosol trends discussed in 2.2.3.1? [Richard Allan, United Kingdom]	Taken into account - The SOD already contained a discussion of the relation of changes in surface solar radiation in section 2.3.3.3, but this subsection is now further expanded to discuss the relation to the post 2000 aerosol trends from MODIS + MISR and a link to Section 2.2. is added.
2-932	2	24	42	24	42	This sentence is very vague. Do these data indicate renewed dimming everywhere, particularly in the SH ? Do they contradict the earlier quoted results for Europe, Asia, etc ? Or do you mean "renewed dimming in certain places" ? [Ileana Bladé, Spain]	Taken into account - the sentence has been revised to emphasize the dimming on the SH.
2-933	2	24	42	24	42	Space required after second comma [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-934	2	24	42	24	42	SH,is -> SH is [Mihai Dima, Romania]	Accepted, corrected as suggested
2-935	2	24	45	24	45	A number of issues remain -> A number of issues have been invoked with regards to the above discrepancies, [Ileana Bladé, Spain]	Noted, we prefer to original formulation.
2-936	2	24	46	24	24	One sentence should be added after " (Wild et al., 2012)." Instrument replacement has produced inhomogeneity of SSR data in China from 1990 to 1993 (Wang et al., 2012). Reference: Wang, K.C., Dickinson, R.E., Wild, M. and Liang, S., 2012. Atmospheric impacts on climatic variability of surface incident solar radiation. Atmos. Chem. Phys., 12(20): 9581-9592. [Kaicun Wang, China]	Taken into account - We added the reference of Wang et al. 2012 to the references referring to data quality issues.
2-937	2	24	52	24	52	is -> was [Ileana Bladé, Spain]	Accepted, corrected as suggested.
2-938	2	24	52	24	52	. The magnitude -> but the magnitude [Ileana Bladé, Spain]	Noted - we do not see a necessity to change the sentence.
2-939	2	24	55	24	55	Wang et al., 2012b should be: Wang, K.C., Dickinson, R.E., Wild, M. and Liang, S., 2012. Atmospheric impacts on climatic variability of surface incident solar radiation. Atmospheric Chemistry and Physics, 12(20): 9581-9592. [Kaicun Wang, China]	Accepted, this is an Endnote problem that is beyond control of the LAs and needs to be fixed.
2-940	2	24		26		Numerous small mistakes, typos, and duplicate words. [Government of France]	Taken into account, the missing blanks between some words seem to have been the result from a conversion of docx files to doc files and was not in the responsibility of the LAs. These errors have been corrected
2-941	2	25	1	25	2	'While extended areas of the globe are not covered by surface measurements and hamper a true global assessment, satellite-derived fluxes can provide a near global picture.' The first half of the statement is again insufficiently clear by not referring explicitly referring to the ocean. It would be better to say 'While major areas of the globe are not covered by surface measurements (including the 2/3 of the surface covered by the ocean) and thus prevent a true global assessment, satellite-derived fluxes can provide a near global picture.' [Simon Josey, United Kingdom of Great Britain & Northern Ireland]	Taken into account - With the revision of the section that ensures the strict usage of land surface sites this should be obvious, and with severe space restriction we did not expand to mention that 2/3 of the Earth surface is covered by oceans, as this is assumed to be common knowledge. We also note in the summary section "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." Therefore this point should be well covered.
2-942	2	25	6	25	6	globallyas -> globally as [Mihai Dima, Romania]	Accepted, corrected as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-943	2	25	6	25	6	Space missing in "globallyas" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-944	2	25	6	25	6	globally as (space between words missing). [Christian-D. Schoenwiese, Germany]	Accepted, corrected as suggested
2-945	2	25	6	25	7	spaces between words are missing [Jeffrey Taylor, United States of America]	Accepted, corrected as suggested
2-946	2	25	7	25	7	decade(Hatzianastassiou -> decada (Hatznianastassiou [Mihai Dima, Romania]	Accepted, corrected as suggested
2-947	2	25	8	25	8	depending on the respective satellite product -> depending on the satellite product [Ileana Bladé, Spain]	Noted - We do not see a necessity to remove the word "respective".
2-948	2	25	13	25	20	spaces between words are missing [Jeffrey Taylor, United States of America]	Accepted, corrected as suggested
2-949	2	25	13	25	28	In the paragraph from lines 13 to 28: suggest there should be some comment on the new result from Wang et al 2012 that incident surface solar radiation averaged over NH land area was +0.87W/sq m per decade over the period 1982-2008 due to changes in atmospheric transparency.. Taken at face value it would seem to dominate CO2 forcing. This result could explain the rapid NH land warming from 1980 and, at least in part, the apparent disconnect between solar irradiance and global temperature since about 1980. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account - we added a related comment to include these results.
2-950	2	25	13			This section on surface solar radiation (SSR) trends claims consistency with diurnal temperature range (DTR) trends (last paragraph of section), 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. [Government of United States of America]	Taken into account. A sentence has been added that the DTR trends have only medium confidence. Both datasets indeed have not been assessed to the full extent possible with respect to potential inhomogeneities in the existing literature. Unpublished results however show that the main conclusions drawn here are not affected by the homogenization process. While we cannot cite those unpublished results we feel no necessity to dismiss this point entirely, but add the caveat with respect to the confidence level to address these concerns.
2-951	2	25	14	25	14	increasetheir -> increase their [Mihai Dima, Romania]	Accepted, corrected as suggested
2-952	2	25	14	25	14	Space missing in "increasetheir" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-953	2	25	16	25	16	Specifically, SSR dimming --> specifically, evidence for SSR dimming [Ileana Bladé, Spain]	Accepted - we modified the sentence as suggested.
2-954	2	25	16	25	18	Although Roderick et al. (2007) GRL suggests that a "stilling" effect (reduced wind speed) may have also contributed to decreases in pan evaporation. [Richard Allan, United Kingdom]	Noted, due to space constraints we cannot further elaborate on a discussion of all possible causes of trends in variables other than radiation, which is the focus of this section. We do not claim that the decline in radiation is the unique exclusive cause for the trends in these other variables.
2-955	2	25	17	25	17	dimmingfrom -> dimming from [Mihai Dima, Romania]	Accepted, corrected as suggested
2-956	2	25	17	25	17	Space missing in "dimmingfrom" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-957	2	25	17		17	Place space between 'dimming' and 'from' [Anne Verhoef, United Kingdom]	Accepted, corrected as suggested
2-958	2	25	18	25	18	Should add some detail on this DTR trend reversal to reveal consistency: "the apparent trend reversal of DTR (from a decrease to increase) …" [Ileana Bladé, Spain]	Noted- but not expanded due to space constraints
2-959	2	25	18	25	20	Later in section 2.4.1.2 it is said that our confidence in global DTR changes is only low to medium, so using DTR data here without some words of caution seems somewhat inconsistent. Of course one can argue that consistency with the observed changes in dimming/brigthening strengthens the evidence for both the DRT and the SSR changes, but the problems with the DRT data should still be aknowledged. [Ileana Bladé, Spain]	Accepted, A caveat on the confidence level has been added. See also extended response to comment 2-950 which raises the same concerns.
2-960	2	25	20	25	20	brighteningis -> brightening is [Mihai Dima, Romania]	Accepted, corrected as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-961	2	25	20	25	20	Space missing in "brighteningis" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-962	2	25	20		20	Place space between 'brightening' and 'is'' [Anne Verhoef, United Kingdom]	Accepted, corrected as suggested
2-963	2	25	21	25	22	Repalce Wang et al., submitted with Wang et al. (2012). Reference: Wang, K.C., Dickinson, R.E., Wild, M. and Liang, S., 2012. Atmospheric impacts on climatic variability of surface incident solar radiation.Atmospheric Chemistry and Physics, 12(20): 9581-9592. [Kaicun Wang, China]	Accepted - the reference has been updated.
2-964	2	25	26	25	28	This should relate more to literature on changes in aerosols. An example is Wang, K., R. E. Dickinson, L. Su, and K. E. Trenberth, 2012: Contrasting Trends of Mass and Optical Properties of Aerosols over the Northern Hemisphere from 1992 to 2011. Atmospheric Chemistry and Physics, 12, 9387-9398, doi:10.5194/acp-12-9387-2012. and references therein. [Kevin Trenberth, United States of America]	Noted- the relation to aerosol is not covered in this paragraph, but in Section 2.3.3.3. The paragraph here discusses related variables that have a better spatial and temporal coverage than direct radiation measurements and may contain information that can be used to improve the spatio-temporal information on surface solar radiation changes. Aerosol observations on the other hand have much shorter records than radiation measurements and therefore are not useful for the purpose described in this paragraph.
2-965	2	25	27	25	27	Wang et al., 2012b should be: Wang, K.C., Dickinson, R.E., Wild, M. and Liang, S., 2012. Atmospheric impacts on climatic variability of surface incident solar radiation.Atmospheric Chemistry and Physics, 12(20): 9581-9592. [Kaicun Wang, China]	Accepted - this is an Endnote problem which needs to be fixed but is beyond our control.
2-966	2	25	30	25	30	instead of just Potsdam show figure 9 from Wang et al which shows solar radiation over several regions. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted, as this is the observations chapter, we intend to present as much as possible directly observed quantities, and not (empirically) derived quantities. Wang et al. present records reconstructed from sunshine duration measurements. Further, the records presented in Wang et al. start not before the 1980s, covering only a small fraction of the time period discussed in this Section, while the Potsdam record starts in 1937, illustrating in addition the dimming and early brightening phases discussed in the text body.
2-967	2	25	34			I thought that even with very good observations it is difficult to close the surface energy beudget (eg Roode et al, 2010, doi:10.1007/s10546-010-9476-1), this is not mentioned in this section. [Geert Jan van Oldenborgh, Netherlands]	Noted - the closure of the energy budget is yet another problem, but it is the temporal variation of its individual terms that is the focus of this subsection. The controversial discussion on the closure of the surface energy budget, particularly to reach consistency between the radiative and non-radiative fluxes is discussed in subsection 2.3.1.
2-968	2	25	36		52	Several changes are suggested in this paragraph and it is rewritten accordingly. Please also add the 3 additional references. [Rolf Philipona, Switzerland]	Taken into account - the 3 references were added and the suggested additional text included, albeit in an abbreviated form due to space limitations.
2-969	2	25	36		52	Long-term measurements of the thermal surface components as well as surface net radiation are available at far fewer sites than SSR. Downward thermal radiation observations started to become available during the early 1990s with the initiation of the BSRN and the ASRB networks and at a limited number of worldwide distributed sites. With data from the ASRB network Philipona et al. (2004) and Philipona et al., (2005) measured increasing downward thermal fluxes since the mid-1990s in the Swiss Alps, corroborating an increasing greenhouse effect. Wild et al. (2008) determined an overall increase of 2.6 W m–2 per decade over the 1990s using BSRN data, in line with model projections and the expectations of an increasing greenhouse effect. For mainland Europe Philipona et al., (2009) show an increase of longwave downward radiation of 2.4 to 2.72 Wm-2 per decade for the period 1981-2005. Wang and Liang (2009) inferred an increase in downward thermal radiation of 2.2 W m–2 per decade over the period 1973–2008 from observations of temperature,	Taken into account - the 3 references were added and the suggested additional text was taken into account, albeit in an abbreviated form due to space limitations.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						humidity and cloud fraction. Prata (2008) estimated a slightly lower increase of 1.7 W m–2 per decade for clear sky conditions over the earlier period 1964–1990, based on radiative transfer calculations using observed temperature and humidity profiles from radiosondes. A contribution from anthropogenic chlorofluorocarbons (CFCs) to the downward thermal radiation has been documented in spectral atmospheric radiation measurements by Evans and Puckrin(1995).There is limited information on changes in surface net radiation, in large part because measurements of upward fluxes at the surface are made at only a few sites and are not spatially representative. Wild et al. (2008; 2004) inferred a decline in land surface net radiation on the order of 2 W m–2 per decade from the 1960s to the 1980s, and an increase at a similar rate from the 1980s to 2000, respectively, based on estimated changes of the individual surface radiative components. Philipona et al., (2009) and Philipona (2012) show an increase of total net radiation (shortwave plus longwave) of 1.3 to 2 Wm-2 per decade, from measurements in central Europe and the Alps over the period 1981-2005. [Rolf Philipona, Switzerland]	
2-970	2	25	36		52	Ref: [Rolf Philipona, Switzerland]	See responses to 2-971, 2-972, 2-973
2-971	2	25	36		52	Philipona, R., Dürr, B., Ohmura, A., and Ruckstuhl, C., 2005. Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe. Geophysical Research Letters, 32, L19809. [Rolf Philipona, Switzerland]	Accepted - reference added
2-972	2	25	36		52	Philipona, R., Behrens, K., and Ruckstuhl, C., 2009. How declining aerosols and rising greenhouse gases forced rapid warming in Europe since the 1980s. Geophysical Research Letters, 36, L02806. [Rolf Philipona, Switzerland]	Accepted - reference added
2-973	2	25	36		52	Philipona, R., 2012. Greenhouse warming and solar brightening in and around the Alps. International Journal of Climatology. Doi:10.1002/joc.3531. [Rolf Philipona, Switzerland]	Accepted - this paper is now also in the reference list.
2-974	2	25	36		52	Long-term measurements of the thermal surface components as well as surface net radiation are available at far fewer sites than SSR. Downward thermal radiation observations started to become available during the early 1990s with the initiation of the BSRN and the ASRB networks and at a limited number of worldwide distributed sites. With data from the ASRB network Philipona et al. (2004) and Philipona et al., (2005) measured increasing downward thermal fluxes since the mid-1990s in the Swiss Alps, corroborating an increasing greenhouse effect. Wild et al. (2008) determined an overall increase of 2.6 W m–2 per decade over the 1990s using BSRN data, in line with model projections and the expectations of an increasing greenhouse effect. For mainland Europe Philipona et al., (2009) show an increase of longwave downward radiation of 2.4 to 2.72 Wm-2 per decade for the period 1981-2005. Wang and Liang (2009) inferred an increase in downward thermal radiation of 2.2 W m–2 per decade over the period 1973–2008 from observations of temperature, humidity and cloud fraction. Prata (2008) estimated a slightly lower increase of 1.7 W m–2 per decade for clear sky conditions over the earlier period 1964–1990, based on radiative transfer calculations using observed temperature and humidity profiles from radiosondes. A contribution from anthropogenic chlorofluorocarbons (CFCs) to the downward thermal radiation has been documented in spectral atmospheric radiation measurements by Evans and Puckrin(1995).There is limited information on changes in surface net radiation, in large part because measurements of upward fluxes at the surface are made at only a few sites and are not spatially representative. Wild et al. (2008; 2004) inferred a decline in land surface net radiation on the order of 2 W m–2 per decade from the 1960s to the 1980s, and an increase at a similar rate from the 1980s to 2000, respectively, based on estimated changes of the individual surface radiative components. Philipona et al., (2009) and Philipona (2012) show an increase of total net radiation (shortwave plus longwave) of 1.3 to 2 Wm-2 per decade, from measurements in central Europe and the Alps over the period 1981-2005. [Rolf Philipona, Switzerland]	This comment is identical to comment 2-969. See response there.
2-975	2	25	37	25	37	Split 'available' and 'during' [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-976	2	25	37	25	37	availableduring -> available during [Mihai Dima, Romania]	Accepted, corrected as suggested
2-977	2	25	37	25	37	Change "availableduring" to "available during" [Government of Brazil]	Accepted, corrected as suggested
2-978	2	25	37	25	37	Space missing in "availableduring" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-979	2	25	37		37	Place space between 'available' and 'during' [Anne Verhoef, United Kingdom]	Accepted, corrected as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-980	2	25	37			space needed after available [Karen Rosenlof, United States of America]	Accepted, corrected as suggested
2-981	2	25	44			Increasing the downward thermal flux at limited ground sites is not necessarily due to an increasing greenhouse effect. The thermal flux can increase due to increasing near surface air temperature. Surface temperature can change due to advection change caused by a large scale dynamics change. Suggest that the authors reconsider this statement. [Government of United States of America]	Taken into account - a related comment has been added at the beginning of section 2.3.3.2
2-982	2	25	48	25	42	It is first stated that measurements of surface net radiation are only available at a few sites but then a result is quoted (a decline of 2 Wm-2 from the 60s to the 80s) without specifying where which is perplexing. [Ileana Bladé, Spain]	Taken into account - These estimates were not based on direct measurements of the surface net radiation (also known as surface radiation balance), which are indeed sparse, but by estimates for the changes in the individual components that make up the surface net radiation. The sentence has been expanded to make this point more clear.
2-983	2	25	51	25	51	Split 'decade' and 'from' [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-984	2	25	51	25	51	decadefrom -> decade from [Mihai Dima, Romania]	Accepted, corrected as suggested
2-985	2	25	51	25	51	Change "decadefrom" to "decade from" [Government of Brazil]	Accepted, corrected as suggested
2-986	2	25	51	25	51	decade from (space between words missing) [Christian-D. Schoenwiese, Germany]	Accepted, corrected as suggested
2-987	2	25	51		51	Place space between 'decade' and 'from' [Anne Verhoef, United Kingdom]	Accepted, corrected as suggested
2-988	2	25				Words run together in several places [Elizabeth Kent, United Kingdom]	Accepted, corrected as suggested
2-989	2	26	0			That summary may disproportionally highlight the SSR trend. Consider including discussion of the significant advances that have been made for assessing the mean state of radiative flux at TOA and surface. Also that there can be discrepancies between surface net radiation and the sum of latent and sensible heat fluxes. [Government of United States of America]	Noted - Advances in the knowledge on the mean state of the TOA and surface radiative budgets are discussed in Section 2.3.1 (including a new Figure on the global mean energy balance). The controversial debate on discrepancies between surface radiative and nonradiative (sensible and latent heat) fluxes is also addressed in Section 2.3.1. Surface solar radiation is the radiative quantity that has been most widely analyzed with respect to trends in the literature appearing since AR4, particularly due to the availability of long-term direct observations. Reliable TOA records from satellite are still short (essentially starting around 2000) and thus hamper extended analyses of trends, the focal point of this section "Changes in Radiation". Therefore also the related literature is much smaller. This also applies to the surface radative components other than surface solar radiation, where the data base of long term records and the related literature covering changes in these quantities is also still small. Irrespectively of these points, knowledge on changes in surface solar radiation are important for the biosphere, the cryosphere, the hydrological cycle, as well as the rapidly growing solar power production industry, and thus largely affect climate and live on this planet. This should deserve some discussion in this context.
2-990	2	26	1	26	1	delete comma after 'aerosols' [Peter Burt, United Kingdom]	Accepted, corrected as suggested
2-991	2	26	4	26	6	The original texts: “While cloud cover changes were found to explain the trends in some areas (e.g., Liley, 2009), this is not always the case particularly in relatively polluted regions such as Europe and China (Norris	Accepted, corrected as suggested

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						and Wild, 2007; Norris and Wild, 2009; Qian et al., 2006; Wild, 2009; Wild, 2012)". In the original text, China, as an individual country, is listed with Europe, which is not an appropriate practice. Therefore, it is recommended to delete the text "if such as Europe and China", which we believe won't affect the conclusion. [Government of China]	
2-992	2	26	6		6	Please add following sentence before SSR dimming … [Rolf Philipona, Switzerland]	See response to 2-993
2-993	2	26	6		6	Ruckstuhl et al.,(2008) show that in Europe from 1981-2005 the direct aerosol effect under cloud free skies had a considerably larger effect on the observed brightening than the cloud related indirect aerosol effect. [Rolf Philipona, Switzerland]	Taken into account - the reference of Ruckstuhl et al. (2008) has been added.
2-994	2	26	12	26	12	Change "increasing" to "changing". [Government of Australia]	Accepted, corrected as suggested
2-995	2	26	13			Make link to section 2.2.3.1? [Richard Allan, United Kingdom]	Accepted, a related statement has been added.
2-996	2	26	13			Is the statement of aerosol trend here consistent with the statement on page 2-25 line 9 to 11? [Government of United States of America]	Taken into account - We reformulated the statement adding "background aerosol optical depth over oceans" here, as the statement on page 2-25 line 9-11 explicitly relates to trends over land which are less well established than over oceans.
2-997	2	26	15			The Mishchenko et al. 2007b result suggesting less scattering of sunlight due to increased aerosol after Pinatubo using AVHRR data has been shown likely to be a sampling artifact (Thomas et al. 2010 ACP) [Richard Allan, United Kingdom]	Noted, this statement is related to the reduction of aerosol optical depth after the recovery from Pinatubo, which is also found in the study by Cermak et al. (2010). They found declining aerosol optical depth during the 1990s consistently in two satellite-derived datasets.
2-998	2	26	18	26	30	spaces between words are missing [Jeffrey Taylor, United States of America]	Accepted, corrected as suggested
2-999	2	26	18	26	37	This is another Wild section in need of revision. [Kevin Trenberth, United States of America]	Taken into account, the section has been revised and the associated literature adjusted.
2-1000	2	26	19	26	19	changesin -> changes in [Mihai Dima, Romania]	Accepted, corrected as suggested
2-1001	2	26	19	26	19	Space missing in "changesin" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-1002	2	26	19	26	19	changes in (space between words missing); etc. (same error also on some other pages). [Christian-D. Schoenwiese, Germany]	Accepted, corrected as suggested
2-1003	2	26	23	26	24	The fact that SSR trends "are in line with observed decadal warming trends" could mean that (a) SSR has a major influence on warming, (b) warming has a major influence on SSR,(c) some other force has caused both. Your statement refers to just one of these options when aany of the 3 might apply. For example, the dominance of ENSO conditions on the El Nino side of SOI of zero would mean sustained Hadley Circulation, which would mean higher average flobal temperatures and warming in the mid latitudes would likely cause a reduction in cloud cover, which in turn would cause an increase in SSR. [John McLean, Australia]	Taken into account - we agree with the reviewer under the assumption that decadal SSR trends are caused by unforced natural variations in the climate system and related cloud changes. If they were to be dominated by aerosol direct and indirect effects, primarily case (a) would apply. The existing literature gives support for the latter, while we do not find much literature providing observational evidence for the other two cases (b) (c) which could be cited here (Note that we are not supposed to discuss modeling studies in this observational chapter). However, we added a statement to include the possibility of these other cases (b), (c) as well. Further, the expression "in line" does not imply a cause-effect and leaves room for all these cases.
2-1004	2	26	23			Changes in aerosol have also been implicated in driving decadal Atlantic temperature (Booth et al. 2012 Nature) [Richard Allan, United Kingdom]	Noted - This is correct, but we are not supposed to discuss findings based on modeling studies in this

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							observational chapter.
2-1005	2	26	25	26	25	2012).For -> 2012). For [Mihai Dima, Romania]	Accepted, corrected as suggested
2-1006	2	26	26	26	26	2009)find that adecline -> 2009) find that a decline [Mihai Dima, Romania]	Accepted, corrected as suggested
2-1007	2	26	26	26	26	Change "adecline" to "a decline" [Government of Brazil]	Accepted, corrected as suggested
2-1008	2	26	26	26	26	Space missing in "adecline" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-1009	2	26	30	26	30	methodshave -> methods have [Mihai Dima, Romania]	Accepted, corrected as suggested
2-1010	2	26	30	26	30	Change "methodshave" to "methods have" [Government of Brazil]	Accepted, corrected as suggested
2-1011	2	26	30	26	30	Space missing in "methodshave" [Birgit Hassler, United States of America]	Accepted, corrected as suggested
2-1012	2	26	30	26	32	Another single-sentence paragraph. Can´t it be tied to something ? [Ileana Bladé, Spain]	Rejected, a new paragraph is chosen here as this is now related to thermal radiation, while the paragraph before deals with solar radiation.
2-1013	2	26	30		30	Place space between 'methods' and 'have' [Anne Verhoef, United Kingdom]	Accepted, corrected as suggested
2-1014	2	26	32		32	Please add following sentence [Rolf Philipona, Switzerland]	This comment belongs to comment 2-1015
2-1015	2	26	32		32	In central Europe Philipona (2012) shows that at Lowland about 50% of the climate forcing is due to solar brightening and 50% due to rising greenhouse gases and water vapor feedback. In contrast in the Alps there is almost no solar brightening but a much larger water vapor feedback is observed. [Rolf Philipona, Switzerland]	Taken into account, a sentence related to the findings of this study has been added.
2-1016	2	26	32			I suggest inserting "leading to reduced net thermal cooling of the surface" after "the surface" since this highlights the important point that the surface cools less efficiently by thermal radiation for warmer temperatures. [Richard Allan, United Kingdom]	Accepted, corrected as suggested
2-1017	2	26	34	26	37	This paragraph is very broad and generalized and seems inconsistent with the material that preceeds it in this section. Also, if the statement is justified, it seems worthy of including in the Summary section (2.3.4) that follows. [Dian Seidel, United States of America]	Noted - we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.
2-1018	2	26	34			A suggestion to make this more precise: "The surface radiative fluxes provide energy for heating fluxes into the surface (which are dominated globally by the oceans due to their large heat capacity) and for surface turbulent fluxes including evaporation…." [Richard Allan, United Kingdom]	Noted - we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.
2-1019	2	26	35	26	36	scalesRamanathan -> scales Ramanathan [Mihai Dima, Romania]	Accepted, corrected as suggested
2-1020	2	26	36	26	36	A new comprehensive review of evapotranspiration should be cited here: Wang and Dickinson (2012). Reference: Wang, K.C. and Dickinson, R.E., 2012. A review of global terrestrial evapotranspiration: Observation, modeling, climatology, and climatic variability. Reviews of Geophysics, 50(2): RG2005. [Kaicun Wang, China]	Noted - but we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.
2-1021	2	26	36	26	37	The consistency between changes in surface radiation and changes in land precipitation seems like a very important point but it is almost glossed over. [Ileana Bladé, Spain]	Noted - we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.
2-1022	2	26	36		37	This last sentence is very surprising. Precipitation changes are very debated! [Rolf Philipona, Switzerland]	Noted - we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.
2-1023	2	26	36			The net radiation over land does not balance with the precipitation over land since most water vapor precipitated over land is advected from ocean. Suggest replacing this statement with “the agreement of interannual variability of the global annual mean net surface radiative flux and the sum of sensible and latent heat fluxes needs to be investigated” (unless such studies have been published recently). [Government of United States of America]	Noted - we eliminated this paragraph as there is not enough space for a differentiated treatment of this issue.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1024	2	26	41	26	42	The conclusion should not vaguely refer to "a continuation of the decadal variations in the tropical radiation budget" but should explicitly indicate that there has been a sustained increase in reflected radiation, a lesser increase in emmited thermal radiation and thus an increase in net TOA radiation. [Ileana Bladé, Spain]	Accepted - we agree that the sentence is unclear. We have revised it as follows: "Compared to AR4, satellite records of TOA radiation fluxes have been substantially extended, and indicate no significant trends in tropical and global radiation budgets since 2000 with very high confidence."
2-1025	2	26	41	26	45	How does no trend in the global albedo fit with the trends in optical depth in Fig 2.10? [Government of Australia]	Noted - there is no robust evidence of a global mean trend in AOD after 2000 from MODIS/MISR (See Section 2.2), thus there is also no evidence for a conflict with the no trend in global mean planetary albedo. Further, variations in global mean planetary albedo do not only depend on aerosol optical depth shown in Figure 2.10, but primarily also on the evolution of cloud characteristics, as clouds have a much larger influence than aerosol on global albedo. Therefore, there is not a clear link between Fig. 2.10 and the trend in global albedo. Also, Fig. 2.10 shows regional trends only, while the conclusion of no trends in planetary albedo relates to its global mean evolution (and does not exclude the presence of regional trends in albedo). Therefore the two cannot really be directly compared.
2-1026	2	26	42	26	45	To say this is "high confidence" is absurd. Please see 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. [Kevin Trenberth, United States of America]	Taken into account - We've modified the sentence to: "Compared to AR4, satellite records of TOA radiation fluxes have been substantially extended, and indicate no significant trends in tropical and global radiation budgets since 2000 with high confidence. "
2-1027	2	26	43	26	43	Given comment 8 this needs to be checked (also in the summary). [European Union]	Noted - we do not see what is meant with comment 8.
2-1028	2	26	44	26	44	variability -> interannual variability [Ileana Bladé, Spain]	Accepted - added as suggested.
2-1029	2	26	44	26	44	We struggled to find any material in the chapter supporting this statement that the variability in the Earths energy imbalance is "related to ENSO." In any case, this seems to go beyond the remit of Chapter 2. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, a more elaborate statement on the relations between the Earth's energy imbalance and ENSO has been added in the text in relation to Figure 2.13.
2-1030	2	26	55		56	It is a fact, that there are not many longwave downward radiation and net radiation measurements reported. However, there is still at least “medium confidence” for increasing downward thermal and net radiation at terrestrial stations. [Rolf Philipona, Switzerland]	Accepted - we changed this to medium confidence in line with the statement made in the executive summary.
2-1031	2	27	1	28	3	I think the box (box 2.3) is very necessary in the text to make people have some idea about global atmospheric reanalysis. If the most important characteristics about each Reanalysis have been given in the box, it would have reduced much misuse of the reanalysis [Qingxiang Li, China]	Noted.
2-1032	2	27	1	28	3	There is one other point that could be made in this box. It is that the timescale for carrying out, diagnosing and publishing careful climate studies in the peer-reviewed literature is comparable or longer than the time between IPCC assessments. This means that the assessments, based as they are on the peer-reviewed literature, tend not to reflect the current capabilities of reanalysis. Some specific comments to this effect are given below. [Adrian Simmons, United Kingdom]	Accepted - Sentence added.
2-1033	2	27	11	27	12	I think use of the word "complete" in this sentence may be misleading. I would suggest using the word "uniform" or "rectified" [Jeffrey Taylor, United States of America]	Editorial
2-1034	2	27	20	27	21	I'm not quite sure what is meant by this sentence - how a variety of groups and approaches indicates robustness of reanalyses products? It would be fair to say that the spread of different approaches may provide some measure of structural uncertainty across the products but I don't think it necessarily increases their	Accepted - Sentence changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						robustness in terms of making us more confident that they're doing a good job unless the spread is very small - but then without validation its still possible that they're all wrong in the same way. [Kate Willett, United Kingdom]	
2-1035	2	27	21	27	21	The existence of several reanalysis alone does not provide an indication of robustness but allows testing for robustness of the results. [Ileana Bladé, Spain]	Accepted - Sentence changed.
2-1036	2	27	21			add "when compared" following the last word of sentence. [Government of Australia]	Accepted - Sentence changed.
2-1037	2	27	24	27	27	Box 2.3 Table 2 line 1. The 20C Reanalysis has been updated to 2010 [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted - Date changed.
2-1038	2	27	24	27	27	This table should include the JRA-55 reanalysis (Ebita et al., SOLA, 2011, Vol. 7, 149−152, doi:10.2151/sola.2011-038). This is a latest generation reanalysis that covers the ERA-40 period, and is being executed both with and without satellite data. Results look very promising, even if there is little yet in the literature that can be quoted in AR5. [Adrian Simmons, United Kingdom]	Accepted - JRA55 added.
2-1039	2	27	24	27	27	The two entries that spell out the acronym ECMWF need a hyphen between "Medium" and "Range". "Range" should still be capitalised. This is ungrammatical, but is the legal name of ECMWF. [Adrian Simmons, United Kingdom]	Editorial
2-1040	2	27	29	27	34	The use of "error" needs some clarification in this paragraph. Ultimately, the error one has in mind is the difference between the truth and the reanalysis and how this difference changes through time. This predominantly comes about because the availability of platforms and observations changes, and these changes then flow onto different characteristics in the reanalysis products etc. The extent to which the reanalyses reflect the models climate and the extent to which observations constrain the analyses through time is another critical issue. [Government of Australia]	Rejected - this information is mentioned in the text.
2-1041	2	27	29	27	34	This paragraph has too negative a tone. The better-quality (more-recent) reanalyses have been demonstrated to be able to capture some trends in temperature, humidity and other variables quite well. As time progresses an increasing number of trends can be expected to become reliable. See later discussion in Chapter 2, and other comments below. A perennial open question is what trends from any given reanalysis are reliable and what trends are questionable. The latter can often be easily answered, such as when a reanalysis time series jumps when a new type of observation is included. Establishing what is good requires intercomparision with other sources of information - from observations and from physical reasoning. So I would advocate a phrasing that brings in the above points, rather than simply says "The ability to characterize long-term trends remains an open question". The following paragraph is better balanced in this regard, but still a bit problematic, as discussed below. [Adrian Simmons, United Kingdom]	Noted. - The improvement of later reanalyses is noted in the next paragraph (see reply to Comment 2-1050) as well as at several instances in the Chapter.
2-1042	2	27	30	27	30	. Their ability -> but their ability [Ileana Bladé, Spain]	Editorial
2-1043	2	27	30	27	30	This is too soft a statement about the value of reanalysis trends, which are generally misleading and relied on much too heavily to make unjustifiable statements about climate changes. [Dian Seidel, United States of America]	Accepted - sentence rephrased (now: "However, they may often be unable to characterize long-term trends").
2-1044	2	27	30			It is not "an open question" as it has been shown by Trenberth et al and other papers that reanalyses are mostly not useful at all for trends or decadal variability with the exception of the surface temperature in Simmons et al 2011. [Kevin Trenberth, United States of America]	Accepted - sentence rephrased (now: "However, they may often be unable to characterize long-term trends").
2-1045	2	27	32	27	34	This sentence should point out another challenge: biases in observational data that change in time. Examples are radiosonde temperatures and humidity, and satellite radiances. It should also be pointed out that these problems related to observations are problems that have to be faced by anyone trying to determine climate trends and low-frequency variability, whether or not they choose reanalysis output to help them. [Adrian Simmons, United Kingdom]	Accepted - Sentence changed ("time-dependent errors" rather than just "errors").
2-1046	2	27	36	27	36	I don´t see the need to start a new paragraph as it seems the sentence follows directly from the previous one. [Ileana Bladé, Spain]	Editorial
2-1047	2	27	36	27	37	The sentence is correct, but I would suggest a different reference or references than Thorne and Vose(2010),	Accepted - Reference added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						or at least an additional one. The limitations of early reanalyses have been discussed by many authors. As long ago as 2004 Santer et al. showed lower stratospheric temperature trends were better in the newer generation ERA-40 than in the previous generation NCEP/NCAR reanalysis, and that same year saw papers by Bengtsson et al. and Simmons et al. on this topic. This was covered in AR4. On the other hand, if Thorne and Vose (2010) is to be retained as a reference, it must be noted that this paper has been the subject of extensive comment by Dee et al.(2011) that has, to the best of my knowledge as one of the authors of the comment, not been the subject of independent refutation. Referring to the one paper (the one by one of the lead authors of this Chapter) but not the other does not display the independence and balance that one looks for in an IPCC assessment. Simplest solution would be to add a reference to Dee et al. (2011 - Bull. Amer. Meteor. Soc., 92, 65-70. doi: http://dx.doi.org/10.1175/2010BAMS3070.1). This could come directly after the reference to Thorne and Vose (2010), but would also work (and perhaps work better) at the end of the following sentence, in the middle of line 38. [Adrian Simmons, United Kingdom]	
2-1048	2	27	38			"...but artifacts are still present, in particular for the hydrological cycle." [Richard Allan, United Kingdom]	Rejected - aertifacts also concern other areas.
2-1049	2	27	39	28	1	I think you mean ERA-Interim, not ERA-40, as ERA-Interim was the main focus of the Simmons et al. 2010 paper. Also, this paper validated the land surface humidity component of ERA-Interim in addition to the land surface temperature. [Kate Willett, United Kingdom]	Noted. - Both reanalyses are analysed in that paper, we now highlight ERA-Interim and added humidity.
2-1050	2	27	39	28	3	This final sentence of the box is formally OK, but it does not tell the whole story. There is a beginning and an end that are missing. The beginning is that soon after ERA-40 production was started, both the producers and some early adopters found two significant problems in polar tropospheric temperatures. The problems were in due course documented in the literature (Randel et al., 2004 - there was an earlier SPARC report - and Uppala et al., 2005) and on the ERA-40 website. So the problems discussed in the set of papers published in 2008 quoted in the box could have been avoided had warnings been heeded. The end is simply that these two problems were well understood once they had been discovered, and have been fixed in ERA-Interim. Of course, there may be addditonal problems as yet unidentified. Not sure how to fix the sentence, but I think it should acknowledge that two problems in polar temperature analysis found in ERA-40 (Uppala et al., 2005) were resolved in ERA-Interim (Dee et al., 2011). [Adrian Simmons, United Kingdom]	Accepted - sentence changed.
2-1051	2	27		27		(In Box2.3 Table1) Correct "Japanese Meteorological Agency" to "Japan Meteorological Agency". [Government of Japan]	Editorial
2-1052	2	27		27		(In Box2.3 Table1) Resolution at equator for the JRA-25 reanalysis should be corrected from 190km to 125km (Onogi et al. 2007). [Government of Japan]	Rejected - Resolution is calculated following Laprise et al. (1992). Reference is added (Laprise R. 1992. The Euler equations of motion with hydrostatic pressure as an independent variable. Mon. Weather Rev. 120: 197–207).
2-1053	2	27				Box 2.3, Table 1 doesn't give enough interesting information to warrant inclusion. Are the periods analyzed and the resolution the key aspects that distinguish the reanalyses? Maybe columns with strengths and weaknesses, or some distinctive feature, would help. [Dian Seidel, United States of America]	Rejected - the references, time periods, and resolutions are important as well as the spelling-out of the scronyms.
2-1054	2	28	1	28	1	I suggest to explicitly explain the meaning of "quasi-indipendent observations" [Claudio Cassardo, Italy]	Accepted - "quasi-independent" is omitted.
2-1055	2	28	3	28	3	It would be nice to end this paragraph with a cautionary statement about reanalyses and an articulation about the AR5 policy in their use. [Dian Seidel, United States of America]	Accepted - A sentence is added on the use of reanalyses at the end of the box.
2-1056	2	28	3	28	3	Add recent analysis of Screen, J. A., and I. Simmonds, 2011: Erroneous Arctic temperature trends in the ERA-40 reanalysis: A closer look. Journal of Climate, 24, 2620-2627. [Ian Simmonds, Australia]	Accepted - Reference added.
2-1057	2	28	3	28	3	Issue: For several reanalyses, artifacts of the surface air temperature are found for some regions for certain periods, and a new approach has been developed to remove such artifacts in our recent work. Suggested Change: add a sentence at the end of line 3 of p. 2-28: "Artifacts of the surface air temperature are also found for some regions in certain periods for both early and recent reanalysis products, and they can be largely removed by the diurnal cycle adjustment using in situ monthly averaged maximum and minimum teperature measurements (Wang and Zeng 2012)."	Rejected - space limitations do not allow discussing various methods of how to deal with the artifacts.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Xubin Zeng, United States of America]	
2-1058	2	28	3			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. [Government of United States of America]	Accepted - A sentence is added on the use of reanalyses at the end of the box.
2-1059	2	28	7	28	7	How about changes in soil temperatures? Is this mentioned any where in the AR5. This study of soil temperature trends could be included: Qian, B., E. G. Gregorich, S. Gameda, D. W. Hopkins, X. L. Wang, 2011: Observed soil temperature trends associated with climate change in Canada. JGR-Atmospheres, 116, D02106, doi:10.1029/2010JD015012. [Xiaolan Wang, Canada]	Noted. Soil temperatures were to originally be included but we found no contributing author and insufficient literature at more than local / national scale to warrant their inclusion and the section was dropped prior to FOD for this reason. Existence of Candaian paper is considered insufficient to warrant a section being added given that we are already at our stipulated length budget.
2-1060	2	28	7	29	10	2.4.1 Land-Surface Air Temperature: A subsetion should be added to discuss about the leveling off of the global land warming in the past 15 years. The aft-1998 surface temperature stagnation and the more frequent cold winters in northern continents over the past 15 years than the previous decades have attracted much attention not only from scientific community, but also from publics and governmental officers. The spatial and temporal structure of the change, and its implications for estimates of long-term LSAT trends, should be properly assessed in this section. [Guoyu Ren, China]	Rejected. This is discussed in the section on combined land and sea surface temperatures. It is in the context of this combined record and not the LSAT record that the majority of diuscussion has occurred and so this is where such discussion is most appropriate. A new box (Box 9.2) has also been added to Chapter 9 where this is discussed further and more holistically and appropriate cross-references have been added.
2-1061	2	28	7	29	30	I did not find in the text any mention to the cooling along the western coast of South America derived from observations. See Falvey and Garreaud 2009 JGR, 114D04102, doi: 10.1029/2008JD010519 [Government of Chile]	Noted. A reference to this paper has now been added to Section 2.4.1.1 where regional analyses are discussed. This analysis agrees with the more global analyses and this point is made explicitly in the revision.
2-1062	2	28	7	34	48	There is a lot of attention in the media/bloggosphere for a stabilization of global temperatures from 1998 up to know. E.g. if we look at the patterns shown in Figure 2.24, lower panel, there seems some credit for that vision. Why not addressing the period 1998-2011 in the analysis? E.g., Table 2.5 on page 2-32 shows periods like 1951-2011 and 1979-2011. To my opinion a good answer to this 'stabilization' has been given on the website of the Met Office (14 October 2012): http://metofficenews.wordpress.com/2012/10/14/met-office-in-the-media-14-october-2012/. My advice would be: analyse the data with respect to the period 1998-2011, next to other sample periods, and argue why global temperatures keep on rising. That will convince the reader who closely follows discussions in the media. [Hans Visser, The Netherlands]	Taken into account. See response to 2-1060
2-1063	2	28	7			There is no mention about changes in temperature in South America. In section 2.3.2 of PBMC - chapter 2 (temperature) there are informations about changes in temperature in South America. [Government of Brazil]	Noted. Temperature changes are discussed from the peer reviewed literature. There is insufficient infornation in this comment to ascertain what is being referred to and whether it is reviewed so no changes can be made in response. In response to 2-1061 a paper discussing results for the Pacific coast of S. America has been added.
2-1064	2	28	7			Sec 2.4 Thank you for noting my (and presumably other) comments and providing trends for the first half of the 20thC to provide a fair comparison with the second half (Table 2.4, 2.5, 2.6, 2.7 and figures). Please do not let anyone talk you into removing these! [Paul Matthews, United Kingdom]	Noted.
2-1065	2	28	11	28	11	Liu et al. (PNAS,2012,109:4074-4079) indicated that the cold winters in the recent years may relate to the declining of Arctic sea ice. It may contribute, at least partly, to the pause of the warming in the last decade (Kerr, Science, 2009, 326:28-29). It would be better if this issue is also examined in this Chapter. [Shaowu Wang, Beijing]	Noted. We reference in section 2.4.3 a recent paper highlighting that much odf the recent temperature plateau in a NH winter half year phenomenon.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1066	2	28	11	28	17	Recommend adding the next step: "Distinguishing major from minor anthropogenic warming will need to compare against the null hypothesis of a multidecadal oscillation of about 50-60 years with peaks about 1940 and 2000 superimposed on an approximately linear trend since the Little Ice Age. e.g., the IPCC CMIP5 versus the minor anthropogenic model of Scafetta (2011), and the null hypotheses of Akasofu (2010), and D'Allea and Easterbrook (2011)." [David L. Hagen, United States of America]	Rejected. Out of chapter scope.
2-1067	2	28	11	28	40	Table 2.4 etc. The periods chosen are "convenient" on a century scale, but do little to test the underlying issue of distinguishing between major natural forcing via a linear rising trend with a superimposed multidecadal oscillation of 50-60 years. See Syun-Ichi Akasofu (2010), D'Alleo and Easterbrook (2011), and Scafetta (2011). Picking the steepest part of a sinusoid appears remarkable until compared to the horizontal trends at peak and valley, and the smaller or negative trend at the opposite phase. Girma shows a very strong 60 year sinusoid in the temperature trend. http://wattsupwiththat.com/2012/09/03/empirical-model-of-the-global-mean-surface-temperature/ Thus I strongly recommend picking the maximum, means and minimum of this sinusoid to do trend analysis of about 30 years. i.e. test for sinusoidal crossovers of about 1910, 1940, 1970 and 2000, and peak/minimum trends at about 1925, 1955, 1985 and 2015. e.g. the temperature trend for the last 16 years is effectively flat - statistically indistinguishable from zero. See Lucia Liljegren at the Blackboard 16 November 2012 http://rankexploits.com/musings/2012/trends-relative-to-models/ Also http://rankexploits.com/musings/2012/arima11-test-reject-ar4-multi-model-mean-since-1980-1995-200120012003/ "We would reject the multi-model mean trend as too warm relative to the observed trend if we based our judgement on trends computed starting in 2001, 2002. For these cases, the observed trend falls below the lower ±95% confidence interval for the multi-model mean." [David L. Hagen, United States of America]	Rejected. Mechanisms are out of chapter scope and there exists a box on trends analysis where the nature of the timeseries and the trends are explicitly discussed. Further, comment pertains largely to unpublished blogosphere writings of unknown quality that have not been peer reviewed. References provided include grossly inadequate detail to be found unambiguously using ISI Web of Knowledge search terms.
2-1068	2	28	11	31	20	Somewhere in this section 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 should me mentioned. This paper shows that on land the surface temperature increases much faster than the temperature in the lower troposphere which is contrary to what models expect see their figure 1: http://www.agu.org/journals/jd/jd0921/2009JD011841/ As they write: These findings strongly suggest that there remain important inconsistencies between surface and satellite records. [Marcel Crok, The Netherlands]	Rejected. Out of Chapter scope with respect to inclusion of models. Out of sub-section scope with respect to tropospheric temperatures.
2-1069	2	28	14	28	14	Reference to 50 and 100 years in ambiguous. Be clear about what the start and end date is. [Government of Australia]	Noted. We have edited to exactly replicate what AR4 said here.
2-1070	2	28	14			"…being almost double that in the last 100 years". I would like to add: "i.e., the LSAT net variation in the last 50 and 100 years is the same". [Franco Desiato, Italy]	Taken into account. See response to 2-1069
2-1071	2	28	15	28	16	Please add a sentence like this: "...and an intensive investigation into urban effects on LSAT trends in rapid developing East Asia region". [Guoyu Ren, China]	Rejected. This is a summarizing of this sub-section. The urbanisation and LULC issue has its own sub-section where these issues are dealt with and where such text already exists and will be retained.
2-1072	2	28	17	28	17	understanding -> quantification/assessments ? (to avoid repetition of understanding) [Ileana Bladé, Spain]	Editorial.
2-1073	2	28	19	28	20	"The Global Historical Climatology … Version 3 .." would be clearer. [Christopher Merchant, United Kingdom]	Editorial.
2-1074	2	28	24	28	24	Definition of "CRUTEM4"? [Birgit Hassler, United States of America]	Editorial. CRUTEM4 makes little sense long handed.
2-1075	2	28	24	28	25	There seem to be some explanations missing in this sentence: "… incorporates additional series..." -> of what? "…newly homogenized versions…" -> of what? [Birgit Hassler, United States of America]	Editorial. Attempted clarifications made.
2-1076	2	28	24	28	25	Among the stations that CRUTEM4 uses, 128 stations have long-term averages calculated across periods other than 1961-90 and a further 734 stations that have no long-term average. In many cases the station data has extended periods of non-supply that makes one wonder about the quality of adjustments due to relocations, changes of equipment etc. Because the number of temperature recordings and the global coverage are not consistent over the period of the data the CRUTEM4 data shoul dbe regarded only as an estimate. [John McLean, Australia]	Noted. It should be clear from the section text and box 2.1 (ordering as in SOD) that all the datasets represent estimates. Indeed they are all referred to as estimates later in this same paragraph. No further specific edits requested or made.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1077	2	28	25			Rohde et al. NOT Rhode et al. (mistake duplicated in other places) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-1078	2	28	25			The reference to the group at 'Berkeley' is totally inappropriate and must be fixed. I complained about this coloquial referencing in the previous version. [Larry Thomason, United States of America]	Rejected. Referencing as BEST would be value laden and even more inappropriate as noted in the responses to FOD review comments to this end.
2-1079	2	28	26	28	26	The long-term variations …, despite the range of approaches -> Despite the range of approaches, the long-term variations …. [Ileana Bladé, Spain]	Editorial.
2-1080	2	28	26			It is suggested that the authors explain how the Berkeley method is 'substantially distinct.' [Government of United States of America]	Noted. This is already covered in the appendix. A reference to that material has now been added here in parentheses to make clear where the interested reader can go for such information.
2-1081	2	28	27	28	27	Also here: seems like some explanations are missing: "… among there various estimates…" Just adding "LSAT" before "estimates" would help already. [Birgit Hassler, United States of America]	Noted. LSAT added as suggested, no further changes appear warranted.
2-1082	2	28	28	28	28	Provide a range for the term "early years" -> 1880-1930? [Birgit Hassler, United States of America]	Noted. Sentence has been deleted
2-1083	2	28	28	28	29	Table 2.4 shows that the range (highest minus lowest) between the groups is smaller for 1880-2011 than between the other periods. The lines may appear to differ in the Fig 2.15, but they are not in the trends. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1082
2-1084	2	28	30	28	31	A conclusion that temperatures have risen is hardly surprising given that the Little Ice Age ended around 1850, but the question is whether the rise reported by CRUTEM3 is accurate. [John McLean, Australia]	Rejected. Assessment does not use CRUTEM3 so comment is moot.
2-1085	2	28	34			Fig 2:15: what are the units? [Kevin Trenberth, United States of America]	Noted. Degrees C as labelled on the figure's y-axis. Adding this information to the caption appears to be a decadence when the space is at such a premium.
2-1086	2	28	38	28	38	The unit of the trends is missing in the table legend [Ileana Bladé, Spain]	Accepted. Added to this caption the units as degrees C / decade.
2-1087	2	28	38	28	38	The differences between the trend results in table 2.4 and 2.5/2.6 are remarkable. Table 2.5 shows that the SST trend in the first half of the 20th century was the same as in the period 1951-2011. This is contrary to popular belief that current warming is taking place at an unprecedented rate. The only thing that seems to accelerate is the LSAT after 1950 and this exactly the metric with all the unresolved issues. The growing discrepancy between LSAT and SST and also between LSAT and LTL (see Klotzbach 2009) provides further evidence that there probably is a warm bias in the LSAT record due to siting issues, UHI, land use change, waste heat, unstable nocturnal boundary layer. This raises the fundamental question whether the LSAT should be used as a metric for global warming/cooling. There is much more attention in AR5 for ocean heat content which is great, but in chapter 10 the global average surface T still plays an important role in detection and attribution and the question is whether this prominent role for global T is warranted. [Marcel Crok, The Netherlands]	Noted. Comment makes no specific requested changes to this table and as such none are made. Much of the comment pertains to subject matters out of chapter scope and the remainder is largely editorializing that we should cover issues which are already covered in the subsequent text and sub-sections in Section 2.4.
2-1088	2	28	38	28	38	Please specify the units of the trend estimates. [John Ogren, United States of America]	Taken into account. See response to 2-1086
2-1089	2	28	38	28	39	Table 2.4: What is the unit of the trends? °C per decade? [Alice Grimm, Brazil]	Taken into account. See response to 2-1086
2-1090	2	28	38	28	39	What are the units of the trend estimates? K/decade? [Peter Guttorp, United States of America]	Taken into account. See response to 2-1086
2-1091	2	28	38	28	39	Table 2.4 Add the temperature and time units. This appears to be deg C/decade but that is nowhere stated. [David L. Hagen, United States of America]	Taken into account. See response to 2-1086
2-1092	2	28	38	28	39	Caption of Table 2.4: Add units of described trends. [Birgit Hassler, United States of America]	Taken into account. See response to 2-1086
2-1093	2	28	38	28	39	Chapter 1 said that period 1998 to 2011 would be shown, so please include it in this table. [John McLean, Australia]	Rejected. The trend is calculated for the combined LSAT and SST datasets in a section 2.4.3 where it is deemed most appropriate. It is also now the subject of

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							a box (9.2) in Chapter 9 to which Chapter 2 has contributed and which is cross-referenced in the text in Section 2.4.3
2-1094	2	28	38	28	39	Units? Presumably degrees/decade. [James Renwick, New Zealand]	Taken into account. See response to 2-1086
2-1095	2	28	38	28	39	Table 2.4: Unit missing.Insert "K" after "Trend estimates" (line 38). [Christian-D. Schoenwiese, Germany]	Taken into account. See response to 2-1086
2-1096	2	28	38	28	40	The information in this table (and subsequent equivalent tables) is very instructive. However, I think there could be even more use made of such tables (possibly in supplementary material, but also referenced in the SPM), where these show differences in global and regional (SREX region) temperatures (possibly other variables too) during many other reference periods. AR4 used 1980-1999, impact studies variously use 1961-1990, 1971-2000 and even 1981-2010, as well as 20-year periods aligned with the AR4 or AR5 (1986-2005). There is also the question of what temperature to use to represent the pre-industrial reference that is used for evaluating global targets such as 2 degC. So overall, it would be very helpful to see the temperature offsets globally and for SREX regions (consistent with the Atlas) among these various reference periods (along with uncertainty ranges). This will help analysts in other fields to interpret the reference observed (and modelled) climates that have been adopted in the literature for diverse periods across a range of studies and circumstances, in light of the most up-to-date assessment of these observations presented in this chapter. Some reference to these "offsets" might be needed in the SPM to reconcile different baselines used in previous assessments as well as helping to provide a working definition of pre-industrial temperature, which currently is not attempted. "Early industrial" is used, and may be the closest attempt that can be offered for instrumental temperature, but this needs to be explained somewhere. Pre-industrial is equated with 1750 elsewhere in the chapter for GHG concentrations, where these can be inferred from proxy information. [Timothy Carter, Finland]	Rejected. Inclusion of such analyses, although appealing, would make the chapter much longer than the permitted length. Such analyses are relatively simple for analysts to perform and more appropriate in the literature.
2-1097	2	28	38	28	40	Table 2.4. No units are given for the trends. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1086
2-1098	2	28	38			Specify units (°C/decade?) in Table 2.4 caption [Franco Desiato, Italy]	Taken into account. See response to 2-1086
2-1099	2	28	38			tbl. 2.4 Are units K per decade? [Government of United States of America]	Taken into account. See response to 2-1086
2-1100	2	28	38			Table 2.4 (and others). Say what the units are!!! [Paul Matthews, United Kingdom]	Taken into account. See response to 2-1086
2-1101	2	28	39	28	39	The start of this chapter says that trend from 1998 to 2011 will be discussed but the most recent trends in this table is 1979-2011. Please add the 1998-2011 trend. [John McLean, Australia]	Taken into account. See response to 2-1093
2-1102	2	28	42	28	42	I think veracity, not verity, is meant if the issue is the records. If the issue is the very concept of a global LSAT then verity is OK. Either way what those "theoretical" challenges may be is left unexplained. The next sentences refer to methodological and data issues, not shedding any light on what those theoretical challenges may be and leading one to wonder if the question is simply being avoided. [Ileana Bladé, Spain]	Rejected. Verity is the intended wording here. Definition of VERITY 1 : the quality or state of being true or real 2 : something (as a statement) that is true; especially : a fundamental and inevitably true value <such eternal verities as honor, love, and patriotism> 3 : the quality or state of being truthful or honest <the king-becoming graces, as justice, verity — Shakespeare>
2-1103	2	28	42	28	42	Don't use the word 'theoretical' here, this is not doing justice to the ongoing debate. The challenges are not a matter of theory although there are theoretical aspects as well. Pielke 2007 notices that in terms of global warming/cooling it's not the global average that matters but σT^4. This is important because using σT^4 instead of just global T will weaken the importance of polar amplification. There are several observational issues as well so just saying "various challenges" is just right. [Marcel Crok, The Netherlands]	Rejected. After re-reading the Pielke et al. reference the basis for the criticisms therein was found to be almost entirely theoretically based so this characterization was felt appropriate.
2-1104	2	28	42	28	45	This section doesn't do justice to the ongoing debate in the field. Many of the unresolved issues in Pielke 2007	Rejected. The basis for the characterization is the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						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]	suite of papers that are discussed in the remainder of the paragraph and not the Parker et al response alone. The reply to Parker et al. does not constitute a final word on the issue beyond that permitted within the confines of the JGR comment and reply format. The reviewer is incorrect to suggest otherwise here.
2-1105	2	28	42	28	52	I am not sure this paragraph is the best way to refute the "theoretical arguments" and convince a skeptical person that the we can have confidence in the LSAT records. The references to the various approaches intended to reduce data and methodological uncertainties is good, but it is followed by a physical argument based on consistency with humidity changes that is so succintly made that it will seem obscure to anyone not already familiar it. Then we revert back to a data argument: consistency between observed and reanalysis data. Why is this even a good argument ? Don´t the reanalysis assimilate SST data? [Ileana Bladé, Spain]	Noted. The reviewer makes no specific suggestions for changes. The reanalyses do indeed assimilate the SST data as a lower boundary condition (except for CFSR) but the point is that we are discussing LSAT which is not assimilated.
2-1106	2	28	42	28	52	The temperature records are likely flawed because the global average CRUTEM3 diverged from HadSST2 starting in 1980 after 130 years of similar values. Hadley Centre assumed the divergence was due to flaws in the monitoring of SSTs but another important factor is that the CRUTEM3 data did not agree with the corresponding data from national meteorological services (a difference of more than 10 degrees for some Russian stations at times) and the data for many observation stations ceased in the CRUTEM3 dataset in the late 1980s although was available from NMSs). Did the IPCC seek an independent audit of HadCRUT3 (of HadCRUT4) data prior to citing it? If not, why not? Surely any review of the associated paper would have been cursory unless the reviewer was able to obtain the data and replicate all the calculations. [John McLean, Australia]	Rejected. The assessment is charged with undertaking a holistic literature review. Not undertaking audits of each and every dataset which would be an impossible task with solely voluntary effort on the part of the (C)LAs. The assessment does not rest on the CRUTEM dataset - it is supported by numerous national, regional, and global analyses produced independently, by trends in other variables and by the global reanalyses products as made clear here and across the remaining sections of this Chapter and other Chapters of the report. The assertion that the divergence since 1980 is down to SST biases is stated as an unsupported strawman argument. To our knowledge such a statement has never been made by the UK Met Office.
2-1107	2	28	42	28	52	General agreement between the datasets is no surprise because they are based on the same data. Please show a graph of average global coverage for each year and the average number of stations supplying data in each year (according to each station record rather than the CRU's statement of the period of supoply) so that the reader is made aware of the limitations of the data. [John McLean, Australia]	Noted. This graphic and associated discussion is present in the appendix already. Its presence is now highlighted in the text.
2-1108	2	28	42	29	15	The claim of unadjusted data is incorrect. In my work (Christy 2006, 2009 and others), a significant homogenization process was utilized, and I would argue that it was more effective than the latest NCDC method which smears gradual (and non-climate) warming throughout the homogenization process. Indeed, this section comes across as if NCDC has the final say on what is correct rather than allowing for NCDC to be just one attempt among many efforts. Now, NCDC researchers have performed some notable research, but it should not be considered the final word to overshadow other results - especially those backed by highlly consistent theory and modeling work (McNider et al.). [John Christy, United States of America]	Noted. Nowhere in this passage is any claim made about unadjusted data beyond the fact that it likely contains inhomogeneities which is not contentious and is consistent with the reviewer's stated concerns. So, it is difficult to therefore understand what changes the reviewer is requesting. The whole passage of text is about our understanding of data issues and their adjustments. All papers referenced in the reviewer's comments are cited appropriately within the section as a whole, although only one of these is cited in this

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							restricted passage. Its citation is felt to be appropriate and in keeping with the reviewer's comment.
2-1109	2	28	42	29	15	I'd like to see this discussion of how recent analyses overcome the type of issues that might be seized upon by climate sceptics sharpened somewhat, as it is important to emphasise the robustness and scientific credibility of the work supporting rises in LSAT. A common sceptic tactic is to pick on a limited aspect of individual studies; it is the overall robustness that needs to be emphasised. [Government of Australia]	Noted. This is what this text is trying to do. Within the space available it is hard to see how to improve this text further or make this message clearer.
2-1110	2	28	44	28	45	This sentence could be beneficially moved to line 17. [Dian Seidel, United States of America]	Accepted
2-1111	2	28	44	28	46	The data is the data is the data. Critics don't question the numbers and agree there is about 0.8 degrees of warming. The issue is what is causing this warming and which part of this 0.8 is really the cause of large scale climatological changes and which is caused by more local/regional processes. [Marcel Crok, The Netherlands]	Noted. No changes requested or made. Comment also appears to have no context in the cited page and line numbers.
2-1112	2	28	47			Rohde et al. NOT Rhode et al. (mistake duplicated in other places) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial. There are many issues with endnote referencing that require fixing.
2-1113	2	28	48	28	50	The reference to Willett et al 2010 is not relevant here. In fact I don't see the logic of this sentence and it could be left out - changes in atmospheric humidity are dealt with else where. [European Union]	Rejected. The point is that the physical expectation, where water is not severly limited, is for RH to remain approximately constant so increasing specific humidity and constant RH is physically consistent with a warming of LSAT. We have nuanced the text a little to make this clearer.
2-1114	2	28	49	28	49	Peterson (2011) is a reaction to Pielke Sr., R.A., C. Davey, and J. Morgan, 2004: Assessing “global warming” with surface heat content. Eos, 85, No. 21, 210-21 and Davey, C.A., R.A. Pielke Sr., and K.P. Gallo, 2006: Differences between near-surface equivalent temperature and temperature trends for the eastern United States – Equivalent temperature as an alternative measure of heat content. Global and Planetary Change, 54, 19–32. The issue here is whether the surface air temperature is the right metric for monitoring warming or cooling. Pielke, Davey et al state that it would be better to use moist enthalpy: "Surface air temperature alone does not capture the real changes in surface air heat content of the Earth system. Even using the limited definition of the term “global warming,”the moisture content of the surface air must be included. Future assessments should include trends and variability of surface heat content in addition to temperature." The study of Peterson shows that surface heat content and temperature can have opposite trends: "Figure 1 shows that heat content tends to be decreasing in Australia despite increases in surface temperature. Presenting heat content as the primary metric for global warming could lead lay readers to erroneously perceive Australia as cooling – after all, its heat (content) is decreasing. Our concern is not just nomenclature. Heat content by any other name if used as a global warming metric has the potential to imply cooling even in places with increasing temperature simply because the location is becoming dryer." Well, here not the lay reader but Peterson himself seems to misunderstand his result. If in Australia the surface heat content is decreasing, this actually means the surface is cooling. [Marcel Crok, The Netherlands]	Rejected. Peterson et al constitutes a stand-alone analysis and is cited appropriately here. At the global level the increase in specific humidity and temperature are mutually supportive.
2-1115	2	28	51	28	52	I recommend adding two references here. I would add Simmons et al. (2004 - J. Geophys. Res., 109, D24115, doi:10.1029/2004JD005306) as this discusses temperature much more thoroughly than Simmons et al.(2010), which had focus more on humidity, and as it also sets the scene for the second reference I would add, Onogi et al. (2007), which showed for JRA-25 what Simmons et al. (2004) did for ERA-40, that these respective reanalyses do quite a good job on near-surface temperature variability and trends (and improve on the earlier generation NCEP/NCAR reanalysis in this regard). [Adrian Simmons, United Kingdom]	Noted. We will cite Onogi et al. in revised line 52. Simmons et al was addressed in AR4 and we have been requested to minimize discussion of papers which precede the current assessment window.
2-1116	2	28	52	28	52	"in very good agreement with observed products", in what sense? [Xuebin Zhang, Canada]	Noted. Changed the subjective 'very good' to quantitative.
2-1117	2	28				Table 2,4 is confusing because (1) it is not clear whether the numbers refer to decadal trends within the time period or something else and (2) the values appear incoonsistent [Government of France]	Taken into account. See response to 2-1086 for (1). It is not clear what the reviewers mean by their point (2) here. The data values are what the datasets imply the trend to be for the different epochs and the datasets are distinct and reflect some estimate of structural

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							uncertainty (Box 2.1).
2-1118	2	28				Table 2.4 2.5 2.6 2.7 2.8 Units (°C /y) are missing. [Government of France]	Taken into account. See response to 2-1086
2-1119	2	28				Table 2.4: what do the numbers mean in the table? Provide units e.g. [K/decade] [Alexander Loew, Germany]	Taken into account. See response to 2-1086
2-1120	2	29	1			The first line, "Particular controversy since AR4 has surrounded the LSAT record over the United States" is unsupported in the following text - the text immediately jumps to the new results for AR5. A line or two indicating the nature of the controversy would be helpful to readers. [Government of United States of America]	Accepted. Words to this effect have been added here.
2-1121	2	29	2	29	2	Again, could you please spell out what the controversy is about ? [Ileana Bladé, Spain]	Taken into account. See response to 2-1120
2-1122	2	29	2	29	15	This paragraph is correct but would flow better if the sentence "A new automated…" were moved to the end of line 9. In doing this, put a period at the end of line 9 and a comma at the end of the moved sentence, leading to "and this homogenization likely removes..." Flow would also be better if line 8 begins "But this modern siting quality..." [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-1123	2	29	5	28	5	The term "poor modern siting" is not really explained. [Birgit Hassler, United States of America]	Accepted. Some edits have been made referring to assessment against official WMO guidance on station siting here.
2-1124	2	29	5	29	5	Here too I feel the text could be more explicit about how this change to sensors lead to pitfalls in measuring maximum/minimum temperatures. In fact minimum and maximum temperatures are introduced abruptly without explicit referral to their relationship with daily temperature; this relationship should be made clear. I thought the WMO standard for daily temperature was the average of the four 6-hourly synoptic times measurement not the average of minimum and maximum temperatures. [Ileana Bladé, Spain]	Accepted. Some edits have been made here to clarify.
2-1125	2	29	5	29	5	'Most sites' is possibly an exaggeration - 'many sites' may be a fairer description of the situation. [Government of Australia]	Rejected. By the objective Leroy criteria well in excess of 50% of the US COOP network sites comprising USHCN do not meet stipulated guidance for well sited stations.
2-1126	2	29	5	29	7	It is not just the replacementy of the screens, but also the replacement of the sensors from LiG to PRT. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Edits should help make this clearer.
2-1127	2	29	6	29	6	replace 'sensor' with 'System (MMTS)' [European Union]	Accepted.
2-1128	2	29	8	29	9	This sentence needs more explanation or should be reworded. The word correlation should be avoided here, and what is siting quality? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. The sentence has been rewritten.
2-1129	2	29	10	29	10	and homogenization removes: would "so that homogenization removes" be accurate and thus more clear ? [Ileana Bladé, Spain]	Noted. Edits elsewhere make this concern moot.
2-1130	2	29	11	29	11	and ensemble of realizations: it is unclear to me what this means [Ileana Bladé, Spain]	Accepted. Clarifying edits have been made here.
2-1131	2	29	11	29	12	As the readers of the report will not read all the cited literature, it is advisable that the results are as clearly described as possible, even if very summarized. I did not understand this description in the context of this paragraph: 'Williams et al. (2012) produced an ensemble of realisations and concluded through assessment against plausible test cases that there existed a propensity to under-estimate adjustments.' Realisations of what? Cases testing what? Adjustments of what? Perhaps a better way to write it is: 'Williams et al. (2012) concluded that there existed a propensity to under-estimate...' [Alice Grimm, Brazil]	Taken into account. See response to 2-1130
2-1132	2	29	12	29	12	Suggest qualifying "a propensity to under-estimate adjustments" with "in certain circumstances" to reflect the fact that it is most problematic [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence has been added to clarify.
2-1133	2	29	13	29	14	Does "minimum and maximum centennial-timescale US LSAT trends" refer to trends in daily max and min temperature, or the max and min trends in temperature from different US locations? [Dian Seidel, United States of America]	Accepted. Clarifying edits added.
2-1134	2	29	15			Change "reanalyses" to "reanalysis". [Adrian Simmons, United Kingdom]	Editorial.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1135	2	29	17	29	17	analysis -> analysis of LSAT [Ileana Bladé, Spain]	Editorial.
2-1136	2	29	18			Are there no recent studies in the Mediterranean area worth to be mentioned? [Franco Desiato, Italy]	Noted. We have tried to be inclusive but its not possible to include, be aware of, or read every paper across the myriad scientific journals that now exist. If the reviewer had suggested papers that were pertinent we would have reviewed them for inclusion, but with no leads we cannot action this comment.
2-1137	2	29	19	29	19	The reference QingXiang et al., 2010 should be Li et al., 2010: the same "Li" as in Li et al. (2009)! The reference is Li, Q., Dong, W., Li, W., Gao, X., Jones, P. D., Kennedy, J. J. and Parker, D. E., 2010: Assessment of the uncertainties in temperature change in China during the last century. Chinese Science Bulletin, 55, 1974-1982. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1138	2	29	19	29	20	An overview of recent studies on climate changes was published, which assessed the annual mean LSAT trends of the last 110 years for mainland China, indicating a country-averaged warming ranging from 0.03ºC/10yr to 0.12ºC/10yr (Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977) [Guoyu Ren, China]	Noted. This paper makes similar conclusions to the four already cited for the Chinese region. In the interests of sapce no additions / changes are therefore made.
2-1139	2	29	19	29	44	These two paragraphs cite three papers that have been submitted but not published. Please remove the cited passages because this is the SOD review and access to those papers is, at this time, very limited. [John McLean, Australia]	Rejected. These papers were lodged with TSU as per guidance. They will be included if they are published by the stipulated date or removed as per guidance. We have followed and continue to follow official guidance with respect to what can and cannot be quoted and cited in the drafts.
2-1140	2	29	20	29	20	The Trewin 2012 paper describes the data set only and contains no results on temperature trends. Trewin B and Smalley R 2012 (Changes in extreme temperatures in Australia, 1910 to 2011, Aust. Met. Oceanogr. J., submitted) notes in passing information on mean temperature trends which is consistent with what is reported in the IPCC text. A much more comprehensive reference, but one which is not (yet) in a peer-reviewed journal, is Fawcett RJB, Trewin BC, Braganza K, Smalley RJ, Jovanovic B and Jones DA, 2012, On the sensitivity of Australian temperature trends and variability to analysis methods and observation networks, CAWCR Technical report 50, available at http://cawcr.gov.au/publications/technicalreports/CTR_050.pdf. This is a technical report of the Australian Bureau of Meteorology and has been internally, but not externally, reviewed. [Government of Australia]	Noted. Guidance is that we should review the peer reviewed literature only so no changes made. The paper, as far as we are aware remained unaccepted at the time of cut-off
2-1141	2	29	20	29	21	update "(Vincent et al., Submitted)" to "Vincent et al., 2012", because this has been published. [Xiaolan Wang, Canada]	Accepted
2-1142	2	29	24	29	24	summertime -> summer time [Mihai Dima, Romania]	Editorial.
2-1143	2	29	24	29	24	Some context for the word 'large' with respect to summertime biases is needed. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. The fact that these were in some cases greater than 1C has been noted in parentheses.
2-1144	2	29	26	29	26	Add reference: Winkler, P., 2009: Revision and necessary correction of the long-term temperature series of Hohenpeissenberg 1781-2006. Theor. Appl. Climatol., 98, 259-268. This author has found a warm bias as large as 0.63 K for the period 1781-1841 due to an instrument error. May be that this error has influenced the whole station network at this time, not only in Germany. [Christian-D. Schoenwiese, Germany]	Accepted
2-1145	2	29	26	29	30	The statement that there is "low confidence" in Antarctic region LSAT temperature changes needs to be updated in the light of new work demonstrating that West Antarctic temperature trends are *larger* than shown in Steig et al., 2009, and that the results of O'Donnell et al. are incompatible with independent borehole temperature evidence. ALL evidence shows *significant* warming over West Antartica, and warming overall for the continent as a whole (but not sigificant) in the last 50 years. Warming overall is at least "medium confidence" and warming over West Antarctica is "virtually certain". 1 Bromwich, D. H. et al. Central West Antarctica among most rapidly warming regions on Earth. Nat. Geosci. in press (2012). THIS PAPER WAS SUBMITTED BEFORE THE IPCC DEADLINE AND IS IN PRESS.	Rejected. The Bromwich et al. paper is for a single site which will by definition have limited power to discriminate the verity of various spatial infilling techniques beyond a relatively restricted sphere of influence (compared to the very substantive size of Antarctica) so does not change the current bottom line conclusion that confidence in pan-Antarctic trends is low given the paucity of direct observations, the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2 Orsi, A. J., Cornuelle, B. D. & Severinghaus, J. P. Little Ice Age cold interval in West Antarctica: Evidence from borehole temperature at the West Antarctic Ice Sheet (WAIS) Divide. Geophys. Res. Lett. 39, L09710, doi:10.1029/2012gl051260 (2012). 3 Schneider, D. P., Deser, C. & Okumura, Y. An assessment and interpretation of the observed warming of West Antarctica in the austral spring. Clim. Dyn. 38, 323-347, doi:10.1007/s00382-010-0985-x (2011). 4 O'Donnell, R., Lewis, N., McIntyre, S. & Condon, J. Improved methods for PCA-based reconstructions: case study using the Steig et al. (2009) Antarctic temperature reconstruction. J. Climate 24, 2099-2115, doi:10.1175/2010jcli3656.1 (2010). 5 Steig, E. J. et al. Warming of the Antarctic ice-sheet surface since the 1957 International Geophysical Year. Nature 457, 459-462, doi:doi:10.1038/nature07669 (2009). [Eric Steig, United States of America]	challenging measurement environment, substantial multi-annual to multi-decadal variability and the necessity for interpolation over very broad regions with substantive topography. Bore hole information is the domain of Chapter 4 and we wish to minimize chapter cross-talk potential so do not include such discussion here. In re-reading the literature and reading the Bromwich et al reference two additional citations have been added. But after due consideration of the sum totality of literature on the subject our conclusion in regards to pan-Antarctic estimates remains unchanged.
2-1146	2	29	27	29	30	A possible interpretation of this would be helpful. If I remember correctly, early projections indicated more rapid polar warming than at lower latitudes. Ditto contrasting trends between northern and southern hemisphere ice. [Government of Australia]	Rejected. Interpretation of the observations in this manner strays into the remit of other chapters.
2-1147	2	29	28	29	28	Antarctic warming is not statistically significant. It is over the Peninsula. Neither of these papers look at trends over the whole continent. It would be quite easy for you to calculate the trend for the continent and compare with ERA-Interim from 1979. There would be good agreement. There is high confidence in the fact that little warming is occurring. This would be much better written this way round. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. See alternative view in 2-1145. It is felt that the current text reflects the balance of evidence in the literature at this time.
2-1148	2	29	28			"(O'Donnell et al., 2011; Steig et al., 2009). Although these agree that Antarctica as a whole is warming..." Not an accurate description of O'Donnell et al., which finds almost all warming in the geographically separate peninsula and no significant warming over most of the continent. [Stephen Gaalema, United States of America]	Rejected. Text is not implying anything about the significance of the trends so review comment is moot.
2-1149	2	29	30	29	30	I think some kind of conclusion statement / take-home point is needed at the end of this section (all previous sections included one and this section more than any other needs a "refocusing" sentence at the end). In fact it seems peculiar that the whole section is devoted to discussing data issues and caveats but not to describe Figure 2.15 at all. Even if there are no big changes since AR4, this figure is still a centerpiece of the report (it is duplicated in the SPM) and should be commented on. I realize the discussion has been saved for the combined land/SST temperature but it still feels peculiar to "skip over" the figure. [Ileana Bladé, Spain]	Noted. Changes to the way we are representing the conclusions means that this now does have a summary statement as per remaining sections. The reviewer is correct that discussion of the timeseries figure is light but given space restrictions that we are operating under it is not really possible to devote extra discussion to this figure in this section.
2-1150	2	29	30	29	31	Issue: The difference in daily and monthly mean using 24 hourly values versus using maximum and minimum temperatures was not recognized in previous IPCC Reports. It is time to mention the difference now. It is fortunate that the trend is minimally affected. Sugested Change: add a short paragraph on line 31 of p. 2-29: "The monthly and annual mean LSAT used in Table 2.4 and Figure 2.15 was computed as the average of daily maximum and minimum temperatures. This is different from the true monthly (or annual) mean temperatures, which is defined as the integral of the continuous temperature measurements in a month (or a year) divided by the integration period and can be very accurately represented using hourly data, as has long been recognized (e.g., Zeng and Wang 2012). These differences, however, have a small impact on the computation of LSAT trend (Wang and Zeng 2012)." [Xubin Zeng, United States of America]	Rejected. The reviewer is over-simplifying the issue here. Outside Anglo-Saxon regions it has not necessarilly been common to create average temperatures from maximum and minimum observations. Even within such regions the time of observation and method likely matter as much, if not more. Metadata is also grossly incomplete such that the method by which the monthly means have been calculated is not known for almost all stations outside a hadful of countries or through time.
2-1151	2	29	32	29	32	One picture says more than a thousand words. Show figure 1 of McNider (2012), see http://www.agu.org/journals/jd/jd1214/2012JD017578/figures.shtml We see in the adjusted data still a huge difference in the Tmin and Tmax trends. Trends in Tmin are twice as large as trends in Tmax. The paper itself makes reasonable that this much larger trend in Tmin has to do with a growing disturbance over time of the nocturnal boundary layer. The rising Tmin are the result of a redistribution of heat in the boundary layer not of an increase of total heat in the boundary layer. This paper makes it clear that Tmin is not a good metric for measuring global warming/cooling. This is further evidence that trends in LSAT are as yet not reliable as a metric for global warming/cooling. AR5 should assess this paper and assess it's consequence for the usability	Noted. This paper has been assessed and is cited appropriately in this section. As no specific changes additional to figure inclusion, which we would note we already had too many Figures per guidance, were requested none are made.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of Tmin as a global warming metric. [Marcel Crok, The Netherlands]	
2-1152	2	29	32	29	32	In section 2.3.3.1 it is claimed that trends in DTR are related to trends in SSR and reasonably- well-understood as a consequence of direct and indirect aerosol effects. Why is this connection not mentioned here? [Geert Jan van Oldenborgh, Netherlands]	Noted. We have made efforts to improve consistency between sections with respect to DTR discussion.
2-1153	2	29	34	29	34	to have narrowed since when ? [Ileana Bladé, Spain]	Accepted.
2-1154	2	29	34	29	37	I suggest you read AR4 and see what they said. They sadi there was a decrease in DTR, but that it stopped around 1980. You're not referring to any extensive paper since Vose et al (2005a). The point is that DTR stopped decreasing around 1980 and AR4 said that. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Have made clear that since 1980 AR4 said it had stopped decreasing. Have also highlighted the uncertainty statement with respect to DTR in the TS of AR4
2-1155	2	29	34	29	45	Please state that DTR is closely correlated to cloud cover and that increasing DTR is consistent with a reduction in cloud cover. [John McLean, Australia]	Rejected. Out of section scope and the hydrological section concludes that cloud changes are somewhat uncertain.
2-1156	2	29	35	29	35	Insert comma after 'However' [Peter Burt, United Kingdom]	Editorial.
2-1157	2	29	39	29	39	change "(Vose et al., 2005a)" to the Vose et al. (2005a) analysis" [Government of Australia]	Editorial.
2-1158	2	29	39	29	39	Replace '…subsequent to (Vose et al., 2005a), reported…' with '…subsequent to Vose et al. (2005a), reported…'. [Alice Grimm, Brazil]	Editorial.
2-1159	2	29	39	29	45	Annual mean DTR in mainland China as a whole experienced a significant decline at a rate of −0.15 ºC/10yr over the time period 1961-2008. The decline is more evident prior to the mid-1980s (Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053) [Guoyu Ren, China]	Accepted.
2-1160	2	29	41	29	41	Rohde et al. NOT Rhode et al. (mistake duplicated in other places) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. See response in other places.
2-1161	2	29	42	29	42	changed -> also changed [Ileana Bladé, Spain]	Editorial
2-1162	2	29	43	29	43	Roy -> However, Roy [Ileana Bladé, Spain]	Editorial
2-1163	2	29	43	29	45	Observation by authors is valid in the sense that the narrowing of DTR has indicated no pause. There is a need however to have more station data over the region to conduct comprehensive analys considerin the sparse station network in the region where there is a diverse orography [Government of United Republic of Tanzania]	Noted. IPCC needs to retain policy neutrality so cannot advocate for improved observations capabilities in this manner.
2-1164	2	29	44	29	44	Christy -> Likewise, Christy [Ileana Bladé, Spain]	Editorial
2-1165	2	29	45			Reductions in DTR has also been linked to reduced surface solar radiation related to aerosol burden (Wild et al. 2007, cf Section 2.3.3.1). [The mention of this in Section 2.3.3.1 could be moved here rather than duplicate] [Richard Allan, United Kingdom]	Noted. This section is about observed trends and not their potential causes
2-1166	2	29	47	29	47	Fawcett et al 2012 found that there was an observed decreasing trend in Australian diurnal temperature range 1910-2010 but that the trend disappeared when the effect of (increasing) mean rainfall was removed, implying that the DTR trend was attributable to rainfall increases. [Government of Australia]	Noted. Assuming Fawcett et al relates to the technical note alluded to in an earlier comment as it is not peer reviewed it cannot be included.
2-1167	2	29	47	29	47	Quoted reference: remove the parentheses around years, i.e. (e.g., Christy et al., 2009; Pielke and Matui, 2005) [Hai Lin, Canada]	Editorial
2-1168	2	29	47	29	47	Please add a paper by Zhou and Ren (2011) (Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053), which attributed the DTR decline in North China completely to the urban effect. [Guoyu Ren, China]	Accepted
2-1169	2	29	47	30	9	Our work specifically dealt with the homogenization issue and the differential DTR results were confirmed.	Noted. The reviewer's work is already heavily cited in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Indeed, the highly detailed work of McNider et al. demonstrated the theoretical underpinning of the observational record (i.e. declining DTR.) While the IPCC authors themselves may not realize it, in my opinion these sections are written in a style to confirm one type of result and diminish work of non-IPCC-authors (a systemic problem noted by the IAC). This bias is something that can be exploited to IPCC's detriment in an objective analysis of the document. [John Christy, United States of America]	this section of text. It is not clear what changes the reviewer is requesting here, nor that any are warranted. This section places the reviewer's work in the context of that by others to answer the specific charge of what trends are apparent in global and regional DTR. The chapter is not charged with assessing the theoretical / modelling aspects of the McNider study in depth. This would be more appropriate in e.g. Chapter 9.
2-1170	2	29	47	30	9	Many of the references here are old and were dealt with in AR4; moreover those by Christy are biased. This needs a new look and reconciling with the extremes section on max and min temperatures. I believe there are other papers by Petersen and others on this topic. [Kevin Trenberth, United States of America]	Rejected. All the references bar 2 post-date the acceptance cut-off of AR4 to our knowledge. Those two are required for context of the papers that follow them. We have checked with Dr. Peterson and he has confirmed that beyond one regional assessment which mentioned in one paragraph DTR he has not published on this topic since AR4.
2-1171	2	29	49	29	50	suggest adding "at night" for clarity [Melissa Free, United States of America]	Editorial.
2-1172	2	29	50	29	52	Shorten and improve flow by changing to "Parker (2006) further investigated this issue arguing that if data were affected in this way, then a trend difference would be expected between calm and windy nights. However, he found no such minimum temperature differences on a global average basis". [Ileana Bladé, Spain]	Editorial.
2-1173	2	29	55	29	55	the missing [Andreas Walter, Germany]	Editorial.
2-1174	2	30	1	30	9	I found this paragraph is very strange in that it essentially invalidates the DTR results discussed in previous IPCC reports and many previous papers. The Vose et al. (2005) and other analyses from the NCDC have used homogenized data in quantifying the DTR trends, although there may be revisions to the homogenization schemes (e.g., for U.S. stations). Previous studies (e.g., Dai et al. 1999, J.Clim) have linked the DTR trends to physically related but indepedently measured variables such as cloudiness (which should be mentioned here too). The questioning presented here is just not scientifically founded, or too vague. [Aiguo Dai, United States of America]	Noted. The issues are documented in some detail in the cited papers which include e.g. side-by-side field campaign comparisons highlighting these issues. This has been bought out in edits to this section.
2-1175	2	30	4	30	4	You may hope to cite Zhou and Ren (2011) (Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053). The paper analyzed the changes in extreme temperature events in mainland China applying an in-homogeneity adjuested dataset. [Guoyu Ren, China]	Accepted
2-1176	2	30	11	30	44	You should mention Fall, S., D. Niyogi, A. Gluhovsky, R. A. Pielke Sr., E. Kalnay, and G. Rochon, 2009: Impacts of land use land cover on temperature trends over the continental United States: Assessment using the North American Regional Reanalysis. Int. J. Climatol., 10.1002/joc.1996. This paper shows that most of the trends in the US can be explained by land use changes. In their words: "As most of the warming trends that we identify can be explained on the basis of LULC changes, we suggest that in addition to considering the greenhouse gases-driven radiative forcings, multi-decadal and longer climate models simulations must further include LULC changes." [Marcel Crok, The Netherlands]	Accepted. We have included a new paragraph that incorporates some material on broader aspects of LULC than the section already contained in response to review comments.
2-1177	2	30	11	31	20	Section 2.4.1.3: the section only reports on one type of land use change effect, that of urban heat islands. Other land use changes have the potential to influence temperatures, e.g. change in the land use around a site from unirrigated to irrigated can depress maximum temperatures. Trewin 2012 found that maximum temperatures at Mildura and Griffith, both locations in regions where irrigated agriculture expanded rapidly from the 1920s to the 1940s, showed anomalous trends (relative to other sites in inland southeast Australia) in maximum temperature over the 1920-1950 period of -0.1 to -0.15 C/decade. [Government of Australia]	Accepted. We have added an additional paragrpah upon such aspects.
2-1178	2	30	11		57	UHI and LULC are not common abbreviations…just write them out in full, it would make the text easier to read. (also on next page) [Karen Rosenlof, United States of America]	Rejected. Given space limitations this is deemed a luxury.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1179	2	30	11			There is no mention about land-use change in South America. [Government of Brazil]	Noted. There are no papers to our knowledge for this region and this particular issue to assess.
2-1180	2	30	11			This section provides a very well written, balanced, and careful treatment of a contentious issue. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
2-1181	2	30	13	31	25	Parts of this section are reasonable, but parts are truly appalling! I suggest you do a proper assessment and not just a review. IPCC is an assessment! You need to assess! This section should be much smaller - as it doesn't warrant this much space. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. See comment 2-1180. We worked very had after the FOD to meet TSU guidance on how to treat contentious issues and also to address fairly the whole set of reviewer comments. We have made modifications to this section in response to other comments which may partially ameliorate the reviewer's concerns as stated.
2-1182	2	30	15			Where it says '...the modified surface affects the storage and transfer of heat' It could say '...the modified surface affects the storage and transfer of heat and water, as well as the airflows'. [JAVIER MARTIN-VIDE, SPAIN]	Accepted.
2-1183	2	30	19	30	19	It seems odd to start a sentence with “For UHI” and then refer to UHI and LULC impacts. Delete LULC for clarity and to make it clear that the estimates of the impact in the following sentence refers to UHI. [Ileana Bladé, Spain]	Noted. The analyses cited in many cases don't try to differentiate between the two so we have actually deleted the sentence opener which also reads better.
2-1184	2	30	19	30	19	Change "most" to "much". [Government of Australia]	Rejected. In our attempts to holistically cover this it became evident that most of the analysis of this issue had indeed focussed upon China in the peer reviewed literature since AR4 so the current wording reflects this. We make a similar point regarding US records in a preceding section.
2-1185	2	30	19	30	24	A most thorough investigation to date for mainland China by Zhang et al. (Zhang, A. Y., G. Y. Ren, J. X. Zhou, Z. Y. Chu, Y.Y. Ren and G.L. Tang. 2010. Urbanization effect on surface air temperature trends over China. Acta Meteorologica Sinica, 68(6): 957-966; Also see: Ren, Y.Y., G.Y. Ren, 2011, A remote-sensing method of selecting reference stations for evaluating urbanization effect on surface air temperature trends, Journal of Climate, 24 (7): 3179-3189; and Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977), using the urban minis rural method, concluded that the region-averaged annual urban warming accounts for at least 27% of the overall annual mean LSAT increase observed at the commonly used dataset of 730 stations for the time period 1961-2004. This result is more reliable than those drawn by using methods of sea surface temperature comparison (affected by the complicated off-shore sea circulations) and observations minis reanalysis (e.g. Trenberth K E. 2004. Rural land use change and climate. Nature, 427: 213), and it was obtained for mainland China (P.R.C.) as a whole rather than for eastern China as by Yang et al. (2011b) or for North China as by Ren et al. (2008). For eastern China and North China, the urban biases are larger according to Zhang et al. (2010), Ren et al. (2008; 2012), mainly due to the more rapid urbanization process. I suggest to cite Zhang et al. (2010), Ren and Ren (2011) and Ren et al. (2012), and to revise the sentence and figures (e.g. "approximately 20%..." to "at least 27% in China" or to "approximately 20%-50% in eastern China"). [Guoyu Ren, China]	Noted. Some of the reviewer's suggested papers have been included after careful consideration. Their advocated bottom line change to the conclusion is not warranted and reflects a narrow view that precludes other perspectives that are equally valid such as those made in 2-1186. This discussion has been substantivley altered as a response to this pair of thoughtful review comments.
2-1186	2	30	19	30	32	AR5 overevaluated the effects of urbanization on the temperature, so I suggest a revisement of this part based on the studies of homogenized data and high resolution models. there are 3 comments in total (references attached): Charpter 2, p30,line31-32：A paragraph after Line 31 could be suggested to add, regarding the issue of inhomogeneity of station observations, which seriously biases the estimation of the effect of urbanization in local warming trend. There have been works showing that the effect of urbanization could be overestimated by using unadjusted observational time series. Rough-resolution modeling also tends to overestimate the effect of urbanization comparing to that in high-resolution modeling. A few references are added below the suggested	Noted. See response also to 2-1185. Many of these analyses have been included in the rewrite here and the role of homogenized vs. raw series analysis and different homogeneity assessments has been stressed in the revisions. The bottom line conclusion is now caveated to sound less deterministic and permit uncertainty in either direction. The fact that homogeneity adjustment reduces if not removes UHI

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						paragraph. Charter 2, p30, line 19-24:In this paragraph, it looks like some overwhelming point of views that the UHI effect is approximately 20% in Eastern China and of the order 0.1℃ per decade nationally, but it is not the case, in northeast China, this effect is quite small. We, and other scientists gave some evidences in some literature, such as Li et al (2010) in Theor Appl Climato, Yang et al (2011) table 1. It is worth noting that inhomogeneity of the observation series (due to changes of observing sites and protocols, etc) biases the estimation of the effect of urbanization. For instance, Peterson (2003) concluded that when the inhomogeneity was adjusted, no significant urbanization effect could be detected in regional average temperature series in the US. The contribution of urbanization to the observed warming trend in the temperature series of Beijing, a site well-influenced by the rapid expansion of the city during the last few decades, is estimated as less profound if the analysis is based on homogenized series (about 40%, Yan et al 2010) than that based on original observations (more than 80%, Ren et al 2007). It is also interesting that the simulated effect of urbanization on regional warming in China based on fine-resolution (3-km) regional modeling (Wang et al., 2012) tends to be half less than that based on rough-resolution (30-km) modeling (Feng et al., 2012). Therefore, it is suggested that previous works based on either unadjusted observations or rough modeling could have overestimated the effect of urbanization in general. References: 1、 Li Q., Li W., Si P., Gao X., Dong W., Phil Jones and Cao L., 2010, Assessment of surface air warming in northeast China, with emphasis on the impacts of urbanization, Theor Appl Climatol, DOI 10.1007/s00704-009-0155-4 2、Yang, X.C., Hou, Y.L. and Chen, B.D., 2011b. Observed surface warming induced by urbanization in east China. Journal of Geophysical Research-Atmospheres, 116: 12. 3、Feng JM, YL Wang, ZG Ma, YH Liu. 2012. Simulating the regional impacts of urbanization and anthropogenic heat release on climate across China. J. Clim., DOI: 10.1175/JCLI-D-11-00333.1 4、Peterson TC 2003: Assessment of urban versus rural in situ surface temperatures in the contiguous United States: No difference found. J Clim, 16: 2941-2959 5、Ren GY, Chu ZY, Chen ZH, et al. 2007: Implications of temporal change in urban heat island intensity observed at Beijing and Wuhan stations. Geophys Res Lett, 34: L05711 6、Wang J, JM Feng, ZW Yan, GS Jia, YH Hu. 2012. Nested High Resolution Modeling of the Impact of Urbanization on Regional Climate in Three Vast Urban Agglomerations in China. J. Geophys. Res., accepted in September 2012 (submitted in May 2012) 7、Yan Z, Li Z, Li Q, Jones PD 2010: Effects of site-change and urbanisation in the Beijing temperature series 1977-2006. International Journal of Climatology 30 (8) 1226–1234 [Qingxiang Li, China]	impacts is now specifically documented.
2-1187	2	30	22	30	22	Citation style wrong [Peter Burt, United Kingdom]	Editorial
2-1188	2	30	22	30	23	Delete commas after 'et al.' [Ian Simmonds, Australia]	Editorial
2-1189	2	30	22	30	26	Some of these Chinese papers use NCEP. There isn't really an excuse for anyone using NCEP if they are only going to look at periods since 1979. They should be using ERA-Interim. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. It is unclear to us that use of NCEP should preclude their assessment and some of these studies preceded ERA-Interim's availability. Changes have been made in this paragraph in response to other reviewer comments including stressing the uncertainty.
2-1190	2	30	23	30	23	Does 20% refer to a fractional area of China, or to a fraction of the trend magnitude? [Dian Seidel, United States of America]	Noted. We have clarified in edits.
2-1191	2	30	23	30	24	what is "the effect"? What does "implicitly ascribes" mean? [Kevin Trenberth, United States of America]	Noted. We have deleted implicitly here.
2-1192	2	30	27	30	27	“apparently rural stations show plausibly unaffected behavior”: the use of both apparently and plausibly seems wishy-washy [Ileana Bladé, Spain]	Accepted. These qualifiers have been deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1193	2	30	27	30	27	Here is where you are reviewing not assessing. 'Conversely' is a poor choice of word to link two unrelated areas. London is a completely different place than cities in China. The point you need to make here is that you can't infer global-land scale trends from a single location or from a specific region. You need to emphasize the studies that have looked over much larger parts of the world - these come in the next paragraph one lines 33-44. This is the best part of this section. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Edits have been made to the remainder of this paragraph to make these aspects significantly clearer.
2-1194	2	30	27	30	27	What is meant by 'plausibly unaffected behaviour'? No discernable background trend? [Kate Willett, United Kingdom]	Noted. No significant trend difference from SST driven expectations. This has been clatrified in edits here.
2-1195	2	30	27	30	30	Trewin 2012 found similar results for 1910-2011 for Sydney to those reported by Jones for London. [Government of Australia]	Accepted. A sentence has been added noting that Trewin found no growing urban influence for three Australian cities (the paper notes this extends also to Melbourne and Hobart).
2-1196	2	30	29	30	29	what are “regionally indicative trends” ? [Ileana Bladé, Spain]	Noted. It means trends that are consistent with other stations within the region. Edits have been made to clarify this here.
2-1197	2	30	30	30	30	Start sentence with a connector. I suggest “Yet”. [Ileana Bladé, Spain]	Editorial
2-1198	2	30	30	30	31	Zhang et al. (Zhang, A. Y., G. Y. Ren, J. X. Zhou, Z. Y. Chu, Y.Y. Ren and G.L. Tang. 2010. Urbanization effect on surface air temperature trends over China. Acta Meteorologica Sinica, 68(6): 957-966) also found that the urban warming in northern Xinjiang, an arid region in northwestern China, is much smaller or even negative for most urban stations. They related this to the possibly enhanced "Oasis effect" near the urban stations. Further works are needed. [Guoyu Ren, China]	Accepted. Sentence added to this effect.
2-1199	2	30	33	30	34	Here you say that estimates of large-scale temperature trends have either avoided urban sites or applied corrections based on urban-rural comparisons. But as in my review comments for the FOD, I question how the first claim can be made since CRU, GISS and NOAA all rely on the GHCN archive, and over 60% of the recent Southern Hemisphere land surface data come from urban airports. So they are not "avoiding" urban sites, and it is even questionable whether there are many countries that have enough rural data to support the estimation of corrections. If you have any studies that specifically "avoid urban sites" then put the citation of that study at that specific point in the sentence, rather than nesting the citations all together at the end of the sentence, where readers can't tell which, if any, support the former claim. [Ross McKitrick, Canada]	Noted. Text has been removed to allow for more discussion of results to avoid over-lengthening this section.
2-1200	2	30	33	30	34	Is "adjusted their [urban] data to match regional rural trends" really the way this work was done? If so, then of course the systematic difference will have been removed. (line 36). [Dian Seidel, United States of America]	Noted. Edits have been made to clarify the meaning here.
2-1201	2	30	34	30	37	This sentence is not clear, particularly what “vice-versa” means. If the urban data were adjusted to match rural data trends why is it noteworthy that the adjustment removed the apparent systematic differences ? [Ileana Bladé, Spain]	Noted. Have deleted the end of this sentence.
2-1202	2	30	42	30	44	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 section is already overly-long and this degree of extra detail would exacerbate this issue.The sentence has been moved elsewhere in the section but no additional changes have been made.
2-1203	2	30	42	30	44	This is an important change from the previous conclusion by Jones (1990), which has been heavily cited by the IPCC reports. However, it is somehow less convincing if it was based on the comparison of coastal land and sea surface temperature trends. [Guoyu Ren, China]	Noted. No changes requested or made.
2-1204	2	30	46	30	46	The McKitrick and Michaels 2004 paper was shown to be fundamentally flawed as it used software which interpreted the latitude in radians rather than degrees. An erratum was subsequently published although the paper was not withdrawn. It may be better not to cite this paper. The later McKitrick papers cited are not affected by this issue. [Government of Australia]	Noted. The citation is necessary for context otherwise what follows makes no sense.
2-1205	2	30	46	30	50	This paragraph refers to the claim in the AR4 that the observed correlation between the spatial pattern of	Rejected. The current text is a fair reflection of the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						warming and the spatial pattern of socioeconomic change becomes statistically insignificant after controlling for the effects of atmospheric circulation changes. That claim was the basis on which the AR4 set aside concerns of non-climatic bias in the surface temperature data, which in turn was an essential assumption for many other conclusions in the AR4. You now acknowledge that this claim was made without any supporting evidence, which is quite an admission. You also need to mention that the only paper to investigate the matter concluded it was not only unsupported but false. You cannot create the insinuation that supporting evidence might have existed but was not provided, especially since in the references at the end of the chapter you list my 2010 paper "Atmospheric Circulations Do Not Explain the Temperature-Industrialization Correlation", but do not mention it in the context of this discussion. So a sentence should be addedsaying something like "The claim was tested in McKitrick (2010) and shown to be likely untrue." You should also list all the follow-on claims in the AR4 that depended on the assertion that the land record was unbiased. [Ross McKitrick, Canada]	issue as understood by the chapter author team.
2-1206	2	30	46	30	57	McKitrick and Michaels (2004) was dealt with in AR4 and shown to be wrong then. It is still wrong. Why are all these papers cited included here when they have no credibility. Associations or correlations are not causal. [Kevin Trenberth, United States of America]	Noted. The citation is necessary for context otherwise what follows makes no sense.
2-1207	2	30	46	31	2	The papers by McKitrick only look at periods since the 1950s and also since 1979. They are invalidated by ERA-Interim. These studies assume that there is the same socioeconomic development over large countries such as Russia. This makes no sense whatsoever. These papers say that they remove the atmospheric circulation influence, but the papers only look at annual timescale data. Any climatologist knows that circulation influences are seasonally dependent, so you can't remove the effect by just looking at annual averages. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. We make the point that reanalyses agreement is an essential component in forming our bottom line conclusion for just such reasons.
2-1208	2	30	46	31	10	This paragraph makes little sense. The ERA-I does not show more warming in Tsfc. But, in any case, these are oblique model depictions of the surface according to the reanalysis used. How can model (i.e. reanalyses) average depictions be used to invalidate an empirical study that specifically studied regional variations? Large-scale reanalyses are woefully inadequate in the high-vertical resolution temperature variations in the boundary layer (especially the nocturnal boundary layer.) Since TMean is being discussed here, there is the strong liklihood that the Reanalyses do not capture the coupling/decoupling of the nocturnal boundary layer that is so important in the value of TMin ... only small changes here can have large changes in TMin and thus TMean. As is, this paragraph is easily challengeable. The McKitrick analysis still stands as an excellent study of the issue. From my own perspective, which is obviously different than that of the IPCC authors, the arguments of McKitrick will prevail in an open, cross-examined analysis of this issue. [John Christy, United States of America]	Noted. We disagree with the reviewer's assessment here. Regardless the reviewer has suggested no explicit changes and so none are made.
2-1209	2	30	46	31	10	Here is the main section where you are reviewing and not assessing. It seems based on two of the papers in this paragraph that you come up with the conclusions at the end of the section. The final conclusions would be reasonable, if you changed likely to very likely. All the studies that have looked at the data at large scales in the previous paragraph have concluded that the urban effect is negligible. ERA-Interim supports that is as well - see Figs 11 and 13 of Jones et al. (2012) - paper used in Table 2.4. ERA-Interim does take input from surface temperature, but it doesn't have a major influence. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. We have tried to make this more of an assessment but we do need to include some review component in addition. Jones et al. reference has been added where compariosns to reanalyses are discussed.
2-1210	2	30	46	31	20	Your summary of the state of play regarding analysis of evidence of contamination of the surface temperature record is reasonable, with one exception. You say a "hypothesized residual warming artefact..." is inconsistent with models. It's not "hypothesized", it's "observed", as your own text earlier noted. So if it's inconsistent with models, that may indicate a problem with the models. In any case, the wording here is much better than the unfortunate tone of earlier assessments. But you need to follow your own argument to its logical conclusion. You have now clearly indicated that several different methods have been applied, they have yielded substantially different conclusions about the data quality issue, and because they are incommensurable in their approaches it is not currently possible to adjudicate the debate. That is the plain meaning of these paragraphs, and I think it does justice to the literature. But, having admitted the matter can't be decided, you go ahead and do so anyway by making up an arbitrary number, concluding it is "likely" that residual biases are no more than 10% of the underlying trend globally and 25% regionally. Where do these numbers come from? Pulling a number out of thin air doesn't turn a guess into a quantitative science. The sentence would be less	Noted. Based upon the balance of reviews on this section only minimal changes are made including some restructuring and reshuffling.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						inconsistent with the 2 paragraphs that precede it if you say "Notwithstanding the foregoing, we will assume that the magnitude of bias is small enough to be ignored, and we hope that turns out to be the case." [Ross McKitrick, Canada]	
2-1211	2	30	46			The authors note that McKitrick and Michaels (2004) and de Laat and Maurellis (2006) concluded that UHI and related LULC have caused 'MUCH' of the observed LSAT warming. However on page 31 it is stated that no published study since AR4 has implied that all, or even the majority of, the recent LSAT warming trend can be accounted for by UHI and LULC effects. The use of the word 'much' implies majority. How much of the trend do McKitrick and Michaels and de Laat and Maurellis attibute to UHI and LULC? This is an important point since a major finding is that UHI and LULC account for likely no more than 10% (25% regionally) of the warming. [Government of United States of America]	Noted. There is no inconsistency here as these earlier studies are prior to AR4. But for avoidance of doubt we have redacted the sentence in the concluding paragraph.
2-1212	2	30	47	30	48	too much unexplained abbreviations [Andreas Walter, Germany]	Noted. All abbreviations used have been long-handed earlier. Given space restriction stipulations it is not possible to long-hand each and every occurrence of these phrases.
2-1213	2	30	50	30	50	. AR4 provided → but provided [Ileana Bladé, Spain]	Editorial.
2-1214	2	30	50	30	50	Bravo for at least mentioning this problem with AR4 [John Christy, United States of America]	Noted.
2-1215	2	30	50	30	50	Remove this sentence about what AR4 said. AR4 made the same arguments about circulation influences as above as their evidence. There is no referenence to this in any published paper. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. After careful analysis and discussion in the final lead author meeting it was decided to retain this sentence.
2-1216	2	30	51	30	51	The McKitrick and Michaels (2007) assertion is inconsistent with changes in sea-surface temperatures, a point eventually made much lower down. More needs to be made of this inconsistency since it implies the M and M (2007) conclusion is most likely wrong. [Government of Australia]	Noted. Based upon the balance of reviews on this section only minimal changes are made.
2-1217	2	30	53	30	53	Again there is this linking phrase 'In contrast'. It is not just the work of Schmidt (2009) that is in contrast, but all the papers in the previous paragraph. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Use of in contrast has been removed here.
2-1218	2	30	55	30	56	'…modified analyses by McKitrick (2010) and McKitrick and Nierenberg (2010) still yielded apparently significant evidence for such contamination of the record.' Why apparently? Is this word used in those articles? If not, it should be withdrawn here. [Alice Grimm, Brazil]	Editorial.
2-1219	2	30	57	31	1	McKitrick (Submitted) noted how such studies need not be contradictory: This seems to refer to the two studies in the previous sentence which are both authored by McKitrick; this is not what is meant. It is also not clear what rebuttal by Parker (2006) is in question here since the Parker 2006 paper is not mentioned in this section. Also add "A new study" by McKitrick before (Submitted). [Ileana Bladé, Spain]	Noted. This sentence has been removed as the submitted paper did not meet the acceptance deadline for inclusion
2-1220	2	30				Table 2.5. Why does not marge this table with Table 2.6? HadSST3 is repeited in Table 2.6 [Government of Chile]	Misplaced comment. In response - the distinction is to ensure against the possibility for including older versions of multiple datasets in all other trend tables. Elsewhere only the latest dataset from each centre is used. Here a different conclusion is wanting to be drawn - about the effects of the change in dataset from this one group and it makes more sense as a stand-alone table.
2-1221	2	31	1	31	10	this is a poor assessment and a lot more weight should be given to Simmons et al. and ERA-interim which is also much better than other reanalyses. This discussion does not discriminate between good studies and those which make no sense and are not physical. The issue of coverage and inadequate weighting of the warming in the Arctic in particular in some analyses such as HADCRU should be brought out. [Kevin Trenberth, United States of America]	Noted. The spatial sampling aspects of Simmons et al are addressed in a later section where they are deemed more appropriate. The Simmons et al analysis is one amongst many which are all providing strong support for the bottom-line conclusion here.
2-1222	2	31	1	31	20	Much of these 20 lines are good arguments, but I see these coming to a much stronger conclusion that it very	Noted. It is not clear that very likely is justified given

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						likely that residual biases account for no larger than 10% of the warming. You are only talking about a possible 10%, which is an order of magnitude, which is precisely what was said in papers referred in previous IPCC Reports. Surely as the effects are at a maximum 10%, we should have 90% confidence in this. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	the presence of remaining uncertainties. Regardless, we have recast this finding in terms of it being unlikely that more than 10%.
2-1223	2	31	2	31	10	Another 'In contrast', but here you come to the right conclusions. More evidence is the direct comparisons in Figs 11 and 13 of Jones et al. (2012). [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. This section has been made its own pargraph and the opening sentence modified. The Efthymiadis and Jones paper has been moved down to this section.
2-1224	2	31	2	31	10	Caution had to be paid when to compare the analyses using the reanalysis products. The reanalysis products may be improper to be used for studies and verifications of long-term trends of the LSAT (there are a few studies which show the impropriety). We should pay more attention to the issue if we have already found it, and we do not need to shift to other data which are more incredible when used for analysis of trends. [Guoyu Ren, China]	Noted. Comment makes no specific request for changes and none are made. The use of reanalyses comparisons has been repeated for different regions with different periods and reanalyses and implications for global LSAT comparisons have been found to be robust.
2-1225	2	31	2	31	10	The last sentence of the this paragraph could be brought up to line 5 after Vose et al. 2012a). It makes an absolutely essential point that should be noted before going on to describe the reanalyses findings in more detail. [Kate Willett, United Kingdom]	Accepted and sentence modified slightly.
2-1226	2	31	6			Better to write "observational data sets" not "observed data sets". It is the atmosphere that is observed, not the data sets. [Adrian Simmons, United Kingdom]	Editorial.
2-1227	2	31	7	31	7	but is green-house gas a social economic indicator? Would it be better to replace this with "land use and land cover change"? [Xuebin Zhang, Canada]	Accepted. Text has been changed here and elsewhere.
2-1228	2	31	9	31	10	This is not 100% true, in that ECMWF reanalyses do analyze land surface air temperature. See Dee et al. (2011) Section 2.2. Reanalysis temperautre is robust owing to the many sources of atmospheric temperature for data assimilation. [Michael Bosilovich, United States of America]	Noted. We have modified to make clear that they do not directly assimilate the varible. In ERA and JRA products they nudge the soil moisture constraint. Stating this is too detailed for the level of assessment here so changed wording hopefully better reflects that some reanalyses use the temperatures indirectly.
2-1229	2	31	9	31	10	This statement is incorrect. Some renalyses assimilate LSAT measurements. ERA-40 and ERA-Interim for example. Simmons et al. (2004) discuss this. The same is true of the JRA-25 reanalysis (Onogi et al., 2007). [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1228
2-1230	2	31	12	31	12	Remove "confounding". UHI and LULC are real influences on temperature. [Government of Australia]	Accepted. Changes have been made to this text to account for this.
2-1231	2	31	12	31	20	2.4.1 Watts et al. (2012) appear to find results at odds with the statement in lines 16-20. Recommend inserting in Line 17: "However, recent analysis by Watts et al. (2012) (public draft pending submission) using the Leroy (2010) classification system found strong differences in thrends between the best weather stations (classes 1&2) and the poor weather stations (clases 3-5). Raw mean temperature trends for well sited stations are 0.145 deg C per decade lower than adjusted mean temperature trends for poorly cited stations. These results suggest major issues that need serious evaluation." Reference: "Anthony Watts, Evan Jones, Stephen McIntyre & John R. Christy, An area and distance weighted analysis of the impacts of station exposure on the U.S. Historical Climatological Network temperatures and temperature trends. Pre-Print Draft Discussion Paper", Posted at: http://wattsupwiththat.files.wordpress.com/2012/07/watts-et-al_2012_discussion_paper_webrelease.pdf Leroy, M., 2010: Siting Classification for Surface Observing Stations on Land, Climate, and Upper-air Observations JMA/WMO Workshop on Quality Management in Surface, 937 Tokyo, Japan 27-30 July 2010 [David L. Hagen, United States of America]	Rejected. Comment refers to unsubmitted and unpublished results outside the purview of the assessment as charged by IPCC.
2-1232	2	31	12	31	31	The value of 10% is far too large and not justifiable. This is written in a very nagative way. The information available is much more credible than given here. Also p 38 l 44 [Kevin Trenberth, United States of America]	Noted. We have recast this finding based upon reviewer comments and author team discussions.
2-1233	2	31	18	31	18	comparisons: is “differences” meant here ? [Ileana Bladé, Spain]	Editorial. We mean comparisons here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1234	2	31	18			Change "reanalyses" to "reanalysis". [Adrian Simmons, United Kingdom]	Editorial.
2-1235	2	31	19	31	20	Somewhere this could be useful to state as an uncertainty that needs a little more efffort to resolve [John Christy, United States of America]	Noted. Guidance is that IPCC should be policy neutral and this is likely straying from that. No changes made.
2-1236	2	31	19	31	20	I suggest to revise the sentence "it is concluded that it is likely that residual biases account for no larger than 10% of the warming trend globally and 25% regionally in rapidly developing regions" to read as " it is concluded that it is likely that residual biases account for no larger than 15% of the warming trend globally and 40% regionally in rapidly developing regions like eastern China". [Guoyu Ren, China]	Taken into account see response to 2-1232
2-1237	2	31	19	31	31	Need to be very clear about what the "10% of the warming" refers to. I assume this is 10% of the warming observed over land (as compared to the warming over the globe). Also, what is the basis for arriving at this 10% figure. [Government of Australia]	Accepted. LSAT added.
2-1238	2	31	22	33	11	Section 2.4.2: This section on Sea Surface Temperature and Marine Air Temperature is a succinct and accurate representation of the current body of knowledge on climate data records of sea surface temperature and marine air temperature. [Government of Australia]	Noted.
2-1239	2	31	24	31	31	Somewhere, possibly here, you should say that more raw data have been digitized since AR4 and more will be in the near future. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected: the 2nd sentence of this paragraph is already saying that "data completeness has improved"; future work plans are generally outside of the AR5 mandate to assess what is in the peer-reviewed literature. There also exists a plot and discussion in the appendix that details with such issues.
2-1240	2	31	25	31	25	Enye missing above second 'n' of Nino [Peter Burt, United Kingdom]	Editorial: spelling consistency to be ensured in the final draft.
2-1241	2	31	25	31	25	Correct "El Nino" to the correct title "El Nino-Southern Oscillation". The failure to correctly name the oscillation reflects badly on the report because it leads the reader to make false interpretations and assumptions. [John McLean, Australia]	Accepted.
2-1242	2	31	33			"uncertainty" would more correct than "error" here [Christopher Merchant, United Kingdom]	Editorial: consistency of terminology to be ensured in the final draft.
2-1243	2	31	35	31	35	Why is the widely-used ERSST v3b dataset not discussed?Specifically, which of the adjustements mentioned for HadSST2 and HadSST3 are also included in ERSST v3b< and how large are the diferences in areas with observations? [Geert Jan van Oldenborgh, Netherlands]	Rejected. B/c of space limitations, detailed description of all SST products could not be included. ERSST v3b is illustrated by Fig. 2.19 and Table 2.6. Table 2.A.5 (in Appendix) provides, in a uniform way, some very basic information about all SST products used in this chapter. The goal of subsection 2.4.2.1 was to summarize progress since AR4, rather than to describe all currently popular SST data sets. Presenting the progress required showing some results about HadSST3 (a new data set) and comparing it with HadSST2 (which was used in AR4): ERSST v3b could not be used for this purpose and thus ended up illustrated less than HadSST2&3 in this subsection.
2-1244	2	31	36	31	45	Please state clearly that broad scale adjustments are made to sea surface temperatures because each method of determining sea temperature requires adjustment but the methods used by each ship were not recorded. [John McLean, Australia]	Rejected. That adjustments were made and that they affect global mean estimates is said here already. The extent to which measurement methods for individual observations are unknown is presented in Figure 2.17.
2-1245	2	31	37	31	45	Figure 2.17 is mentioned before Figure 2.16 [Jeffrey Taylor, United States of America]	Editorial: Figures will be renumbered

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1246	2	31	39	31	40	Change "Early data were systematically biased cold" change to "Early data suffered a systematic cold bias" [Government of Australia]	Editorial. Have made an edit here along the lines suggested.
2-1247	2	31	40	31	40	Replace 'a great deal of' with 'some' - 'a great deal of' seems to be overstating the case, particularly in the tropics [Government of Australia]	Editorial: have removed the qualitative assessment here.
2-1248	2	31	41	31	42	"This effect has long been recognised". Brooks (1926) could be cited here. Brooks, C. (1926), Observing water-surface temperatures at sea, Monthly Weather Review, 54(6), 241{253, doi:10.1175/1520-0493(1926)54<241:OWTAS>2.0.CO;2. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1249	2	31	42	31	43	Best also to mention ERSST as it takes a different approach to bucket bias adjustments. Folland and Parker, Rayner et al. are only half the story. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1250	2	31	43	31	44	The SST adjustments were made using physical models of buckets, tuned to optimise the annual cycles of SST and guided by NMAT and historical metadata. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. "Other sources" is used in lieu of physical model used by the UKMO. Spelling out the UKMO method here would require also to distinguish it from the alternative ERSST approach. Space limitations prevent us from getting into this level of detail.
2-1251	2	31	43	31	45	This sentence is wrong. The SST adjustments are not made using NMATs. They have been made with models of the different types of buckets in use prior to 1941 and assumptions about when they were used. NMAT was just used to indicate which period of SST measurements was likely in error. NMAT measurements are not used to make the adjustments. The fact that the two are independent reinforces the need for the bucket-type adjustments. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. The sentence is correct as written. It is obviously correct for the ERSST corrections. UKMO corrections used NMAT data to tune their model.
2-1252	2	31	53	31	53	Reference "Kennedy et al., 2011…" in caption to Figure 2.16 does not agree with reference in caption for Figure 2.16 on page 158. [Birgit Hassler, United States of America]	Accepted
2-1253	2	31	55	31	56	It's not true that measurement methods with "smaller" biases were used after 1941. What happened was that there was a mix of measurement methods of differing sign so the overall bias was generally smaller and the individual systematic errors were harder to detect. Kennedy et al (2011a) shows ships in modern period with biases of order 1K. The two parts of the sentence make it sound as if the aim of the changes in the observing network were deliberately made to reduce bias. This may be part of the story, but if it occurred it was more by chance than design. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account and edited accordingly
2-1254	2	31	58	31	58	The individual biases are not "smaller". I would suggest removing "smaller" or making the qualification that it is the bias in large scale averages that is being referred to. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account and edited accordingly
2-1255	2	31				Table 2.6. Why does not include HadSST" in this Table and fix a little the text) [Government of Chile]	Rejected: only the most recent SST data sets from each group are included into this table for consistency with similar tables elsewhere.
2-1256	2	32	1	32	6	Did you audit HadSST3 before using it? Brohan et al incorporated SST data from Royal Navy ships into HadSST3 but an inspection of the data for all ships whose names start with A, B or C (about 13% of the total RN ships) reveals that 49.5% of observations were taken when the ships were in port, which means shallow water, often fed by rivers and potentially with local heat sources. This begs the question of how frequently "in port" observations were used in HadSST3 and what impact their removal might have. Include the appropriate reservations in your statements. [John McLean, Australia]	Rejected. Details of data quality control for HadSST3 is presented in Kennedy et al. (2011c) which is referenced in the previous paragraph. It is not the job of IPCC to audit each and every dataset in substantive detail.
2-1257	2	32	1	32	6	Please show a graph of NH, SH and global SST coverage so that the readers can understand the limitations of the data. In the SH, SST coverage was a mere 6% of the entire hemisphere in August 1945, which means that the temperature anomaly for the ocean in the rest of the hemisphere was assumed to be the same as the average of that 6%. [John McLean, Australia]	Rejected. Please note that (1) Figure 2.16 is meant to show effects of data processing methods on global mean estimates from uninterpolated data sets, not to present best estimates of the global temperature mean; (2) Figure 2.A.1 in Appendix shows the percentage of sampled area as a function of time; (3) space limitations preclude the authors from presenting SST results by hemisphere.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1258	2	32	2	32	2	should 'ERI-to-bucket' be 'bucket-to-ERI'? [Government of Australia]	Taken into account: sentence rewritten. During WWII most of the observations in the historical database were from ERIs at US ships; after the end of the war the portion of measurements by uninsulated buckets from the UK ships increased dramatically (Thompson et al. 2008). This de facto ERI-to-bucket transition in 1945 among the available observations had to be adjusted.
2-1259	2	32	2	32	2	Define ERI. Replace "ERI-to-Bucket" by "bucket-to-ERI" as buckets measurements are older than ERI ones. Explain why buoys SST are consitently 0,2°C colder than ERI SST in figure 2.17. [Government of France]	Accepted: to define ERI. Rejected: to replace "ERI-to-bucket" by vice versa -- see 2-1258. Taken into account: to explain ERI-buoy difference.
2-1260	2	32	2	32	2	The acronym ERI (presumably Engine Room Intake) is not defined. A very brief explanation of why the temperature bias arose, linked to the artefact of warm-biased ERI temperatures, may be helpful here. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted: to define ERI. Taken into account: to explain ERI bias -- paragraph revised,
2-1261	2	32	2	32	2	"mostly ERI to mostly bucket transition". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial: sentence revised
2-1262	2	32	5	32	5	For periods → Nevertheless, for periods [Ileana Bladé, Spain]	Editorial: sentence revised
2-1263	2	32	9	32	9	Units are missing for table values [Ileana Bladé, Spain]	Taken into account. See response tio comments on earlier similar section tables.
2-1264	2	32	9	32	11	Table 2.5 Add the temperature and time units. This appears to be deg C/decade but that is nowhere stated. [David L. Hagen, United States of America]	Taken into account. See response to 2-1263
2-1265	2	32	9	32	11	Caption of Table 2.5: Add units of described trends. [Birgit Hassler, United States of America]	Taken into account. See response to 2-1263
2-1266	2	32	9	32	11	Units? Presumably degrees/decade. [James Renwick, New Zealand]	Taken into account. See response to 2-1263
2-1267	2	32	9	32	12	Table 2.5 some idea of the parametric uncertainty in HadSST3 would be a useful piece of additional information particularly in the period 1979-2011 when the parametric uncertainty is particularly important relative to other uncertainties. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected: unclear how to present this material unambiguously without significant increase in length.
2-1268	2	32	9	32	12	Table 2.5 Caption does not give units of trends [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1263
2-1269	2	32	9			Specify units (°C/decade?) in Table 2.5 caption [Franco Desiato, Italy]	Taken into account. See response to 2-1263
2-1270	2	32	20	32	20	Reference "Kennedy et al., 2011…" in caption to Figure 2.17 does not agree with reference in caption for Figure 2.17 on page 159. [Birgit Hassler, United States of America]	Accepted
2-1271	2	32	22	32	25	Again, here the word "error" wrongly used as a synonym of "uncertainty". One can correctly say that the "error distribution" is estimated, but it is not the error that is estimated. WMO now recommends following metrological nomenclature which clearly distinguishes error and uncertainty. [Christopher Merchant, United Kingdom]	Editorial: revised to "random error magnitude"
2-1272	2	32	22	32	34	Error and uncertainty are confused here. In the first sentence I think it is worth clarifying that "The traditional approach to estimating uncertainty due to random error in in situ SST data assumed the independence of individual measurement errors" [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial: revised accordingly
2-1273	2	32	23	32	23	Kent and Berry (2008) did not introduce platform-dependent biases, they introduced the idea of needing to consider biases correlated for individual platforms [Elizabeth Kent, United Kingdom]	Accepted
2-1274	2	32	25	32	25	I think the final use of "error" in this line should read "uncertainty" [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-1275	2	32	28	32	28	MAT not introduced [Andreas Walter, Germany]	Editorial. MAT has been defined and then used in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							several of the preceding components of the marine temperatures section.
2-1276	2	32	29	32	29	Should "daytime" be inserted before MAT? If not, the sentence is confusing as written. [Dian Seidel, United States of America]	Accepted
2-1277	2	32	33	32	33	multidecadal: wouldn't “long-term” be better here ? [Ileana Bladé, Spain]	Editorial: not clear if this suggestion improves the text.
2-1278	2	32	37	32	38	SST estimates from satellites are just that - estimates of SSTs from satellites. It doesn't make sense to state they are designed for "meteorology" purposes. Indeed, one suspects that the most widespread use of these data and reason for their collection is applications in climate (such as seasonal prediction) and resource management. [Government of Australia]	Taken into account and revised accordingly
2-1279	2	32	38	32	38	Should make reference to the fact that a key issue with satellite and other SST data is that they all represent slightly different depths of the near surface ocean. This introduces differences which need to be accounted for - these are not errors, simply measurements of slightly different metrics which can't simply be spliced together. [Government of Australia]	Rejected: discussed in Appendix, sec 2.A.4.3.2
2-1280	2	32	39			IR for infra-red has not previously been defined in this chapter [Christopher Merchant, United Kingdom]	Editorial
2-1281	2	32	47	32	51	Mention that ATSR has now stopped with ENVISAT's demise. Also presume that AATSR should be ATSR. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account: enddate for the ATSR data set is added, AATSR abbreviation introduced.
2-1282	2	32	48			Since it is a series of 3 sensors, should it read "their combined record"? [Christopher Merchant, United Kingdom]	Accepted
2-1283	2	32	49	32	49	to support atmospheric effects removal : allow? provide? (support seems an odd choice) [Ileana Bladé, Spain]	Accepted
2-1284	2	32	49	32	49	The independence of ASTR from the in situ observing system is important. But what should also have been already mentioned is the dependence of the IR & MW SST on the in situ network for removal of bias. [Elizabeth Kent, United Kingdom]	Accepted
2-1285	2	32	49			While "atmospheric effects removal" is correct, it won't mean much to general readers. The key point is that they are intended to support SST estimation without recourse to in situ observations, and I propose the text should express it this way. [Christopher Merchant, United Kingdom]	Taken into account (combined with 2-1284).
2-1286	2	32	51			"product seems" or "products seem" but not "product seem" [Christopher Merchant, United Kingdom]	Editorial
2-1287	2	32	52			"the agreement" -- no comparison has been defined in this shortened version of the previous text, so this no longer makes sense. Needs to be reworded to something like: "Figure 2.18 shows the level of agreement found between independent ATSR and in situ observations, averaged to global monthly means. (Merchant et al., in press)." [Christopher Merchant, United Kingdom]	Editorial: not clear if this suggestion improves the text. In combination with the previous sentence, the "agreement" is clearly meant to be between ATSR and in situ data. More details are given in the caption of Figure 2.18.
2-1288	2	32		33		tables 2,5 and 2,6 are also confusing similarly to table 2,4 [Government of France]	Editorial: meaning of numbers has been clarified.
2-1289	2	32				Table 2.5: what do the numbers mean in the table? Provide units e.g. [K/decade] [Alexander Loew, Germany]	Taken into account in modifications to the first table caption (Table 2.4) which this one cross-references.
2-1290	2	32				Table 2.5: Units should be added to either the caption or the table [Jeffrey Taylor, United States of America]	Taken into account. See response to 2-1289
2-1291	2	32				Figure 2.17: Why not include the "unknown yellow measurements" in the top panel in the bottom panel as well? [Jeffrey Taylor, United States of America]	Rejected: unclear how it would help the interpretation of data biases
2-1292	2	32				Figure 2.17: ERI (Engine Room Intake) should be defined in the second line not the fourth [Jeffrey Taylor, United States of America]	Editorial
2-1293	2	33	5	33	11	Little details is given to help understand the figure. It ought to be noted that the SST retrievals from ATSR1 are less reliable than from ATSR2 and AATSR, otherwise the obvious discrepancy between ATSR and HadSST3 goes unremarked. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected: it is already noted (l.7-8) that D3 are more accurate than D2, and these are shown by different line types, as identified in the caption and in the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							legend.
2-1294	2	33	13	33	13	Change "interpolated" to something else. The distinction between "gridded" and "interpolated" SSTs is also very subtle and it is not clear why interpolated analyses are considered separately here. Univariate statistical/optimal interpolation just reflects a slightly more complicated weighted average, and as such is not fundamentally different from grid averaged datasets. [Government of Australia]	Taken into account and revised accordingly
2-1295	2	33	20	33	20	I think it is correct to say "some products use the AVHRR satellite data as well". None so far use ATSR. It's worth checking whether ATSR is used in any of them. I know that It isn't used in HadISST1. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-1296	2	33	25	33	32	It seems odd not to have one single sentence commenting this table/figure. [Ileana Bladé, Spain]	Taken into account and revised accordingly
2-1297	2	33	30	33	30	Add "interpolated" to the Table label. [Government of Australia]	Taken into account and revised accordingly
2-1298	2	33	30	33	31	Explain that some of the differences in trend in Table 2.6 are due to non co-location of interpolated compared to spatially incomplete data sets. Interpolated data sets estimate trends over a markedly larger total global ocean area. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account and revised accordingly
2-1299	2	33	30	33	31	Table 2.6. Add the temperature and time units. This appears to be deg C/decade but that is nowhere stated. [David L. Hagen, United States of America]	Accepted
2-1300	2	33	30	33	31	Caption of Table 2.6: Add units of described trends. [Birgit Hassler, United States of America]	Taken into account (see 2-1300)
2-1301	2	33	30	33	31	Chapter 1 said that period 1998 to 2011 would be shown, so please include it in this table. [John McLean, Australia]	Rejected. This issue is discussed in the context of comibined land and ocean global products in the next section.
2-1302	2	33	30	33	31	Units? Presumably degrees/decade. [James Renwick, New Zealand]	Taken into account (see 2-1300)
2-1303	2	33	30	33	31	Pleaes add some explanation for the huge differneces in trends between the datasets in Table2.6, in particular HadISST1 and HadSST3. Have the corrections that go into HadSST3 also been incorporated in HADISST1? [Geert Jan van Oldenborgh, Netherlands]	Taken into account and revised accordingly
2-1304	2	33	30	33	32	It would be useful to include HadNMAT2 in Table 2.6. The series is in Figure 2.19. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected: not extended to cover 2012 by submission deadline.
2-1305	2	33	30	33	32	Table 2.6. some idea of the parametric uncertainty in HadSST3 would be a useful piece of additional information particularly in the period 1979-2011 when the parametric uncertainty is particularly important relative to other uncertainties. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Rejected: unclear how to present this material unambiguously without significant increase in length.
2-1306	2	33	30	33	32	Table 2.6 Caption does not give units of trends [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account (see 2-1300)
2-1307	2	33	30			Specify units (°C/decade?) in Table 2.6 caption [Franco Desiato, Italy]	Taken into account (see 2-1300)
2-1308	2	33	30			There seems to be no obvious reason not to combine tables 2.5 and 2.6 into a single table. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. A significant effort was expended to achieve similarity in tables of temperature section. Therefore, Tables 2.4, 2.6, 2.7, and 2.8 all present state-of-the-art products (of different temperature variables); Table 2.5 is an odd-table-out: it compares a present state-of-the-art data set (HadSST3) with its predecessor used in AR4 (HadSST2).
2-1309	2	33	31	33	31	COBE-SST is never referred to in the text. [Geert Jan van Oldenborgh, Netherlands]	Rejected. The use and presentation of COBE-SST in this chapter is similar to that of ERSST v3b. Please see the response to 2-1243.
2-1310	2	33	33	33	33	Table 2.6: Trends in for the period 1979-2011, particularly the HadSST3 and ERSSTV3b, are apparently stronger as compared to longer term trends for the periods such as 1951-2011. [Government of India]	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1311	2	33	34	33	34	Why "ocean" here and "sea" elsewhere? [Elizabeth Kent, United Kingdom]	Noted. Changed to Sea
2-1312	2	33	34	33	50	"c. 1970 onwards" is seriously misleading with highly publicized little warming for the last 16years, or at least since 2000. The multidecadal oscillation appears to peak about 1940 and 2000 with statistically little warming since 2000. In keeping with the observation on page 33 lines 17-20, for scientific accuracy and credibility, I strongly recommend changing "c. 1970 onwards" to "c. 1970 to c. 2000". [David L. Hagen, United States of America]	Noted. Section has been significantly edited.
2-1313	2	33	34	34	2	Consider including an explanation for observed non-linear warming. It appears to be linked to atmospheric oscillations, such as the PDO which would explain why there was no measurable warming from 1940-1970. [Government of United States of America]	Rejected. Mechanisms are the domain of either other sections or other chapters.
2-1314	2	33	34	34	24	Adding to the three datasets another independent global mean temperature dataset from Japan Meteorological Agency, which is referred to in Chapter 10, serves to further consolidate the consistency, and better understand the uncertainty, associated with the temperature variability and long term trend. [Government of Japan]	Rejected. We contacted the curators of the JMA analysis to ascertain its publication status. As far as we could ascertain there existed no peer reviewed paper in either Japanese or English describing the dataset and its construction. Given our charge to assess the peer-reviewed literature it was deemed not possible to incorporate this analysis.
2-1315	2	33	34			HadCRUT4 has serious errors. The paper about HadCRUT4 (Morice et al, 2010) says that global average temperature anomalies are the mean of the two hemispheric averages but this is not true of the published data. It should be possible to use the gridded data, weighted by the cosine of the latitide of the centre of each grid cell, to calculate hemispheric averages. While this was true of HadCRUT3 it is not true of HadCRUT4. I suspect that you are citing HadCRUT4 without first properly auditing the data. Your faith in peer review is touching but surely you don't think that peer review completely replicated the data processing? I have examined the HadCRUT4 data and it is flawed. [John McLean, Australia]	Rejected. The rationale behind the processing choice in HadCRUT4 is thoroughly documented in Morice et al., 2012. It is not an unreasonable approach for accounting for the incomplete and changing data availability through time. All three groups take a different approach. That they agree / differ tells us something meaningful about the uncertainty that arises through the choice of averaging procedure.
2-1316	2	33	36	33	36	AR4 concluded that " the global average surface temperature had increased, especially since 1950". Do you mean 1979 ? The current table 2.7 clearly indicates that the trends before and 1950 are comparable, especially for HADCRUT4. [Ileana Bladé, Spain]	Noted. We have expliciitly replicated now language from the ES of Trenberth et al.
2-1317	2	33	37	33	37	I think the word "temperature" is missing after "Subsequent developments in land and SST". [Ileana Bladé, Spain]	Noted. Land changed to LSAT
2-1318	2	33	41	33	42	Can you link up the data-product names with the references more explicitly to help those less familiar with the global temperature records? [Kate Willett, United Kingdom]	Accepted
2-1319	2	33	44	33	44	Precede sentence with something like "The conclusion continues to be that ..." [Ileana Bladé, Spain]	Noted. Sentence has been deleted.
2-1320	2	33	44	33	45	why has the warming not bee linear? Why are there two warming periods? Explaination missing and needed [Andreas Walter, Germany]	Rejected. We are simply describing the timeseries nature here. The physical interpretation of the causes is the domain of Chapter 10.
2-1321	2	33	44	33	50	This is a most welcome description of the temperature changes that is missing on the previous sections on LSAT and SST. I realize that doing the same for both LSAT and SST may seem very repetitious but it still seems bizarre for those sections not to include any description of the figures/tables. [Ileana Bladé, Spain]	Noted. Due to space restrictions we cannot expand to discuss these, and would prefer to discuss here primarily.
2-1322	2	33	44			The reference to Simmons et al. (2010) is rather misleading here. Simmons et al. did not show CRUTEM4 samples higher latitudes better than CRUTEM3. Rather, these authors showed ERA-Interim exhibited more warming over land than CRUTEM3, arguing that this was because it sampled high-latitude northern hemispheric areas better than CRUTEM3, and warming in these areas was larger than average. Jones et al. (2012a or 2012b; this reference is duplicated on page 2-86 lines 38-43) showed that CRUTEM4 sampled higher latitudes better, and gave results in better agreement with ERA-Interim than CRUTEM3 did. This is quite an important point, as it shows the reanalysis approach leading the more classical CRUTEM approach in this particular aspect. So I think it is worth spelling out as indicated above. [Adrian Simmons, United Kingdom]	Accepted. Text has now been changed and expanded to make this clearer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1323	2	33	45	33	45	change 'c.1900 to c.1940 and c.1970 onwards' to 'from around 1900 to around 1940 and from around 1970 onwards' [Government of Australia]	Editorial.
2-1324	2	33	45	33	45	This sentence is incorrect. Warming did not commence in 1970. It commenced in early 1977, which incidentally was 7 months after the Pacific Climate shift, which saw an ENSO shift to the El Nino side of absolutely neutral (SOI = 0) where it had previously been slightly on the La Nina side. (Refer - Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures") IPCC AR4 said that warming commenced in 1977, after the 1976-1977 Climate Shift, so why isn't that mentioned here? Further, warming has NOT continued "since the 1970s"; chapter 10 says correctly that there's been no statistically significant warming since 1997. [John McLean, Australia]	Rejected. There is so much inter-annual variability that pinning resumption to specific years is not warranted. The more general nature of these statements as made better reflects our understanding here.
2-1325	2	33	45	33	49	Reference to and explanation of the SPM statement 'Each of the last three decades has been significantly warmer than all the preceding decades since 1850' should fit here. If it is in the SPM it must be explained somewhere in the chapter text and chapter executive summary with reference to the published papers supporting this statement. It is a useful statement for policy makers. [Government of Australia]	Noted. This is what the text here says, but the 1850s context was missing and ahs been added. No further changes have been made as this should now increase substantively the provenance of this key finding back to this figure and discussion.
2-1326	2	33	46	33	46	In what context are the decades "significantly warmer". Is this statistical? [Government of Australia]	Noted. It is significant relative to a broad range of quantified sources of uncertainty as stated explicitly in the text. No changes made.
2-1327	2	33	47	33	47	Confusing grammar. Change to “in HadCRUT4, a dataset that explicitly quantifies ...” [Ileana Bladé, Spain]	Editorial.
2-1328	2	33	49	33	49	Are the "top ten" years the 10 warmest? [Dian Seidel, United States of America]	Noted. Edits to simplify and clarify have been made here.
2-1329	2	33	50	34	2	Can this quantity be shown for the other data products too to get an idea of the structural uncertainty on this important number? Is there a discussion of what is 'pre-industrial' anywhere? This relates strongly to policies trying to keep warming below 2 degrees and so monitoring this trajectory and being clear about whether it is from pre-industrial or early industrial is important. [European Union]	Noted. These values are shown in the appendix which is now explicitly referenced here. Data availability limits our ability to consider such quantification from an earlier start date as should be obvious from the discussion in the preceding sections.
2-1330	2	33				Table 2.6: Units should be added to either the caption or the table [Jeffrey Taylor, United States of America]	Noted. Taken into account with comments on first section table.
2-1331	2	34	1	34	2	The modelling chapters seem to have settled on 0.60 K for the difference early-industrial (estimated by 1850-1900) to 1986-2005 because 1886-1905 could be biased low due to the eruptions of the Krakatau, Santa Maria and other volcanoes in this period. Is it possible to quote this interval and error margin here? [Geert Jan van Oldenborgh, Netherlands]	Accepted. We have worked with later chapters to create the required estimates and placed these in our appendix.
2-1332	2	34	12	34	12	This interest has spread to HadCRUT4 which shows the slowdown, if a little less extreme, and 2012 data will confirm this. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1333
2-1333	2	34	12	34	12	You are using HadCRUT4, so don't refer to HadCRUT3. This apparent levelling off wasn't unique to HadCRUT3. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have simplified the text here by not calling out HadCRUT3 in this manner. This has also led to some additional edits further down within this paragraph for narrative consistency.
2-1334	2	34	12	34	12	"Flattening" is seen in all the global series in the sense that the n-year trends (for n between 5 and 16 at least) are lower at the end of the series than they are if they end say 5 years before the end of the series. The problem was most obvious in HadCRUT3 so that's the one that got picked on, but it affects them all. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1333
2-1335	2	34	12	34	12	HadCRUT3 is mentioned for the first time here I think and so should have a reference (Brohan et al. 2006) and or say (as previously used in AR4) perhaps? [Kate Willett, United Kingdom]	Taken into account. See response to 2-1333
2-1336	2	34	12	34	16	In this context, and at the risk of sounding self-serving, you may want to mention the Liebmann et al. (2010)	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						paper, which examines the sensitivity of global temperature trends to the choice of time period. It clearly shows, using three different methods to correct for autocorrelation. that changes shorter than a few decades (which can be either positive or negative) are not statistically significant and should not be interpreted as indicative of long-term trends. The paper also shows that the positive trends of long duration (several decades and longer) ending in recent years are extremely unlikely to have occurred by chance. [Liebmann, B., Dole, R. M., Jones, C., Bladé, I., and Allured, D. (2010). Influence of Choice of Time Period on Global Surface Temperature Trend Estimates. Bulletin of the American Meteorological Society, 91(11), 1485–1491. DOI:10.1175/2010BAMS3030.1] [Ileana Bladé, Spain]	
2-1337	2	34	14	34	15	The claim here about similar phases is badly misleading, since the similar phases described eg by Easterling and Wehner include the 1982 El Chichon eruption and the cooling associated with that. So this argument is spurious. [Paul Matthews, United Kingdom]	Rejected. The papers cited take a range of approaches and consider a range of epochs and all conclude that for shorter time-periods autocorrelated series noise can dominate multi-decadal trends as the text states. This does not rest on Easterling and Wehner and nor is Easterling and Wehner dependent upon a period including 1982.
2-1338	2	34	15	34	15	I believe that the reference "Grant Foster and Stefan, 2011" should be "Foster and Rahmstorf, 2011." [Nathaniel Johnson, United States of America]	Taken into account. See response to 2-1339
2-1339	2	34	15	34	15	Reference should be Foster and Rahmstorf (2011) unless we're suddenly going to first names! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Editorial. This is endnote doing something strange and will be fixed.
2-1340	2	34	15	34	15	"Grant Foster and Stefan" should be "Foster and Rahmstorf". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1339
2-1341	2	34	15			Grant Foster and Stefan should presumably be "Foster and Rahmstorf". [Government of United States of America]	Taken into account. See response to 2-1339
2-1342	2	34	16	34	20	In the light of recent media attention its worth being explicit about the years over which these trends are calculated 1998-2011? May be worth drawing out some of the other periods of 'no apparent warming' although I guess the point is that it is in this most recent period of accelarated warming that such a persistent period of no further warming is a little surprising, albeit not incongruous to the larger picture of anthropogenically forced warming. Is there a place for a discussion of choice of start-end dates here - possibly not? [Kate Willett, United Kingdom]	Noted. For avoidance of doubt the timeframe will be made explicit. The reviewer is correct that there is not space or remit to consider additional periods / trend lengths but these are covered in detail amongst the cited references. There is also now a new Box (9.2) that considers this hiatus behavior in more detail.
2-1343	2	34	17	34	17	Add under the climate model simulations sentence that one of the earliest of these papers which comes to this conclusion is: Knight, J., J. J. Kennedy, C. Folland, G. Harris, G. S. Jones, M. Palmer, D. Parker, A. Scaife, and P. Stott, 2009: Do global temperature trends over the last decade falsify climate predictions? In: State of the Climate in 2008. T.C. Peterson, and M. O. Baringer, Eds., Bull. Amer. Met Soc., 90, S22-S23 [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Noted and this is the Peterson et al. cite. Endnoteweb makes it impossible to cite anything more than the report as a whole.
2-1344	2	34	17	34	20	The statement "none of these are statistically significant" is very confusing. Are the authors saying that in spite of warming trends being observed from 1998 in the three global combined land and ocean surface temperature data sets (HadCRUT4, MLOST and GISS) after the addition of high latitude Northern Hemisphere land temperatures, these warming trends are not statistically significant? The sentence should be reworded so that the meaning is very clear and cannot be misinterpreted, particularly since Section 2.4.5 states that "It is virtually certain that global near surface temperatures have increased. Globally averaged near-surface combined land and ocean temperatures, according to several independent analyses, are consistent in exhibiting warming since 1901, much of which has occurred since 1979." The rest of Section 2.4.3 is well written and logical. [Government of Australia]	Noted. We have added explicitly a caveat about the short time period to avoid such conflation here.
2-1345	2	34	17			"primarily as a result of incorporation of more high-latitude NH land data"- this wording suggests that the results can be manipulated by careful selection of the data to include in the analysis. Consider rewording. [Government of United States of America]	Noted. Edits in response to comments elsewhere have led to deletion of this text.
2-1346	2	34	18	34	19	In view of the above results, where it states all products now show a warming trend since 1998 it could be clarifed that "all products" includes ERA-Interim as well as the HadCRUT4, MLOST and GISS products that	Rejected. While the suggestion is absolutely correct that several reanalyses products also exhibit warming

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						are referred to explicitly. [Adrian Simmons, United Kingdom]	this is deemed likely to confuse rather than aid interpretation here by bringing in datasets that are much shorter than the centennial timescale records constituting the section focus. This would have to be supported by a new sentence with references that may serve to distract the reader.
2-1347	2	34	19	34	19	Style change (per vs /) [Peter Burt, United Kingdom]	Editorial.
2-1348	2	34	19	34	20	These minute trends, given to an accuracy of 0.001 degrees/decade, are meaningless and should be omitted. [Paul Matthews, United Kingdom]	Rejected. These are consistent with trends reported elsewhere.
2-1349	2	34	19	34	20	"none of these are staitically significant" should read "none of these are staitically significantly different from zero or longer-term trends" [Geert Jan van Oldenborgh, Netherlands]	Noted. The null was only that they were different from zero- this has been clarified. As we did not test for difference from the long-term trend we should not state so.
2-1350	2	34	20	34	20	Cross ref to Chapter 10, subsection:The evolution of global temperature since 1998. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Noted. Comment has been superceded by Box 9.2
2-1351	2	34	20	34	20	Ambiguous. Do you mean 1998-2011 or 1999-2011? Adding 2012 may slightly reduce these trends. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1342
2-1352	2	34	20	34	20	Need to make clear that it is unlikely that you will have statistically significant warming over such a short period of time given the interannual standard deviation (~0.2 °C) of global temperature and the background rate of global temperature rise (~0.015°C/yr). In this context, it is quite meaningless to seek significance for the trend. It is also unclear why one would seek to define a trend starting in 1998 from a published science point of view. [Government of Australia]	Noted. This has been alleviated by edits suggested elsewhere. The rationale behind such a calculation is given within the paragraph. Comments elsewhere demanded doing this for every variable etc. which we do not do for the scientific reasons stated here.
2-1353	2	34	30	34	32	It is a bit surprising that only one concise sentence serves to describe the left panel of Figure 2.22. Perhaps some continental-scale features could be highlighted. For instance, all datasets suggest that the long-term warming has been somewhat more pronounced over Asia than North America. Also, while the enhanced warming in the NH high latitudes is evident, the statement that “warming is generally greater over land than oceans” seems less justified (and also less expected at these long time scales) or at least it should be somewhat nuanced. For starters, there is so little coverage over land in the SH in HadCRU that the statement does not even apply. For GISS data, the trends seem to be comparable over land and ocean poleward of about 45ºN except in Asia. [Ileana Bladé, Spain]	Rejected. Space limitations mean such commentary cannot be added.
2-1354	2	34	30	34	33	The greater warming in mid- to high-latitudes is not obvious in the left-hand panels. The greater warming at higher latitudes in the Northern Hemisphere (only) is obvious in the right-hand panels. Consider changing the text to emphasise this, e.g. This warming is generally greater over land than oceans and greater in mid- to high-latitude regions, though is not evident in the figure. Over the satellite era most of the globe again experienced warming (Figure 2.22, right hand panels). Over this shorter period a greater proportion of the globe exhibits cooling, in particular over the oceans, while the higher latitudes of the Northern Hemisphere (only) have clearly experienced greater warming than the global mean. [James Renwick, New Zealand]	Noted. To meet length limits this sentence has been deleted.
2-1355	2	34	30	34	36	Ocean cooling is restricted to the Pacific Ocean and Southern Ocean where strong coastal and oceanic upwellings exist. Surface ocean cooling might thus be linked to changes in upwelling conditions and/or changes in upwelled deep water properties or deep water masses. Changes in upwelled deep water masses may be a consequence of upwelling modification. Oxygen minimum zones have been shown to thicken over the last 50 years (Stramma et al., 2008, Science, 320, 655-658), probably because of less O2 diffusion (increase SST) and O2 over-consumption (increasing phytodetritus decomposition at depth). [Government of France]	Noted. Comment is out of Chapter scope and should be directed to Chapter 3.
2-1356	2	34	30	34	36	Fig. 2.22: Data generated by MLOST and GISS over South America and Africa are not reliable for the period 1901-2011, since these continents have almost no data before 1950 and the interpolation is almost baseless. [Alice Grimm, Brazil]	Noted. The point of using all three datasets is to highlight what is robust and what is not between the different datasets.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1357	2	34	32	34	32	The word "again" should probably be changed to "also" because the satellite era falls within the period since 1901. [Dian Seidel, United States of America]	Editorial
2-1358	2	34	33	34	33	Greater than what? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Subsequent changes to the figures make this moot.
2-1359	2	34	33			Section 2.4.3: I think that a clear statement relating to the apparent lack of warming since around 2000 should be made. Clearly, this is too short a period to calcuate physically insightful trends yet it is a noticable feature of the graphs of global temperature change. I suggest mentioning, "A reduced rate of warming since around 2000, compared to the previous decades, is a noticable feature of Fig. 2.21. While the global SST have been static over this period (Fig. 2.18) heat has continued to accumulate in the sub-surface ocean (Lyman et al. 2010; Loeb et al. 2012) indicative of natural variability in heat distribution by the oceans." [Richard Allan, United Kingdom]	Rejected. There is now a box that covers these issues in Chapter 9 instead
2-1360	2	34	39	34	43	I must have missed it. How are global averages calculated, i.e. average of available grids (not very good), or zonals created first from grids, then zonals used for global average (better represents a global estimate.) [John Christy, United States of America]	Noted. We use the series derived by the dataset providers for such means. But comment is referring to maps which have no such averaging so it is unclear how comment relates to content or what changes are requested as clearly cannot be changes here.
2-1361	2	34	39	34	48	Worth noting that these error ranges are much than for land and much more like the SST ranges. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Comment appears a fragment. Reviewer may be suggesting noting that ranges are much smaller than for land. But without more context it seems unwise to make any changes.
2-1362	2	34	39			The figure caption refers to "the three global datasets for 1901-2011", whereas on page 2-28, lines 19-31, there is reference to a fourth product, from the Berkely group. So is it appropriate to the refer to the three datasets? Moreover, in the opening figures of Chapter 10, results are presented from a JMA product as well as "the three global datasets for 1901-2011". If JMA results are included in Chapter 10, they should be discussed in Chapter 2. [Adrian Simmons, United Kingdom]	Noted. The Berkeley product is land only so not truly global in nature so not relevant here. See response to 2-1314 for discussion of JMA analysis.
2-1363	2	34	40	34	40	70% seems rather restrictive, likely creating larger gaps over the Pacific in HadCRUT4 than needed. Could this be reduced to say 40%, keeping the other restriction? [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected. 70% was a value that collectively the author team felt cofortable with after significant discussion in the meeting in Marrakech and we see no compelling rationale to revisit it here.
2-1364	2	34	42	34	43	The HadCRUT4 dataset is interpolated on thev grid box scale (5x5), so the statement that it has no interpolation is wrong. [Geert Jan van Oldenborgh, Netherlands]	Accepted. Edits to clarify have been made.
2-1365	2	34	42	34	43	A more accurate way to formulte this is "Differences reflect to a large extent the assumptions made for areas with no observations. HadCRUT4 assumes that these have the same temperature anomaly as the global mean over all areas with observations, whereas the GISS dataset extrapolates from stations surrounding the data void. NCDC uses an intermediate procedure." [Geert Jan van Oldenborgh, Netherlands]	Taken into account. See response to 2-1364
2-1366	2	34	46	34	46	Caption of Table 2.7: Add units of described trends. [Birgit Hassler, United States of America]	Taken into account with edits to earlier tables which this cross-references.
2-1367	2	34	46	34	46	Chapter 1 said that period 1998 to 2011 would be shown, so please include it in this table. [John McLean, Australia]	Rejected. The values are shown in the text.
2-1368	2	34	46	34	46	Units? Presumably degrees/decade. [James Renwick, New Zealand]	Taken into account. See response to 2-1366
2-1369	2	34	46			Specify units (°C/decade?) in Table 2.7 caption [Franco Desiato, Italy]	Taken into account. See response to 2-1366
2-1370	2	34	49	38	22	The whole of the debate about the problems of reconciling radiosonde data with GCM models is missing. Figure 10.7 in WG1 of AR4 showed a predicted heating of about 0.6 deg C per decade between 400 and 100hPa and -30 deg S to 30deg N. However, none of the data from satellites or radiosondes confirms anything like that rate of heating. Allen, Robert J. and Sherwood, Steven C. (2008) Warming maximum in the tropical upper troposphere deduced from thermal winds. Nature Geosci 1 (6), 399- 403,	Rejected. GCM comparisons and their implication are discussed in detail in Chapters 9 and 10 and are out of scope for this Chapter. No changes made.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						http://dx.doi.org/10.1038/ngeo208 note "Climate models and theoretical expectations have predicted that the upper troposphere should be warming faster than the surface. Surprisingly, direct temperature observations from radiosonde and satellite data have often not shown this expected trend," for instance - and then go on to suggest biases in the data. Lanzante, John R., Melissa Free, 2008: Comparison of Radiosonde and GCM Vertical Temperature Trend Profiles: Effects of Dataset Choice and Data Homogenization. J. Climate, 21, 5417–5435. doi: http://dx.doi.org/10.1175/2008JCLI2287.1 (Not quoted in SOD) note that even after homogenization "in general the observed trend profiles were more similar to one another than either was to the GCM profiles." Singer, S Fred, (2011). Lack of Consistency Between Modeled and Observed Temperature Trends Energy & Environment, 22, 375-406 DOI - 10.1260/0958-305X.22.4.375 (Not quoted in SOD) drew attention to the fact that "The US Climate Change Science Program [CCSP, 2006] reported, and Douglass et al. [2007] and NIPCC [2008] confirmed, a "potentially serious inconsistency" between modeled and observed trends in tropical surface and tropospheric temperatures." and noted further that "Santer's key graph --- misleadingly suggests an overlap between observations and modeled trends. His "new observational estimates" conflict with satellite data. His modeled trends are an artifact, merely reflecting chaotic and structural model uncertainties that had been overlooked. Thus the conclusion of "consistency" is not supportable and accordingly does not validate model-derived projections of dangerous anthropogenic global warming." Douglass, D. H., Christy, J. R., Pearson, B. D. and Singer, S. F. (2008), A comparison of tropical temperature trends with model predictions. Int. J. Climatol., 28: 1693–1701. doi: 10.1002/joc.1651 (Not quoted in SOD) note "Model results and observed temperature trends are in disagreement in most of the tropical troposphere, being separated by more than twice the uncertainty of the model mean." Titchner, Holly A., P. W. Thorne, M. P. McCarthy, S. F. B. Tett, L. Haimberger, D. E. Parker, 2009: Critically Reassessing Tropospheric Temperature Trends from Radiosondes Using Realistic Validation Experiments. J. Climate, 22, 465–485. doi: http://dx.doi.org/10.1175/2008JCLI2419.1 (Not quoted in SOD) note that "tropical tropospheric trends in the unadjusted daytime radiosonde observations, and in many current upper-air datasets, are biased cold, but the degree of this bias cannot be robustly quantified." It was my clear understanding that the Assessment Report was to review the current literature - there are some notable omissions, as I have noted above. Moreover, I had understood that the Report was to give due weight to differences of opinion - instead, any reading of the present text leaves the clear impression that the debate is being studiously avoided rather than being addressed. And one thing my own review of the debate has brought home to me is that even the satellite data does not come near the predictions that were made in AR4 - the discrepancy between ALL data and the models is wide. This debate MUST be reflected in the text. [Philip Lloyd, South Africa]	
2-1371	2	34	49	38	22	This section, 2.4.4, is problematic for two overall reasons, discussed immediately below in separate comments. Please also see comment 283 concerning a sentence on upper-tropospheric temperature included in Chapter 10. [Adrian Simmons, United Kingdom]	Noted. We do not have sufficiuent reference to ascertain the Chapter 10 reference given the way the comments are repackaged and partitioned by TSU.
2-1372	2	34	49	38	22	The absence of results from at least the ERA-Interim reanalysis, without any discussion as to why, is inexcusable. These results should be shown alongside those of the radiosonde and MSU datasets. I do not advocate showing results from a reanalysis just because there has been a peer reviewed publication on that reanalysis in general. But where there have been peer-reviewed publications that include the trends from a reanalysis, and those trends have not been shown to be evidently wrong, but rather "in the pack" of radiosonde and MSU analyses, then I fail to see why the results from that reanalysis should not be shown. Not showing them is contrary to the basic philosophy esposed on page 2-7, lines 38 and 39. [Adrian Simmons, United Kingdom]	Noted. The use of reanalysis was discussed in this context in both Marrakech and again in Hobart. As the reviewer is likely aware the ERA-Interim results were included in a figure in the FOD and removed as a result of reviewer comments on FOD (these comments will be made public along with responses) and some 'lively' within chapter discussions. This was replaced with a 2-panel set of maps for UAH and RSS that show pretty much equivalent things. Following the reviewer's suggestion that published comparisons exist, we have looked for examples since AR4 (which is our remit), beyond state of the climate BAMS articles, where ERA-Interim tropospheric and stratospheric behavior is clearly documented and discussed and compared to existing estimates (including but not limited to Dee et al., 2011 and Haimberger et al., 2012). We could not find any

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							substantive intercomparisons and therefore do not feel able to add citations. We would strongly encourage such analyses which could then be cited within future assessments both by IPCC and others. It is clear that reanalyses bring useful insights and the non-inclusion of a discussion of ERA-Interim or any other reanalysis should not be taken to imply otherwise. It is simply that appropriate literature that adds to existing content is not available to assess at this time.
2-1373	2	34	49	38	22	More specifically, the heritage paper indicating useful upper-air temperature variability/trend information from ERA is that of Santer et al (2004) based on ERA-40, and discussed in AR4. ERA-40 (and NRA) results were included in Fig. 3.18 of the AR4 WG1 report. ERA-Interim has improved on ERA-40. Its bias adjustment schemes have been discussed in the peer-reviewed literature by Dee & Uppala (2009 - Quart. J. R. Meteorol. Soc., 135, 1830-1841) and trends have been compared with other estimates in Dee et al. (2011 - reference given in an earlier comment) and Haimberger et al. (2012 - reference cited in Chapter 2). So why suppress ERA-Interim data in section 2.4.4 without any discussion? ERA-Interim trends are not fully independent of the others - ERA-Interim assimilates the RAOBCORE version of the radiosonde data, but it also assimilates MSU and much more satellite data (see next comment) so it represents a reconciliation of a wide variety of data. Of course there are some issues (known ones are acknowledged in the literature) related to the near-tropopause region in particular due to biases in temperature from aircraft (corrected now in operational forecasting and for future reanalyses) and from introducing GPS RO data. But there are also acknowledged problems in the radiosonde and MSU datasets for which results are included, as discussed in section 2.4.4. [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1372
2-1374	2	34	49	38	22	Section 2.4.4 is also weak in its discusssion of the available satellite data. Aside from the specific comment on the SSU data given below, it should be pointed out in this section that AMSU-A is the replacement not only for MSU but also for SSU (and to be up-to-date one should also now mention the ATMS microwave instrument on the latest operational US polar orbiter, NPP, data from which are already bias-corrected and assimilated for operational forecasting at ECMWF). In addition to the microwave instruments, there are also the HIRS infrared instruments that are the only ones that span the full period from 1979 to the present day, and the newer high-resolution infrared sounders (AIRS, IASI, CrIS) which are proving to be highly stable, and which now, in the case of AIRS, have provided data for more than ten years, spanning troposphere and stratosphere. VTPR instruments provided infrared temperature (and humidity) data from 1973 to 1978, and these data have been assimilated in ERA-40 and (I believe) JRA-55. These types of infrared data are more difficult to use than microwave data in the presence of cloud, but this issue has been solved for operational weather forecasting, and hence reanalysis, and in any case is much less of a problem for the stratospheric sounding channels. [Adrian Simmons, United Kingdom]	Noted. Given space limitations much of this cannot be covered. Some is covered within the appendix, some other is covered by edits to specific suggestions elsewhere within the text and the remainder is discussing records too short to be in-scope as the Chapter charge was given to us. This does not obviate its potential utility down the road but a clear balance needs to be struck in what is included given the very tight space restrictions stipulated.
2-1375	2	34	49			In Section 2.4.4. discussions of the middle and upper stratosphere, a new paper by Thompson et al (2012) should be cited, and the main conclusions can be included in the section. Thompson, DWJ, DJ Seidel, WJ Randel, C-Z Zou, A Butler, C Mears, A Osso, C Long and R Lin, The mystery of recent stratospheric temperature trends, Nature, in press. [Dian Seidel, United States of America]	Accepted. This paper has been added.
2-1376	2	34	51	35	2	"… indistinguishable from reported surface trends …" should add the following to be accurate and honest, "… i.e. that there was no tropospheric amplification of the trend in the observations." Neglecting to add such a statement could provide an easy opening to criticism in the future evaluations of the AR5. [John Christy, United States of America]	Rejected. This statement is already directly implicit within the current statement. Given space constraints we cannot add such additional details here and elsewhere. The question of model-observation consistency is covered in Chapter 9 and detection and attribution in Chapter 10.
2-1377	2	34	51	35	2	The leading sentences for section 2.4.4 do not make a logical progression. Is it possible that some text was inadvertently omitted here? [Government of United States of America]	Accepted. We have added some text here to improve the flow.
2-1378	2	34	51	35	2	Can you think of another verb than 'concluded'? [Phil Jones, United Kingdom of Great Britain & Northern	Editorial.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ireland]	
2-1379	2	34				Table 2.7: what do the numbers mean in the table? Provide units e.g. [K/decade] [Alexander Loew, Germany]	Taken into account with edits to earlier tables which this cross-references.
2-1380	2	34				Table 2.7: Units should be added to either the caption or the table [Jeffrey Taylor, United States of America]	Taken into account. See response to 2-1379
2-1381	2	35	2	35	2	Suggest adding "and has lead to some revisions and improvements in associated products" at end of sentence to emphasise that this new knowledge has supported the development of improved datasets. [Government of Australia]	Accepted.
2-1382	2	35	2	35	2	"weather balloon records" should be changed to "balloon-borne radiosonde records." since balloons themselves measure nothing. [Dale Hurst, United States of America]	Accepted.
2-1383	2	35	6	36	27	I have an uneasy feeling in this section. I see "Mears submitted" a few times. This appears problematic from this vantage point - a lead author, fully aware of the inside goings-on of the text, is able to submit a paper (or 2?) under the deadline before outsiders (such as this reviewer) can have a chance to do due diligence. Special care is needed not to over-promote a paper that will be allowed to have the "last" word in a document such as the IPCC. (This is not to say Mears-submitted might be inaccurate.) On the other hand, the IPCC process is problematic due to this basic fact - it is out of date as I type this because papers submitted after last July will be excluded. It's an unsolvable dilemma when the product is static rather than dynamic (i.e. web-based.) At times it probably feels like a fools errand. [John Christy, United States of America]	Noted. As the reviewer notes this is an inevitable issue in any assessment process. The reviewer had access, should they have requested, to a version of the paper lodged with TSU as per guidance. The section conclusion does not rest on the Mears et al. analysis and cites multiple papers including many lead or co-authored by the reviewer to reach its bottom line conclusion. The Mears et al analysis was instigated by RSS and in no sense was solicited or invited by IPCC. It was funded under US government grants.
2-1384	2	35	7	35	9	By how far does the AMSU record referenced to here corresponds to a Fundamental Climate Data Record (FCDR)? Trend calculations from longterm satellite records are not reliable until an appropriate processing of the data has been made. To my knowledge no AMSU FCDR is existing so far. What is the reference for the statment made here? [Alexander Loew, Germany]	Noted. As is clearly stated in the section text AMSU-A data have been used in at least 3 peer-reviewed datasets of MSU / AMSU data.
2-1385	2	35	7			add the word "mandatory" to line 7 so it reads, "...reported at mandatory pressure levels." [Government of United States of America]	Editorial.
2-1386	2	35	14	35	14	These two references ought to be the other way around. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Editorial.
2-1387	2	35	15	35	15	Should reference be "Seidel et al., 2011" instead of "Seidel, 2011"? [Birgit Hassler, United States of America]	Editorial.
2-1388	2	35	24	35	24	Insert "global" between "only two" and 'radiosonde data sets' [Government of Australia]	Editorial.
2-1389	2	35	26	35	26	Please give supporting reference(s) [Peter Burt, United Kingdom]	Rejected. It is unclear what the reviewer is requesting supporting references for as this sentence already contains supporting references.
2-1390	2	35	27	35	29	Might be worth expanding this sentence to be clear that ERA reanalysis products were used for homogenisation. [Kate Willett, United Kingdom]	Noted. This detail is provided in the Appendix.
2-1391	2	35	30			"...non-climatic data issues remained in the deep tropics..." Consider describing these issues. [Government of United States of America]	Accepted. Clarified.
2-1392	2	35	35	35	35	exhibited → exhibit [Ileana Bladé, Spain]	Editorial.
2-1393	2	35	38	35	38	"and even here is" - where is "here" ? [Dale Hurst, United States of America]	Noted. Changes made for clarity here.
2-1394	2	35	39	35	39	What does the 0.1 °C per decade refer to? Is it trend, decadal mean, local regional averages etc.? Please clarify. [Government of Australia]	Taken into account. See response to 2-1393
2-1395	2	35	41	35	52	This paragraph gives more detail than is necessary [Melissa Free, United States of America]	Noted. Detail is commensurate with that given elsewhere for other products. Without specific comments it is hard to enact any edits in response to

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							this comment.
2-1396	2	35	42	35	42	Wrong use of verb (deprecated means "to strongly disapprove of (something)"). I think you mean discredited (or similar) [Peter Burt, United Kingdom]	Taken into account. See response to 2-1397
2-1397	2	35	42	35	42	The word "deprecated" does not seem warranted in the AR5. How about "no longer used". [Dian Seidel, United States of America]	Accepted
2-1398	2	35	42			Sorry to miss this one earlier - I am unable to find a way of inserting a new line in your spreadsheet. It is reported that AR4 considered estimates from three groups, but that the estimates of VG2 are "now deprecated". This is rather unsatisfactory. Can a reason be given for the deprecation, and an indication given of whether any AR4 conclusions were misleading because of problems with VG2's work? Is there a peer-reviewed reference? [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1397
2-1399	2	35	45	35	45	"(Zou and Wang, 2010)" should be "Zou and Wang (2010)" - remove parentheses [Dale Hurst, United States of America]	Editorial.
2-1400	2	35	45	35	45	MT not explained [Andreas Walter, Germany]	Noted. MT was defined in the opening to section 2.4.4
2-1401	2	35	46	35	46	use of the NEW inter-satellite [Ileana Bladé, Spain]	Editorial.
2-1402	2	35	51	35	51	where it is → where they are [Ileana Bladé, Spain]	Editorial.
2-1403	2	35	54	36	4	Reference to the work of Kobayashi et al.(2009) should be made in this paragraph, not in the supplementary material. Firstly, Kobayashi did far more than show that SSU cell pressures varied from instrument to instrument, as implied in the supplementary material. Rather, he addressed the problem of drift of SSU data from individual instruments due to leakage of gas from the cell. This work is seminal in that it forms the basis not only for the assimilation of SSU data in the latest generation of reanalyses, but also for the reprocessing of the SSU data by Wang et al.(2012c), whose work is (appropriately) discussed in this paragraph. [Adrian Simmons, United Kingdom]	Noted. Having re-read Kobayashi et al. and Wang et al. this detail seems more appropriate retained in the appendix given space limitations. This paragraph has been substantively rewritten. This rewrite has taken into account this comment in trying to make clearer the lineage and dependencies without getting overly technical..
2-1404	2	35	54	36	4	Kobayashi et al.(2009) also discuss the transition from assimilation of SSU data to assimilation of AMSU-A data, which should be acknowledged in the supplementary material, if not in this paragraph. Again, reanalysis (not perfectly, but interpretably) provides a coherent picture of mid-to-upper stratospheric cooling spanning both the SSU and AMSU-A periods that continues despite the absence of cooling in the lower stratosphere since the mid-1990s. This work is, however, not yet submitted for publication, so under IPCC procedures it regrettably seems appropriate for now to assign "low confidence in the details above the lower stratosphere" as stated in the final sentence of the paragraph. [Adrian Simmons, United Kingdom]	Accepted. We have added a sentence to the main text to reflect that AMSU continues the record and that initial efforts to understand the effects of the different measurement properties have been made citing Kobayashi et al.
2-1405	2	36	4	36	4	As suggested above, here add reference to Thompson et al. (2012) [Dian Seidel, United States of America]	Accepted
2-1406	2	36	6			Section 2.4.4.2 seems odd and reads less smoothly than 2.4.4.4. It could be removed without loss of critical information [Dian Seidel, United States of America]	Rejected. Section 2.4.4.2 is reflecting a broad range of literature since AR4 and is retained.
2-1407	2	36	7		4	and page prior, also note, in Wang paper, there are differences in the latitudinal structure of trends between Nash and the NOAA data sets. [Karen Rosenlof, United States of America]	Accepted
2-1408	2	36	8	36	8	Change 'have' to 'has' [Peter Burt, United Kingdom]	Editorial.
2-1409	2	36	8	36	27	The section 2,4,4,2 is too long and a bit confusing and should be shortened [Government of France]	Rejected. Detail is commensurate with that given elsewhere for other similar issues and reflects a large body of literature which needed to be assessed. Without specific comments it is hard to enact any edits in response to this comment.
2-1410	2	36	14	36	31	This is a poor assessment because some of the studies are known to wrong, and we know there are major biases in radiosondes owing to changing sensors over time. It should not result in "low confidence" because some studies should be discounted. [Kevin Trenberth, United States of America]	Noted. We have changed the bottom line conclusion of this section in response to this and other comments and to be consistent with style changes elsewhere.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1411	2	36	16			Change "reanalyses" to "reanalysis". [Adrian Simmons, United Kingdom]	Editorial.
2-1412	2	36	18	36	18	. Mears et al. (Submitted) –> They also [Ileana Bladé, Spain]	Editorial.
2-1413	2	36	20	36	21	Though the Po-Chedley vs. Christy issue is down in the weeds, the authors should be aware that I reproduced the Po-Chedley result exactly and demonstrated how they improperly applied their "adjustment." I believe my response has been accepted (I'm away from my office until after the IPCC due date.) It would be acceptible to completely eliminate this contested issue since the following sentences (lines 22ff) summarize the uncertainty issues. [John Christy, United States of America]	Accepted. In keeping with requests elsewhere to shorten / simplify this section this text has been deleted here.
2-1414	2	36	23	36	23	Delete "common" - the residual biases differ between MSU and radiosondes. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected. Instead clarifying text that these are within measurement class commonalities has been inserted.
2-1415	2	36	24	36	26	Were the differences between RSS and others (unexplained by RSS parameteric variation) for the troposphere, stratosphere or both? [John Christy, United States of America]	Noted. Within the space confines given it is not possible to expand on the issue and it is hoped that the reader will access the underlying literature for such detail.
2-1416	2	36	26	30	26	McKitrick et al (2010) found: This statement is made at face value but, later, extensive discussion is made of the differences between satellite and all radiosonde products. I find this confusing. If the McKitrick finding is to be taken with a grain of caution (since it is effectively dismissed later on), could you specify why ? [Ileana Bladé, Spain]	Taken into account. See response to 2-1417
2-1417	2	36	26	36	31	This final sentence is odd and runs counter to the rest of the paragraph. If it's true, which I doubt, then your conclusions in the short final pragraph are wrong. Also there ought to be more confidence in the tropopsheric as opposed to the stratospheric temperature trends. The trends are more consistent for 1958-2011 and for the LT for 1979-2011 than you seem to realise. The warming of the LT and cooling of the LS for 1958-2011 are very highly statistically significant. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Given confusion over this sentence and requests to shorten / simplify this section it has been deleted
2-1418	2	36	29	36	31	Good. A balanced assessment. [Guoyu Ren, China]	Noted. Text has been rewritten in response to comments elsewhere, new text to same effect has been added at the end of Section 2.4.4. in its stead
2-1419	2	36	29	36	31	The time period for this statement is missing. Also, what "details" are of concern -- trend magnitudes, spatial (x,y) pattern, vertical variations? [Dian Seidel, United States of America]	Noted. This text has now been rewritten and moved.
2-1420	2	36	29	36	31	This is a little pessimistic. See comments 41 and 42. It could be argued that there is better than low confidence in key details of the low-frequency variability - the lower stratospheric warming from El Chichon and Pinatubo and the absence of lower stratospheric cooling since the mid-1990s. Sorting out what is trend and what is low frequency variability seems to be the challenge, not ascertaining the value of the slope of a ftted linear trend line. [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1419
2-1421	2	36	30	36	30	all of which are uncertain → perhaps “all of which have large uncertainties” would be better [Ileana Bladé, Spain]	Editorial.
2-1422	2	36	31	36	31	Not clear what 'upper air' means in this context, please give heights [Peter Burt, United Kingdom]	Editorial.
2-1423	2	36	33	37	3	A recent study (Johnson and Xie 2010) provides additional observational evidence that tropical tropospheric warming has been amplified with height. Johnson and Xie (2010) examine the trend in the sea surface temperature (SST) threshold for tropical convection since 1979 and find that there is a statiscally significant upward trend that closely parallels the tropical mean SST. Such a correspondence between tropical mean SST and convective threshold is expected only if there is amplified upper tropospheric warming that accompanies moist adiabatic lapse rate adjustment. Reference: Johnson, N. C., and S.-P. Xie, 2010: Changes in the sea surface temperature threshold for tropical convection. Nature Geoscience, 3, 842-845. [Hiroki Tokinaga, United States of America]	Noted. Out of Chapter scope as not direct observations of the upper air.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1424	2	36	35	37	3	Well done sections on RO and TWE [John Christy, United States of America]	Noted.
2-1425	2	36	35			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]	Noted. Such details have been considered but are too much detail for inclusion in the text.
2-1426	2	36	39	36	40	which is not the directly measured quantity: seems unnecessary [Ileana Bladé, Spain]	Editorial.
2-1427	2	36	42	36	42	to' should be 'of' [Peter Burt, United Kingdom]	Editorial
2-1428	2	36	45	36	46	"between different MSU and radiosonde products trends" - should this be products' ? Can also omit "products" or substitute "temperature" [Dale Hurst, United States of America]	Noted. Text has been clarified here.
2-1429	2	36	48	36	48	"far less obviously" doesn't seem to be a good basis for a scientific analysis. Lots of things are not obvious but are very important. [Dian Seidel, United States of America]	Accepted. Removed the subjective 'obviously' here.
2-1430	2	36	48	36	57	Why is this stuff given so much space and weight? It is really crappy stuff and all of the atmospheric reanalyses, none of which are discussed, have more credibility. Afterall they are multivariate and use winds and temperatures. Moreover they benefit from satellite data. The method dealt with here does not warrant any space. [Kevin Trenberth, United States of America]	Rejected. This is literature apposite to the trends issue that has appeared since AR5 and needs to be assessed accordingly.
2-1431	2	36	56	36	57	what exactly are "systematic wind speed bias sampling effects" - this is jargon [Dale Hurst, United States of America]	Noted. Text has been simplified here.
2-1432	2	37	2	37	2	Write 'thermal winds' as this term has not been used or explained earlier in the paragraph. [James Renwick, New Zealand]	Editorial.
2-1433	2	37	7	37	18	This paragraph would read better if the final sentence were to be moved so it comes after the sentence on lines 11-13, adding a a couple of extra words (overall, however): "Global mean lower stratospheric temperatures have decreased overall since the mid-20th Century, punctuated by short-lived warming events associated with explosive volcanic activity (Figure 2.24, top). However, since the mid-1990s little net change has occured in lower stratospheric temperatures." [Adrian Simmons, United Kingdom]	Accepted. Text has also been shortened.
2-1434	2	37	7	37	19	It should be stated that the uncertainties in Table 2.8 are standard values of statistical error only (i.e. representing the confidence that a straight line through a variable time series is a useful model even if the data are perfect) - they do not account for measurement uncertainty. The latter is what we really need and mentioned on pg 35. Indeed, I don't think statistical uncertainty is very interesting - it is simply a function variance. [John Christy, United States of America]	Noted. This is clear already from the table caption for this suite of tables and a similar caveat applies to all tables. We have wrestled with how to adequately represent uncertainties.
2-1435	2	37	9	37	9	troposphere has warmed: Perhaps repeat “at a comparable rate than surface temperatures” ? [Ileana Bladé, Spain]	Rejected. This seems unnecessary extra verbiage.
2-1436	2	37	9	37	10	"Uncertainty relates to the rate rather than sign of long-term changes, at least at the global mean" - is an awkward way to state that long-term trends in global mean temperatures are statistically different from zero. [Dale Hurst, United States of America]	Accepted. Text has been removed for expediency.
2-1437	2	37	11	37	12	mention the cause of the stratospheric temperature decrease [Rolf Müller, Germany]	Rejected. Causes are the domain of Chapter 10.
2-1438	2	37	11			Is "slightly" the right word to describe the amplification of temperature variations from the surface to the upper troposphere? ERA-40 and ERA-Interim are in quite good agreement for temperature variability in the tropical trosposphere, and they show amplification by a factor of a little over two in going from the surface to 300hPa, which is not so slight (and consistent with the range of values seen in climate models - see Figure 10.7). The ERA-40 result may have been published by someone, though I don't have a reference to hand. For ERA-Interim we have the added bonus of an improved upper tropospheric humidity analysis, which shows good consistency between specific humidity variations at 300hPa and temperature variations at both 300hPa and the surface. This is discussed by Dessler & Davis (2010). Some further discussion will be given in a paper that will soon be prepared on extended ERA-Interim results, but this unfortunately will come too late for AR5.	Noted. Replaced slightly with somewhat. Reviewer is confusing the global discussion here with the tropical issue where the amplification is more substantial (and likely dominates the global mean picture).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Adrian Simmons, United Kingdom]	
2-1439	2	37	13	37	14	"are larger still than for the troposphere" is very awkward [Dale Hurst, United States of America]	Noted. Text has been simplified.
2-1440	2	37	17	37	17	Why make up a verb like 'caveat' when there is a perfectly good one - namely 'caution' [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Editorial.
2-1441	2	37	18	37	19	An explanation of this would be very useful to avoid questions about a possible inconsistency with temperature increases in the troposphere [Guus Velders, Netherlands]	Rejected. Causes of changes are the domain of Chapter 10. Model behaviour is the domain of Chapter 9.
2-1442	2	37	27	37	27	What are the units for the table entries? [Dale Hurst, United States of America]	Taken into account in modifications to the first table caption which this one cross-references.
2-1443	2	37	27	37	29	Table legend not clear, please define LT, MT, LS [Peter Burt, United Kingdom]	Rejected. Table caption cross-references Figure 2.23 where these are clearly defined.
2-1444	2	37	27	37	29	Caption of Table 2.8: Add units of described trends. [Birgit Hassler, United States of America]	Taken into account. See response to 2-1442
2-1445	2	37	27	37	29	Units? Presumably degrees/decade. [James Renwick, New Zealand]	Taken into account. See response to 2-1442
2-1446	2	37	27	37	30	I calculate different trends than shown here for the radiosonde datasets. How was the global trend determined? Are these simple averages of the cosine(lat) weighted grids, or are zonals taken first then from those the global calculated? Here is what I have for 1979-2011 Global LT trends (used in BAMS Climate of 2011) HadAT2 +0.169, RAOBCORE1.5 +0.129, RICHv1.5 +0.146, RATPAC +0.165, ERA-I +0.121. We need to figure out why these are different than the values in this Table. I suspect it may be the geographic misrepresentation of a global mean produced as the cosine(lat) weighted grids rather than cosine(lat) weighted zonals. [John Christy, United States of America]	Noted. The code used will be released. It calculates trends using OLS from global means derived using zonal means. Zonal means are calculated only where the data exists for three gridboxes within the band.
2-1447	2	37	27	37	30	Table 2.8: significance levels should be assigned to the linear trends. [Guoyu Ren, China]	Rejected. We use 5-95% Cis consistent with guidance and consistent with all other section tables.
2-1448	2	37	27	37	30	Table: remove one digit from all numbers. [Kevin Trenberth, United States of America]	Rejected. We discussed at both the Marrakech and Hobart meetings the issue of how many sf / dp to retain in these tables. In the end the decision was to retain the current table reporting precision so that similarities and differences were more obvious.
2-1449	2	37	33	37	33	tropospheric layers exhibit much smoother geographic trends → this distinction is subtle over land, perhaps “smoother” is enough. [Ileana Bladé, Spain]	Editorial.
2-1450	2	37	35	37	35	exhibits substantial structure → this seems to contradict the earlier statement that the LT trends are smooth. Perhaps “exhibit substantial large-scale structure” [Ileana Bladé, Spain]	Editorial.
2-1451	2	37	36	37	36	There are *also* secondary stratospheric … [Ileana Bladé, Spain]	Editorial.
2-1452	2	37	36	37	36	In addition to this spatial structure in stratospheric temperature trends, there is also temporal structure, because the "trends" are not at all monotonic decreases. [Dian Seidel, United States of America]	Noted. Hopefully this is clear from the timeseries plots and other information already present.
2-1453	2	37	39	37	42	Figure 2.25. Interesting - my original recommendation was to use a simple average of UAH and RSS for this plot. Would it be possible to include ERA-I as a third column? I think this will give confidence (or at least enlightenment) that Reanalyses (at least ERA) is making a lot of progress. There really is no difference (well within error bars) between RSS and UAH LT. I do have a problem with the signficance ascriptions. The authors are aware that signficance means nothing in terms of causation, i.e. statistically signicifant trends over any time period can (and have) been due to natural variability alone. Given the overall bias of the IPCC (i.e. a strong sentiment among the carefully selected authors toward the belief that most of the warming in the past 50 years is human induced) the perception allowed to creep in here is that "significant" means "human caused". A simple statement that "significance does not imply a specific causation" would be very honest and helpful to the readers to help avoid what I see so often in IPCC sections of "jump to conclusion" statements.	Noted. We included Era-Interim in this plot in the FOD and following reviewer comments dropped this in favour of UAH and RSS in the multi-panel configuration. We discussed further at Hobart and decided against changing this figure for a third time. Much though ERA_Interim would be interesting to add space restrictions preclude adding another panel here. The significance is clearly labelled as trend significance. The causes of any changes are the domain of Chapter 10 and we make no effort to

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[John Christy, United States of America]	ascribe causes here.
2-1454	2	37	44	37	44	I suggest some introductory words, such as “Regarding the vertical structure of the trends” to introduce the following two sections properly. [Ileana Bladé, Spain]	Rejected. Space constraints require brevity that preclude such introductory remarks.
2-1455	2	37	44	37	46	The agreement shown in table 2.8 is remarkable. For example the LT (1971-2011) trends all overlap as do the MT trends (1971-2011). It is not clear how one could then state that "distinctly data set dependent". Within the range of uncertainty the datasets show remarkable consistency. Suggest a rewrite - as this degree of agreement represents a significant advance on earlier IPCC reports and implies a significant narrowing of our uncertainty around observed tropospheric trends. [Government of Australia]	Rejected. Differences are still significant when a difference series which removes the covariability between datasets arising from real interannual variations are utilized. Great care must be taken in comparing datasets and uncertainties that appropriate uncertainties are considered.
2-1456	2	37	44	38	3	the differences shown in figure 2.26 and commented here are statistically signiicant? If yes or if not, everywhere (i.e. bot in positive and negative trend zones)? [Claudio Cassardo, Italy]	Noted. Vertically integrated trend significance is readily available from the table for global measures.
2-1457	2	37	44	38	10	Figure2.26 and discussion: this claims to have "global" trends from radiosondes but there is not such thing given the coverage. This needs major work and should account for coverage and its changes over time. Why start trends in 1958 when there is no basis for assuming a linear trend? [Kevin Trenberth, United States of America]	Rejected. Preceding text and text in the appendix makes clear that the radiosondes are not truly 'global'. The basis for starting in 1958 is that this is the start date for the radiosonde products as made clear in the appendix material.
2-1458	2	37	46	37	46	"...data set-dependent,..." [James Renwick, New Zealand]	Editorial.
2-1459	2	38	3	38	3	Should read 'of the order of' [Government of Australia]	Editorial.
2-1460	2	38	8	38	8	what is the impact of the height of the pressure level changing over time in these trends? [Government of Australia]	Noted. This effect is minor compared to the trends.
2-1461	2	38	12	38	13	Insert"in trends" after 'differences" . It is not clear that the datasets differ more in recent years (probably the reverse), but the trends do. [Melissa Free, United States of America]	Accepted.
2-1462	2	38	14	38	14	Although it is cumbersome, it would be good to include version numbers or letters with these radiosonde datasets. For RAOBCORE, there are very large differences between version 1.4 and the current version 1.5, and it is unclear which is used here. [Dian Seidel, United States of America]	Noted. This information is in the figure in-line key.
2-1463	2	38	19	38	19	"...data set-dependent,..." [James Renwick, New Zealand]	Editorial.
2-1464	2	38	19	38	19	For the period 1979-2011, a new study (Seidel et al., 2012) supports this conclusion about the increase of tropical tropospheric temperature trends with height. (Seidel, D. J., M. P. Free, and J. S. Wang, Reexamining the warming in the tropical upper troposphere: Models versus radiosonde observations, Geophys. Res. Lett., doi:10.1029/2012GL053850, in press, accepted 16 October 2012) [Dian Seidel, United States of America]	Noted. As we are discussing here the figure we have decided against addition of a specific citation here - we could have cited many additional papers cited earlier in the section 2.4 but it was felt that this would confuse reader interpretation. We have advised Chapters 9 and 10 taht aspects of thsi study are of interest to them.
2-1465	2	38	20	38	24	Strongly recommend against using the highly politically charged term "unequivocal". Physicists hardly ever use it. Recommend stating "very high confidence". [David L. Hagen, United States of America]	Noted. Unequivocal does not appear within this segment of SOD so no action taken.
2-1466	2	38	24	38	24	I like this well-written summary. [Ileana Bladé, Spain]	Noted. These have now been folded into the end of each section which is consistent with SREX and the remainder of the chapter.
2-1467	2	38	24	38	58	The sentence in line 26 is meaningless without a time period and likely false since the Holocene Climatic Optimum, and 2001, 2002. Strongly recommend correcting this to state: "It is virtually certain that global near surface temperatures have increased since the Little Ice Age c. 1850 and since the cool portion of the multi-decadal oscillation c. 1955." [David L. Hagen, United States of America]	Rejected. Comment has no peer reviewed basis and is asking to move outside chapter scope.
2-1468	2	38	26	38	26	AR4 said the warming was unequivocal. What is the point in saying it is virtually certain. This is a ridiculous statement. The warming is beyond any doubt! Come out and say this. This comes again for land on line 34	Accepted. Our understanding of the uncertainty guidance was that we could not use such language.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						and for SSTs on line 52. These are mutually supportive, so why is it not beyond doubt. If you estimate the chance of being wrong, it is not 1%, but something like 0.000001%. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	We checked this with the TSU at Hobart who advised to the contrary and that where appropriate we could make fact based statements. In light of this both this statement and statements regarding many of the atmospheric constituents have been changed.
2-1469	2	38	26	38	26	"…near-surface…" [James Renwick, New Zealand]	Noted. Summary section has been removed and new summaries added to each section taking parts of the section text and also revised ES text agreed at the Hobart meeting into account. In doing so comments on this text have been taken into account.
2-1470	2	38	26			"...temperatures have increased." append "since the nineteenth century." for consistency with FAQ 2.1 [Richard Allan, United Kingdom]	Taken into account. See response to 2-1469
2-1471	2	38	27	38	28	This would be much more accurately stated by saying "much of which occurred between 1970 and 2000." [David L. Hagen, United States of America]	Taken into account. See response to 2-1469
2-1472	2	38	28	38	28	change '1979' to 'the start of the satellite record (1979)' [Government of Australia]	Taken into account. See response to 2-1469
2-1473	2	38	28	38	28	You need to define what 'much' is! Is much more than 50%, for example. Soon I can see IPCC defining much and some, as they do with likelihood terms. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1474	2	38	28	38	35	At line 28 it is written "since 1979" while at line 35 "since the 1970s". I think that it should be better an uniformity. [Claudio Cassardo, Italy]	Taken into account. See response to 2-1469
2-1475	2	38	28			I suggest replacing "much of which has occurred since 1979." with "much of which has occurred in the 1980s and 1990s" which I think is strictly more accurate. [Richard Allan, United Kingdom]	Taken into account. See response to 2-1469
2-1476	2	38	28			Why is 1979 singled out here for the year after which most of the warming has occurred? Interannual variability makes it difficult to pin things down to a single year, and there is a case for saying "mid-1970s" not "1979". Note that in the following paragraph it is simply stated that warming over land has been marked "since the 1970s". [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1469
2-1477	2	38	30	38	30	State that 'much' (see I'm doing it now - but it is about a third of the variance on interannual timescales) is due to ENSO and Volcanoes. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1478	2	38	31	38	31	The meaning of 'redundancy in measurement and analysis techniques' is not entirely clear here. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1479	2	38	31	38	31	How can confidence (presumably since AR4) have increased beyond unequivocal? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1480	2	38	34	38	35	This sentence, saying that warming has been "marked since the 1970s", is using weasel words to avoid repeating the statement in the previous paragraph that most of the warming has occurred since 1979. [John McLean, Australia]	Taken into account. See response to 2-1469
2-1481	2	38	34	39	13	As a climate generalist/popularizer but non-specialist in temperature data, I find this section well organized and convincing. Would it not be appropriate, however, to mention already here the analysis of the ex-skeptic now in press at JGR-Atmospheres ? [Robert Kandel, France]	Taken into account. See response to 2-1469
2-1482	2	38	36	38	38	Lack of standardisation of instruments in (and prior to) the 19th century should also be mentioned here. [Government of Australia]	Taken into account. See response to 2-1469
2-1483	2	38	37	38	37	See earlier comment - the spread of the datasets before 1880 isn't that much greater. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1484	2	38	37	38	37	Better to change "...prior to 1880 owing to ..." to " ...prior to 1900 owing to ...". [Guoyu Ren, China]	Taken into account. See response to 2-1469

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1485	2	38	42	38	45	The percentages refer to which period? I think this is an important information. [Claudio Cassardo, Italy]	Taken into account. See response to 2-1469
2-1486	2	38	43	38	43	Change to very likely - see earlier comments. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1487	2	38	43	38	45	Please change the sentence "it is concluded that it is likely that ... no more than 10% of the land surface air temperature warming trend globally and 25% regionally in rapidly developing regions" to "it is concluded that it is likely that ...no more than 15% of the land surface air temperature warming trend globally and 40% regionally in rapidly developing regions". [Guoyu Ren, China]	Taken into account. See response to 2-1469
2-1488	2	38	43			Change "reanalyses" to "reanalysis". [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1469
2-1489	2	38	45	38	45	Perhaps not put a" 25% " here, because we have not tested this for all regions. [Government of Australia]	Taken into account. See response to 2-1469
2-1490	2	38	49	38	50	It is not very correct. Zhou and Ren (Zhou, Y.Q. and G.Y. Ren. 2009. Urbanization effect on change of maximum temperature, minimum temperature and daily temperature range in North China. Plateau Meteorology, 28 (5): 1158-1166; Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053) have applied the in-homogeneity adjusted data to analyze the urban effect on DTR trends of North China over the time period 1961-2008, and the dominant influence of urbanization on the linear trends of annual and seasonal mean DTR as observed at urban stations and national stations. [Guoyu Ren, China]	Taken into account. See response to 2-1469
2-1491	2	38	52	38	58	The summary needs to be modified to reconcile the following new findings. However, some studies point out that the temperatures in the last decade went in to a hiatus, contrary to what one would expect from the increased GHGs continuing to trap the energy. Several other explanations have, emerged to explain the recent hiatus ((Meehl et al., 2011; Solomon et al., 2012). While this does not lead to conclude that (a) the anthropogenic climate change is fake, (b) that the GHGs do not have an effect, and (c) that the increasing temperature trend due to anthropogenic warming has stopped (easterlings 2012), however, the possibility that the global warming has abated, either due to natural cycles or adjustment of climate to such forcing, cannot be entirely ruled out at this stage. Easterling, D. R., and M. F. Wehner (2009), Is the climate warming or cooling?, Geophys. Res. Lett., 36, L08706, doi:10.1029/2009GL037810. Meehl, G. A., et al., 2011: Model-based evidence of deep-ocean heat uptake during surface-temperature hiatus periods, Nature Climate Change, 1, 360–364 (2011), doi:10.1038/nclimate1229 Solomon, S. et al., 2010: Contributions of Stratospheric Water Vapor to Decadal Changes in the Rate of Global Warming Science 327, 1219 . DOI: 10.1126/science1182488 [Government of India]	Taken into account. See response to 2-1469
2-1492	2	38	52	38	58	The citing of papers focusses on new papers since 2007 so be consistent and talk about temperatures in the same period. Stop ignoring the elephant in the room by talking about large time spans and clearly state that there has been no statistically significant warming since January 1997. Chapter 10 says this so why isn't here in chapter 2. [John McLean, Australia]	Taken into account. See response to 2-1469
2-1493	2	38	56	38	56	Change 'alter' to 'affect'. These changes haven't altered that much at all. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1494	2	39	1	39	2	Here you say virtually certain, but back on p36 lines 29-31 you said there was low confidence in upper air trends? Also if the trends from balloons and satellites are virtually certain, those from the surface must be better, so they have to be beyobd doubt, so unequivocal. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-1469
2-1495	2	39	1	39	9	The wording here is a bit confusing. When you say it is "virtually certain" the troposphere has warmed globally, but there is only "medium to low confidence" about the rate and vertical structure, the obvious question is, if you have such low confidence about the rate, how are you "virtually certain" it is not zero? I am not arguing that it is zero, what I am saying is that your language, being far removed from conventional uncertainty terminology, is creating paradoxes. You are saying something like, "we are virtually certain the troposphere is	Taken into account. See response to 2-1469

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						warming at a rate we are very uncertain about." Are you trying to say that the confidence interval is wide, but that it does not include zero? Couldn't you just say it that way? I seem to recall the IAC raised this point about contradictory layered statements where you are virtually certain about something for which you have low confidence in the evidence, or vice versa. [Ross McKitrick, Canada]	
2-1496	2	39	1	39	9	Further on this same point, the case of the tropics matters quite a bit to readers and comes up in Chapter 9 and ought to be discussed more clearly. When you say "confidence is low" in the tropical upper troposphere, it is not clear by that point what you are referring to. Confidence in the data quality, or in the existence of a trend? Given the first, the second follows pretty directly. In MMH2010 (ref. next cell; note it is the Correction not the original paper) the trend in the tropical lower troposphere is significant or marginally significant over 1979-2009 in all 4 data sets, but in the mid-troposphere it is insignificant in 3 of the 4 data sets. So there is a fair degree of consistency in those results. [Ross McKitrick, Canada]	Taken into account. See response to 2-1469
2-1497	2	39	1	39	9	R. McKitrick, Stephen McIntyre, Chad Herman, Corrigendum, Atmospheric Science Letters, 2011, 12, 4 [Ross McKitrick, Canada]	Taken into account. See response to 2-1469
2-1498	2	39	2	39	3	I do not think it is accurate to use "virtually certain" here. Observations from mainland China did not show a significant warming of the whole troposphere over the past 50 years (Wang Y, Ren GY. 2005. Change in free atmospheric temperature over china during 1961–2004. Climate and Environment Research (in Chinese) 10(4): 780–790; Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q. and Zhang, J., 2007, Detection, causes and projection of climate change over China: an overview of recent progress, Advance in Atmospheric Sciences, 24 (6), 954-971; Guo Y, Ding Y. 2009. Long-term free-atmosphere temperature trends in China derived from homogenized in situ radiosonde temperature series. Journal of Climate 22: 1037–1051). Besides, is it necessary to exclusively indicate whether the increase is significant statistically? [Guoyu Ren, China]	Taken into account. See response to 2-1469
2-1499	2	39	3	39	6	Please make the SPM conclusion (SPM-3, lines 28-29) consistent with this part of the text. We suggest to change “medium confidence” to “low to medium confidence” in the SPM. [Government of Netherlands]	Taken into account. See response to 2-1469
2-1500	2	39	5	39	6	It is curious to read that confidence is low "particularly … over the shorter period since 1979" since it is since 1979 that we have had a variety of satellite data in addition to radiosonde data. I'm not sure many reanalysis practioners would agree with the statement. Is confidence low beacuse satellite information can't be properly reconciled with radiosonde information? If so, confidence should also be low before 1979 when (apart from VTPR and some yet-to-be-recovered early satellite data) there is only radiosonde data. Or is it just that there has been more low frequency variability since 1979 to confuse the picture? A few extra word might help here. [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1469
2-1501	2	39	8	39	9	add the phrase, "indicating no tropospheric amplification of the surface trend value." [John Christy, United States of America]	Taken into account. See response to 2-1469
2-1502	2	39	8	39	9	Last sentence somewhat confusing - does the statement "encompass surface warming estimates" apply just to satellites, or to all types of observation? If the latter, it appears to contradict the earlier finding that LSAT data-sets virtually certainly show warming. Also, the thrust of lines 6 through 9 appears inconsistent with the confident statement made in lines 46-47 of same page. [Government of Australia]	Taken into account. See response to 2-1469
2-1503	2	39	8			large uncertainty has been amplified here by lack of discrimination among studies and datasets as to their quality and credibility. [Kevin Trenberth, United States of America]	Taken into account. See response to 2-1469
2-1504	2	39	11			I would like to read here something on the current state ok knowledge about the (possible) relationship between tropospheric warming and stratospheric cooling [Franco Desiato, Italy]	Taken into account. See response to 2-1469
2-1505	2	39	13	39	13	Stratospheric temperatures have levelled off over the past (almost) two decades, not just the past decade. [Dian Seidel, United States of America]	Taken into account. See response to 2-1469
2-1506	2	39	14	39	14	In addition to the problems stated that pertain to SSU data from satellites, a key additional problem is the lack of independent observations from in situ observing systems or from other satellite programs. [Dian Seidel, United States of America]	Taken into account. See response to 2-1469
2-1507	2	39	14			"Substantially less mature" is too strong a way of referring to mid-to-upper stratospheric data. "Poorly	Taken into account. See response to 2-1469

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						documented" is not right. Firstly, radiosondes regularly reach the mid stratosphere. Then we have AMSU-A data for the past 14 years, and they are no more or less mature in the upper stratosphere than they are in the troposphere and lower stratosphere. We have HIRS, for which a succession of these instruments has been providing data for the whole period since 1978. SSU data have been around almost as long as MSU data. They have been rather neglected in the past, but have been substantially rehabilitated (and documented) in recent years as noted in an earlier comment, and their use in reanalysis is well established. The same is true for the other mature data noted above. The reanalysis results are in the public domain through conference presentations, and there is a good chance that they will be documented in the literature by around the time this assessment report is published. This precludes their use in AR5, but it would perhaps be prudent to tone down the remarks about the mid-to-upper stratosphere. [Adrian Simmons, United Kingdom]	
2-1508	2	39	18	39	18	The title of the FAQ should be "How do we know the world has warmed?" Using "warming" (present tense) suggests that the authors are naively extrapolating the long term trend forwards. The reasons for believing that "warming" is still occurring despite a reduction in the rate of temperature change comes not from careful perusal of the observations and extrapolation of pas trends but on physical understanding of the climate system. The observed record is an important part of that understanding, but it cannot and should not be made to stand in its place as proof that the world is "warming". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. We had intended to change this in the SOD as noted in responses to the FOD. Unfortunately in pulling together the SOD section contributions this reverted. We now utilize the intended title version here.
2-1509	2	39	18	39	24	The opening of FAQ 2.1 is misleading (see proposed revision to opening line in next row below) in that there is no mention that climate is characterized by cycles of warming and cooling. By mentioning only warming ("That the world has warmed since the nineteenth century is unequivocal."), the less informed reader might incorrectly think that the present cycle of warming is somehow unprecedented. How would scientists of past generations react to this? Consider Thomas Jefferson, who wrote in great detail about the climate of Virginia in letters and in a book. Jefferson, who measured the low and high temperature every day from 1776 to at least 1824, observed, measured and then wrote: "A change in our climate however is taking place very sensibly...Snows are less frequent and less deep...The elderly inform me the earth used to be covered with snow about three months in every year...." (Thomas Jefferson. 1782. Notes on the State of Virginia. University of Oxford.) Should not policy makers be informed that cycles of warming and cooling are common in both the historical and paleo records? The next row below offers a suggested amendment to this section. [Forrest Mims, United States of America]	Rejected. The FAQ concentrates on our knowledge from the instrumental / direct observation records available. To broaden in this sense goes beyond the agreed remit for the FAQ. It is beyond doubt that the world is warmer in the early 21st Century than it was in the late 19th Century and this factual statement is correct.
2-1510	2	39	18			FAQ 2.1 is nicely done, but it a major change in style and tone from the rest of the chapter. Does is really belong in the chapter, or will it be pulled out later for inclusion in a separate FAQ document? In this location, the contrast with the very technical style of Ch. 2 makes the latter seem unduly dense. [Dian Seidel, United States of America]	Noted. It will exist both within the Chapter and elsewhere.
2-1511	2	39	18			FAQ 2.1: We note the use of 'multiple climate indicators' in the chapeau, and would encourage the authors to emphasise the independence of these multiple lines of evidence. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Independent has been added here.
2-1512	2	39	18			FAQ 2.1: Figure 2: We would encourage adding a sentence to the caption, directing the reader to the relevant sections of Chapter 2, 3 and 4 where the quantities shown here are assessed. This is important, because in some instances different datasets are shown here to those used in the underlying chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. We held a cross-chapter meeting in Hobart to address the issue of consistency and relevant section leads will now submit their datasets they feel are suitable for inclusion to the panels. We will add pointers to the relevant Chapters to the appendix material where the datasets included are detailed.
2-1513	2	39	20	39	20	Continuing from the previous row above, here is a proposed brief addition and revision to the opening line of FAQ 2.1: "Climate is characterized by alternating periods of warming and cooling. Evidence that the world is now warming comes from…." [Forrest Mims, United States of America]	Rejected. See response to 2-1509
2-1514	2	39	20	40	13	It is impossible to measure the average tempeature of the earth. let alone any part of the :climate system, so there is no reliable evidence that it is warming. However the currently available temperature series all agree that there has been no warming for the past ten years, so the "warming is certainly NOT "equivocal" and this entire paragraph is nonsense. [Vincent Gray, New Zealand]	Rejected. The paragraph is about long-term change and not the last ten years. Regardless, as shown earlier even since 1998 the datasets agree that the global mean surface temperature has increased.
2-1515	2	39	21	39	21	Suggest reference to David A. Jones, Dean A. Collins , Simon McGree, Blair Trewin, Eden Skilling, Howard	Rejected. Comment is likely misplaced as it has no

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Diamond, Ofa Fa'anunu, David Hiriasia, Sébastien Hugony, Rod Hutchinson, Kasis Inape, Lee Jacklick, Philip Malsale, Terencio Moniz, Maria Ngemaes, Alan Porteous, Sunny Seuseu, Llyod Tahani, Franklin Teimitsi, Ueneta Toorua, Maarametua Vaiimene, Varanisese Vuniyayawa and Hilia Vavae. Submitted to Australian Meteorological and Oceanographic Journal 20 July 2012. Accepted subject to minor revisions, October 2012. "An updated analysis of homogeneous temperature trends at Pacific Island stations" [Government of Australia]	relevance to the text. Further, FAQs are not to include citations.
2-1516	2	39	21	40	21	I realize that this is an FAQ, but you could give a number for the warming since 1900. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. We would prefer not to add numbers that detract from the flow. The numbers are available in the Chapter text, the chapter summary and presumably the SPM. It feels like overkill to include them again and comes at the expense of clarity.
2-1517	2	39	26	39	26	Replace "Discussion around warming" by "Discussion around uncertainties of warming". Reason: Be more explicit. [Urs Neu, Switzerland]	Taken into account. See response to 2-1518
2-1518	2	39	26	39	26	This FAQ, and several others, use the word "around", which I suspect may not be understood in the same way by all readers. I suggest replacing "Discussion around warming" with "Discussion about climate warming". [Francis Zwiers, Canada]	Accepted.
2-1519	2	39	26	39	29	FAQ 2.1: This is not a good way to start this FAQ. Only some segments of the public centre a discussion around warming on biases in the land-based weather stations. Suggest moving this first sentence down to tag onto line 38, but the sentence will also require some explanation (are these biases real, have they been adequately dealt with in the estimation of the warming since 1900, etc). The second sentence repeats what is in the italicized header so could be deleted. The text for the FAQ would then start with line 36. [Government of Canada]	Noted. Rather than delete this text we would prefer to nuance it to address this concern as the rationale behind the analysis is weaker without its inclusion.
2-1520	2	39	28	39	28	Replace "a wide range of physically-consistent measurements" by "a wide range of independent physically-consistent measurements". Reason: it is crucial for the reasoning that these measurements are independent (as in the figure caption). [Urs Neu, Switzerland]	Accepted.
2-1521	2	39	33	39	33	delete comma that is separating the subject from the verb [Ileana Bladé, Spain]	Editorial.
2-1522	2	39	33	39	33	Delete comma between "world" and "exhibit". Or add a comma before "which" in line 32. Reason: is misleading to reader [Urs Neu, Switzerland]	Taken into account. See response to 2-1521
2-1523	2	39	36	39	38	I suggest this paragraph be made more quantitative by specifying the actual trend in surface temperature, from the chapter text. [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1524	2	39	38	39	38	Replace 1900 by 1950. Reason: Anthropogenic warming is mainly since 1950. Many other paragraphs refer to the period since 1950 (e.g. ocean heat uptake in line 50, or FAQ 2.2, first sentence). We should try to relate always to the same period if possible. [Urs Neu, Switzerland]	Rejected. The rationale behind different periods is that the length of record differs for different variables as is evident from FAQ 2.1, Figure 2. The FAQ is not concerned with the underlying causes.
2-1525	2	39	40	39	42	I suggest this paragraph be made more quantitative by specifying the actual trend in ship and SST temperatures, from the chapter text. [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1526	2	39	41	39	42	Suggest mentioning SSTs before marine air temperatures, since we presumably have more confidence in the SSTs than in NMATs. [Francis Zwiers, Canada]	Rejected. Text flows better for a reader by talking about air temperature over land and the ocean and then sea surface temperatures. The reviewer is correct, however, that SST analyses are more mature.
2-1527	2	39	44	39	45	Why should this follow directly from the fact that the ocean and atmosphere are both fluid bodies? The fluids could be highly stratified … [Francis Zwiers, Canada]	Rejected. Mixed could be added to make clear that they are not substantially stratified. There is a conflict here between scientific exactitude and public understandability and we wrestled with this in response to FOD review comments which suggested the original use of mixed was incorrect. It is unclear

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							that we can come up with a solution that pleases all sceintists!
2-1528	2	39	44	39	46	After 130 years of very close correlation, HadSST2 started diverging from CRUTEM3 in 1980. Assuming that the oceans recently started absorbing more heat is likely wrong because we would have seen more divergence in the two temperature datasets previously (e.g. 1915-1945 when temperatures were rising). A warm ocean would, logically, lose more heat to the atmosphere via evaporation and convection. Simple physics says that heat rises, not sinks. To claim that the oceans are now storing heat cannot be sustained because ARGO and other monitoring would have recorded that heat in passing, but this did not happen. [John McLean, Australia]	Rejected. These issues are discussed in the relevant chapters and such details are not appropriate in a FAQ.
2-1529	2	39	44	39	47	Can some numbers be given here for observed changes or trends in upper ocean temperature, and in tropospheric temperature ? [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1530	2	39	44			suggest deleting "if the" and "is real, it" to avoid the impression of doubt. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
2-1531	2	39	45	39	25	Add 'deeper' to 'down' or qualify somehow. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-1532	2	39	46	39	46	"Analyses of measurements made by weather balloons" - weather balloons do not make measurements, radiosondes do. [Dale Hurst, United States of America]	Accepted.
2-1533	2	39	47	39	47	FAQ 2.1: Add a statement about warming of the ocean as the first sentence in this paragraph mentions how warming will propagate up into the lower atmosphere and down into the upper oceans. [Government of Canada]	Noted. The first sentence of the next paragraph deals with this aspect. We have now made that the last sentence of this paragraph and started the subsequent pargraph one sentence later.
2-1534	2	39	47	39	47	Replace "the active weather layer of the atmosphere" by "the layer of the atmosphere with intense air exchange to and from the surface layer". Reason: This seems more relevant in relation with the discussed argument. [Urs Neu, Switzerland]	Rejected. This is unduly complicated for the required remit of a FAQ.
2-1535	2	39	48	39	48	FAQ2.1: Suggest adding here a short paragraph about stratospheric cooling. This cooling is reported on in the SPM as an indicator of climate change but as it is not readily understood as part of the 'warming story'. Readers need to be directed somewhere where this topic is explained. [Government of Canada]	Rejected. The FAQ is including solely parameters we would expect to be correlated with surface temperatures without invoking the need for a tutorial.
2-1536	2	39	49	39	49	Use % for consistency [Peter Burt, United Kingdom]	Accepted
2-1537	2	39	49	39	49	FAQ2.1: Should the word 'excess' be added before 'energy' (i.e., more than 90% of the excess energy absorbed by the climate system…..)? [Government of Canada]	Accepted.
2-1538	2	39	49	39	53	Add the actual precentage contributions to sea level rise of thermal expansion, melting of glaciers and ice sheets, and changes in land storage ? [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1539	2	39	50	39	50	Replace "global records of ocean heat content" by "the increase in global records of ocean heat content" [Urs Neu, Switzerland]	Taken into account. See response to 2-1540
2-1540	2	39	50	39	50	Suggest replacing language such as "as evidenced in" with "can be seen from" to try to keep the responses as accessible as possible. [Francis Zwiers, Canada]	Accepted.
2-1541	2	39	52	39	53	Add "irrigation". E.g., "as do changes in land storage of water, and mining water for irrigation." [David L. Hagen, United States of America]	Noted. Have changed land storage to land storage and usage here.
2-1542	2	39	52	39	53	Replace "land storage of water" with "storage of water on land". Maybe add a note that the contribution to sea-level change from ground water pumping and impoundment is small. [Francis Zwiers, Canada]	Noted. Some changes have been made here to this end although the caveat seems unduly complicated and there is some literature which suggests the impact may be non-negligible which we should be careful not to appear to discount here.
2-1543	2	39	55	39	55	"because warmer air can hold more water vapour." [Dale Hurst, United States of America]	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1544	2	39	55	39	55	This sentence is very doubtful. Simple hydrology says that a shortage of surface moisture results in higher temperatures, as was the case in Australia from 2002 to 2008. [John McLean, Australia]	Noted. Edit in response to 2-1543 addresses.
2-1545	2	39	55	39	57	This paragraph is rather vague, and the "on average" in the first sentence makes it just about passable. But I have several detailed comments on this point below. [Adrian Simmons, United Kingdom]	Noted. No specific suggestions given in this particular comment so no changes made in response. See subsequent responses to detailed comments on the hydrology section.
2-1546	2	39	55	39	57	Give a number for the observed trend in specific humidity over the land, and over the oceans ? [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1547	2	40	1	40	8	Give numbers for the observed cryospheric trends ? [David Wratt, New Zealand]	Taken into account. See response to 2-1516
2-1548	2	40	2	40	2	Maybe replace "icy" with "frozen", to use a word that maybe better encompasses all parts of the cryosphere. [Francis Zwiers, Canada]	Accepted.
2-1549	2	40	3	40	3	It was stated earlier in this report that many glaciers started retreating in 1850, at the end of the Little Ice Age. To now claim that this has only occurred over the last 20 years is inconsisten as well as false. [John McLean, Australia]	Rejected. The claim here is about seeing annual decreases every year for over 20 years, not about the long-term trend. There is no conflict as asserted by the reviewer between the earlier text and this point.
2-1550	2	40	3	40	3	Maybe replace "stored in" with "contained in"? [Francis Zwiers, Canada]	Accepted.
2-1551	2	40	4			add the words, "at least partially" to line 4 so that it reads, "...and the lost mass at least partially contributes to..." [Government of United States of America]	Noted. Have added ', in part,' here.
2-1552	2	40	5	40	5	Change to "spring and summer" (twice). Snow cover has decreased just as much in summer as in spring, if not more. [Geert Jan van Oldenborgh, Netherlands]	Noted. While the reviewer is correct the plotted parameter is spring snow cover and it is the spring cover that is of most immediate and direct interest here.
2-1553	2	40	6	40	7	Please also report that anecdotal evidence from shipping records shows that Arctic ice retreated significantly in the 1930s. Such a statement appears in chapter 10, so why is it not here? [John McLean, Australia]	Noted. The reviewer has answered their own question in that it is not based upon instrumental data which is the basis for such datasets.
2-1554	2	40	7	40	7	Suggest replacing "the time of the summer minimum extent" with "the time of the mimimum extent, which occurs in September at the end of the annual melt season", or something of that type, to give a bit more information about when the minimum occurs. [Francis Zwiers, Canada]	Accepted.
2-1555	2	40	7	40	8	Better: "By contrast, the increase in Antarctic sea ice has been smaller." [Geert Jan van Oldenborgh, Netherlands]	Accepted.
2-1556	2	40	8	40	8	FAQ2.1: The lack of change in Antarctic ice needs at least a bit of context. Suggest strong regional warming is noted in discussing arctic sea ice loss and then some mention could be made of regional temp change in the Antarctic and whether or not other factors are also implicated. [Government of Canada]	Taken into account. See response to 2-1557
2-1557	2	40	8	40	8	Is it possible to briefly mention increasing sea ice maxima in Antarctica here in addition to its distinctly different causal mechanism compared to the Arctic (winds vs temperatures) or is that too much to get into here? [Kate Willett, United Kingdom]	Rejected. As the reviewer notes this is too detailed for this FAQ discussion
2-1558	2	40	8			Misleading to say "comparatively little change", which most readers would probably interpret immediately following the Arctic statement as still suggesting a small decrease or no change. Better to be accurate (and consistent with chapter 4), and state "a small increase". [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account. See response to 2-1555
2-1559	2	40	10	40	10	The first line in this paragraph is too cautious. How about: ' Many analyses show global warming in the last century is virtually certain, changes in other datasets are less certain due to gaps or uncertainties, however... [European Union]	Rejected. We prefer the current wording here.
2-1560	2	40	10	40	14	From a purely aesthetic standpoint I would remove "of increasing water vapour" from this sentence and revert to the FOD version of this final paragraph. [John Kennedy, United Kingdom of Great Britain & Northern	Noted. We have removed this text but retained the rest of this paragraph.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ireland]	
2-1561	2	40	10	40	21	This paragraph and figure make a fair point - however, the reader is left wondering if the documented changes are due to global variability or anthropogenic enhancement/changes? [Government of United States of America]	Rejected. This is out of FAQ scope. To conflate with these issues would conflict with other FAQs and Chapters.
2-1562	2	40	10			Reads: "Individually, any single analysis might be unconvincing, but ..." This seems a little too conciliatory; I suggest: "Although a single given analysis might not be viewed as conclusive, the fact that multiple independent indicators and data sets reach such a conclusion allows high confidence to be placed in that conclusion." [Stephen E Schwartz, United States of America]	Rejected. We prefer the current wording here.
2-1563	2	40	11	40	11	FAQ2.1: As worded, this sentence suggests there are many independent research groups assessing the collective data sets on different indicators. If that is the case, fine, but if not, rewording is recommended to clarify that assessment of separate lines of evidence by different research groups, has led to conclusions of warming, and when considered together, the collective evidence renders the conclusion unequivocal. [Government of Canada]	Rejected. As a cursory glance of FAQ 2.2 Figure 2 would confirm there are indeed multiple independent analyses of each indicator so the current wording is as intended.
2-1564	2	40	11	40	13	Suggest removing the word "all", because we can't know for sure what all research groups, or individuals within or outside groups, conclude. [Dian Seidel, United States of America]	Noted. We prefer to retain the current wording here.
2-1565	2	40	11			The use of the words "many" and "all" in the statement "many independent research groups all reach the same conclusion" is a bit clumsy. The word "all" does not add anything. Many research groups may reach the same conclusion, but other research goups may reach a different conclusion. The statement would be strengtherned if it could be stated "almost all research groups reach the same conclusion" or even "all research groups ...". But the word "all" as used in line 11 does not strengthen the statement. [Adrian Simmons, United Kingdom]	Taken into account. See response to 2-1564
2-1566	2	40	13	40	13	Add "since 1900" to the end of the paragraph. [Dian Seidel, United States of America]	Noted. We have added since the late 19th Century as some series go back to 1850.
2-1567	2	40	14	41	50	Recent paper by Quintana and Aceituno Atmósfera 25(1), 1-22 (2012) describe from observation what happens in the western side of southern South America [Government of Chile]	precipitation was not assessed on a regional basis in this chapter. This would be beyond the scope of this chapter, regional is considered in Chapt. 14.
2-1568	2	40	14	41	54	Section 2.5.1.2 Spatial Variability of Observed Trends: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Thanks
2-1569	2	40	16	40	21	I draw your attention to the lack of correlation between the relatively consistent rise in sea level and the intermittent periods of warming depicted in the sea surface temperature graph. (E.g. no warming 1945-1976 and yet sea level continued to rise.) Please explain why the rise in sea level is not consistent with SST. [John McLean, Australia]	Rejected. This is addressed within other relevant chapters and is out of scope here.
2-1570	2	40	17	40	17	Suggesting using a different word to "redundant". This scientific meaning and the common meaning and interpretation can be very different. [Government of Australia]	Accepted. Have used independent as suggested elsewhere
2-1571	2	40	17	40	21	The sea level data sets used here are inconsistent with what is in Chapter 3. At least need to add Ray and Douglas sea level time series. Are the glacier mass balance time series consistent with Chapter 4? Suggest these need updating. [Government of Australia]	Taken into account. See response to 2-1512
2-1572	2	40	18		19	"All data sets have been used." What about tree-ring proxy temperature data? [Stephen E Schwartz, United States of America]	Noted. We have clarified within the caption that we are talking about instrumentally based datasets here.
2-1573	2	40	25	41	50	The observation chapter (2.5), which in the end gives the impression we know less about precipitation: is there no publication giving some confidence in observed reinforcement of seasonal contrasts in well observed (1951-2011) mid latitude regions ? This seems to be the case in France and in the Iberic peninsula, based on discussion with scientists. [Government of France]	Declined. It is not at all clear what is being asked for by the comment and includes reference to anecdotal evidence about some precipitation changes in France etc.
2-1574	2	40	25			Section 2.5: In general section 2.5 devotes too much space to saying what AR4 said and not enough to saying	Declined. AR4 results are summarized to place more

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						what has changed since AR4. This is particularly true of sections 2.5.1, 2.5.2 and 2.5.3. [J. Graham Cogley, Canada]	recent results into proper context. Further, for global scale precipitation not that much has changed in 5 years.
2-1575	2	40	25			In section 2.3.1 of PBMC - chapter 2 (Hydrology: precipitation and streamflow) there are informations about changes in precipitation in South America. This section 2.5 of IPCC WG1 AR5 only mention on page 41: "in the mid-latitudes of the SH there is much decadal variability but little evidence of long-term change". However, there are many papers about decadal variability in many regions of South America, e.g., Amazonia. [Government of Brazil]	Declined. The statement refers to much decadal variability and is referenced so no need for additional references. Also, no references were provided in the comment.
2-1576	2	40	25			Section 2.5, Changes in Hydrological Cycle is generally well written. The section is a fair and balanced review of the current state of knowledge and correctly recognizes the limitations placed on such analysis by the scarcity and quality of data. [Government of Canada]	Thank you for the compliment.
2-1577	2	40	25			Considering that the hydrological cycle does not change, to be more accurate we have to talk about "changes in the variables of the hydrological cycle" or "changes in the componens of hydrological cycle" [this comment could be applied for other chapters of the report] [José Daniel Pabon-Caicedo, Colombia]	Declined. The water cycle changes if one or more component changes. Only the general definition of the water cycle (e.g. composed of evap, precip, runoff etc.) doesn't change.
2-1578	2	40	27	40	30	This sentence states that water vapor is the most important greenhouse gas. It would be helpful to explain that this because of it's abundance, not its global warming potential. Otherwise this can be seen to contradict Chapter 6 which states that CO2, CH4 and N2O are the most important greenhouse gases (representing 80% of total radiative forcing). [Government of United States of America]	Accepted, will provide a brief revision.
2-1579	2	40	27	40	37	Section 2.5 Changes in Hydrological Cycle: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Thank you for the compliment.
2-1580	2	40	27	49	24	Given the importance of the hydrological cycle is it possible to summarise the weaknesses in our understanding and whether these are due to observational data limitations, model capabilities or other? [European Union]	This is included in AR4 and owing to space constraints the reader is pointed to that discussion.
2-1581	2	40	28	40	28	Insert 'on' after 'as' [Peter Burt, United Kingdom]	Editorial, will be corrected
2-1582	2	40	30	40	30	water vapor … also the most important greenhouse gas, providing roughly 90 of the 150 Wm-2 of the natural greenhouse effect. I suggest adding this quantification here, even if it appears in the response to the FAQ on water vapor forcing because some self-styled s(k)eptics quote a figure of 99% coming from a non-peer-reviewed book review. [Robert Kandel, France]	See answer to 1578
2-1583	2	40	30			Suggest re-wording without "important" or being more clear as to what (e.g., absolute effects rather than changes) is being referred to. The relative importance of CO2 and water vapor depend on the parameter under consideration. [Government of United States of America]	Editorial, will be corrected
2-1584	2	40	31	40	32	Satellite estimates of precipitation may include land areas since 1979, but are they sufficiently reliable for climate purposes, especially at middle and high latitudes? [Adrian Simmons, United Kingdom]	Accepted, will provide a brief revision.
2-1585	2	40	31			This seems to imply that land is well covered, but ocean is not. There are extensive land regions that lack long-term measurements of precipitation. [Government of United States of America]	Will clarify. The high latitudes, are not well covered, esp. in the early 20th century, as well as Africa and S. Am.
2-1586	2	40	32	40	32	"near global coverage …", but satellite estimate of precipitation is not reliable for high latitude. [Xuebin Zhang, Canada]	Accepted, will provide a brief revision.
2-1587	2	40	33	40	33	Insert comma aftyer 'cycle' [Peter Burt, United Kingdom]	Editorial, will be corrected
2-1588	2	40	34	40	34	This is too general. More specific information about the methods used, including how expert views were integrated to estimate the probability would make things more convincing. [Dora Marinova, Australia]	Decline, beyond the scope here.
2-1589	2	40	35	40	36	"A more detailed discussion of issues with measurements of precipitation, and climate impacts of the	This entire assessment of global and latitudinal bands

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						hydrological cycle including aerosols and the energy balance and other impacts are contained in section 3.3 of AR4." In view of the statements in the executive summary on chapt 2, page 4, line 29-32 "When virtually all the land area is filled in using a reconstruction method, the resulting time series shows little change in land-based precipitation since 1900. This is different from AR4, which reported that global precipitation averaged over land areas has increased, with most of the increase occurring in the early to mid 20th Century" and line 45-47 "The most recent and most comprehensive analyses of river runoff which include newly assembled observational records do not support the AR4 conclusion that global runoff increased during the 20th Century", would it not be preferable (rather than relying solely on AR4 discussion) to also highlight a few changes that have occurred in methodology/analysis with regard to measurements of precipitation and new insights on the climate impacts of the hydrological cycle, apart from using the 'statistically reconstructed data' (page 40 lines 52-54) with two citations. [Government of United States of America]	has been revised. Yes, this assessment is different, partially due to new insights on data availability issues in the first half of the 20th century.
2-1590	2	40	36	40	36	delete “and other impacts”; you already said climate impacts will be discussed [Ileana Bladé, Spain]	Editorial, will be corrected
2-1591	2	40	36	40	36	Replace "and other impacts" with a comma. [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1592	2	40	39	41	10	2.5.1 Large Scale Changes in Precipitation. This is missing the major research of WJR Alexander et al. on the > 100 year data over Southern Africa. Add reference: "WJR Alexander et al., Linkages between solar activity, climate predictability and water resource development, Journal South African Institution of Civil Engineering Vol 49 No 2, June 2007, Pages 32–44, Paper 659" http://nzclimatescience.net/images/PDFs/alexander2707.pdf and "WJR Alexander (2006) Climate change and its consequences – an African perspective, Technical report, 474 pp." [David L. Hagen, United States of America]	Declined. Firstly, the paper is about South African rainfall, not large (continental) scale changes. Second the Alexander et al. paper is not in a mainstream or even secondary climate journal thus the expertise of the reviewers is of question. Last, solar variability on these time scales is much too small to have a real influence on the hydrological cycle.
2-1593	2	40	39	41	10	Strongly recommend adding: "Alexander et al. (2007) found a high degree of assurance that there is a synchronous linkage between the statistically significant, 21-year periodicity from the Hale solar cycle in rainfall (precipitation), river flow (runoff), and flood peak maxima, but not in surface evaporation. They found a 600% difference in average river flow for the three years about the solar minimum versus the following three years." [David L. Hagen, United States of America]	See answer to previous comment (1592).
2-1594	2	40	41			Section 2.5.1.1: This section is confusing. The title indicates land and land-ocean. However, the text is variably ambiguous on which domain is being discussed at any time. The context suggests that it is always land-only that is being spoken of: analysis of trends in the land-ocean data of Smith et al. is mentioned only in comparison with what appear to be land-only data sets. (The caption of Figure 2.28 is itself ambiguous and does not resolve the ambiguity in the text.) [Government of United States of America]	Fig. 2.28 is land only for all data sets shown. Discussions about data problems, such as inhomogeneities and data coverage that appeared in AR4 are not repeated owing to space constraints but the reader is pointed to AR4. We will consider a new figure showing ocean precipitation from various data sets.
2-1595	2	40	43	1	3	Please give version numbers for all datasets used. Is the CRU dataset CRU TS 3.10.01? [Geert Jan van Oldenborgh, Netherlands]	Accepted, will provide a brief revision to include version numbers.
2-1596	2	40	43	40	43	The sentence is talking about the datasets used in AR4 but the reference for CRU is 2012, this was not the dataset used in AR4, but an earlier version [Moira Evelina Doyle, Argentina]	Yes it was an earlier version but discussed in Harris et al. 2012.
2-1597	2	40	43	40	46	The period of the shown trends is far too long. 5AR reports on papers published since 2007 so why isn't the data for the last 15 or 20 years explicitly discussed too? [John McLean, Australia]	We are concerned with long term changes, not shorter term decadal variability.
2-1598	2	40	43	40	58	Fig 2.28 is confusing. This section appears to refer only to land based precipitation but in places it implies otherwise. Nonetheless there is no discussion of ocean precipitation at all. The increases in land precip are known to be associated with ENSO and the La Nina phase, and lack of discussion and consideration of this aspect leads to erroneous and misleading conclusions. Even for the land there is the issue of changing observational database that is not dealt with adequately. There are also other major concerns with section 2.5.1.1 and Fig 2.28 which shows quite large and really unacceptable disagreement among the changes over time, especially in recent years. These differences are greatest for 30N to 30S and GPCP does not seem realistic in its La Nina signature (which is verified by sea level changes in Boening et al. 2012 GRL). [Kevin Trenberth, United States of America]	Fig. 2.28 is land only for all data sets shown. Discussions about data problems, such as inhomogeneities and data coverage that appeared in AR4 are not repeated owing to space constraints but the reader is pointed to AR4. We will consider a new figure showing ocean precipitation from various data sets. GPCP may not seem realistic for ENSO but it is processed correctly for the figure.
2-1599	2	40	43	41	10	Section 2.5.1.1 Global Land and Combined Land-Ocean Areas: Text was checked for inconsistencies with	Thank you for the compliment.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	
2-1600	2	40	43	41	10	Its not obvious which (if any of these) are land-ocean combined products. Where are the ocean only products of CMAP, GPCP and RSS? [Kate Willett, United Kingdom]	These are all land only plots. Ocean precip is assessed in the oceans chapter.
2-1601	2	40	43	41	50	It is clear that data coverage is really limiting the ability to constrain and understand changes in precipitation which arguably have the most direct impact on society/livelihood. The authors could make clearer statements on where the limitations of the observing system are greatest due to coverage gaps or larger variability leading to greater uncertainty. [European Union]	We clarify this in the assessment statements in the Exec. Summary and text.
2-1602	2	40	43	41	50	Section 2.5.1.1 Its clear that data availability is a massive issue here. Is this a good place to state that monitoring capacity needs to be improved and historical data digitised/shared? Also, there seems to be a lot of talk of wet regions getting wetter, dry regions getting dryer and intensification of the hydrological cycle. Can this be concluded given recent literature (Zhou, Y. P., K-M. Xu, Y. C. Sud and A. K. Betts, 2011: Recent trends of the tropical hydrological cycle inferred from Global Precipitation Climatology Project and International Satellite Cloud Climatology Project data. J. Geophys. Res., 116, D09101, doi:10.1029/2010JD015197.) and if so it would be good to link in with other indicators of this such as ocean salinity Durack, P. J., S. E. Wijffels and R. J. Matear, 2012: Ocean Salinities Reveal Strong Global Water Cycle Intensification During 1950 to 2000. Science, 336 (6080), 455-458, DOI: 10.1126/science.1212222. [Kate Willett, United Kingdom]	It might be a good place but this may not be an appropriate kind of statement for an assessment.
2-1603	2	40	43	41	54	See major comment. Precipitation is intermittent and changes should deal with frequency, intensity, duration, sequence, type and amount, not just amount. Fig. 29 is not readable. The language lines 34-36 is confusing. The uncertainty in part arises from the changing coverage and stations that come and go and differ among datasets. This is not done well in this section [Kevin Trenberth, United States of America]	We appreciate the comment, however this is a literature assessment, not new research. Fig. 2.29 is being redone for readability. We also acknowledge that data coverage etc. contributes to the uncertainty.
2-1604	2	40	43			It should be mentioned that these are (presumably) raingauge-based estmates of precipitation. [Richard Allan, United Kingdom]	Yes, will clarify
2-1605	2	40	45	40	45	I think you mean the trend was positive (not increasing, which technically means a different thing) [Ileana Bladé, Spain]	Yes, will clarify
2-1606	2	40	45	40	45	should 'increasing' be 'positive' here? [Government of Australia]	Yes, will clarify
2-1607	2	40	47	40	47	"trends in". [J. Graham Cogley, Canada]	Thanks.
2-1608	2	40	48	40	48	precipitation → land precipitation [Ileana Bladé, Spain]	Yes this is land only, clarified in text.
2-1609	2	40	48	40	49	We are wondering how a paper in 1992 could provide a data set updated through 2011? Is the Vose et al. (1992) reference still the best to be used? We suggest the citation is moved to come after "GHCN data set". [Thomas Stocker/ WGI TSU, Switzerland]	Unfortunately yes this is still the best reference for GHCN precipitation data. It is regularly updated, but a new paper has not been written.
2-1610	2	40	49	40	52	Sentences seem fractured. Maybe rephrasing? [Birgit Hassler, United States of America]	will consider rephrasing
2-1611	2	40	49			Similarly to #28, it could be mentioned that GPCP is a combined satellite and rainguage dataset [Richard Allan, United Kingdom]	Yes, will clarify
2-1612	2	40	50	40	56	GPCP produced different data products, which product is used here? [Xuebin Zhang, Canada]	will clarify version number
2-1613	2	40	51	40	51	Add abbreviation for Global Precipitation Climatology Center in brackets? [Birgit Hassler, United States of America]	Editorial, will be corrected
2-1614	2	40	52	40	52	"...data set by Smith et al. (2010)." [James Renwick, New Zealand]	Editorial, will be corrected
2-1615	2	40	52	40	53	but EOF reconstruction can only be as good as input source data. [Xuebin Zhang, Canada]	Yes, but the large scale patterns are well captured by EOFs, although its unclear how well for precipitation.
2-1616	2	40	54	40	54	Vose et al., Submitted skip or update [European Union]	Decline, this is still the relevant reference for GHCN.
2-1617	2	40	54	40	54	These two NOAA products provide coverage everywhere by EOF approaches. There are places with little or	Places with large errors owing to lack of data (e.g.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						know data, such as the Antarctic. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Antarctica) are excluded. The methods don't completely fill in the globe.
2-1618	2	40	55	40	56	Not clear from the text what region or period the first part of the sentence refers to. I assume the “global” region (bottom plot). What do the non-mentioned datasets show? Also, it is fair to compare a land/sea average with land-only averages? Finally, it is hard to identify the curves with significant upward trends in this figure and you don't provide a table with trend values … [Ileana Bladé, Spain]	we have added the data sets to the plot. Also, these are all land only.
2-1619	2	40	55	40	56	The CRU precipitation also shows an increase since 1901 in Figure 2.28. Also, update to incorporate Smith, T.M., P.A. Arkin, L.Ren, and S.S.P. Shen, 2012: Improved Reconstruction of Global Precipitation since 1900. J. Atmos. Oceanic Technol., 29, 1505–1517 (doi: http://dx.doi.org/10.1175/JTECH-D-12-00001.1) who appear to show, in their Fig. 8, a bit more of an increase than does their previous analysis. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Yes, in the new table of trends CRU shows some significant trends. Also we have updated to the new verision of Smith et al. 2012.
2-1620	2	40	56	40	56	It is not very clear from the sentences before that the GPCC and Smith data sets are different data sets. Please clarify. [Birgit Hassler, United States of America]	Will clarify.
2-1621	2	40	58	40	58	The legend indicates land precipitation but the yellow curve is for the global (land/sea) Smith dataset. Also “global” used further down denotes “over all land areas”, which is confusing. [Ileana Bladé, Spain]	The Smith data set was masked to provide land only. This is clarified in the text.
2-1622	2	40	58	40	58	Why is the climatological reference period here 1981-2000, instead of 1961-1990 as stated in the Introduction and used elsewhere? [Ileana Bladé, Spain]	will correct.
2-1623	2	40	58	40	58	The GHCN dataset has been updated and changed/improved ? The green bars look somewhat different than the ones in the corresponding AR4 figure 3.12. I am also surprised by the choice of lumping together wet tropical and dry subtropical regions (panel c in Fig. 2.28). [Ileana Bladé, Spain]	Yes, the data set has been updated. Also there are many ways to do latitudinal bands.
2-1624	2	40				How do the precipitation products shown in Figure 2-28 agree on means? For example what are the maximum and minimum values of the global annual mean precipitation? [Government of United States of America]	Decline, this is not relevant for looking at temporal changes. The global mean precipitation is different for different data sets owing to differences in the spatial distribution of the input data etc.
2-1625	2	41	1	41	3	Fig. 2.28: For temperature the base period for climatology was 1961-1990. Why is the period 1981-2000 used for precipitation? A same period would be adequate. In page 2-6, lines 14-15, it is informed that where possible the interval 1961-1990 has been chosen as the climatological reference period. Is it not possible for precipitation? Why? [Alice Grimm, Brazil]	This is to include satellite products as well as in situ.
2-1626	2	41	1	41	3	(figure 2.28) Show the same vertical scale on all 5 graphs and do NOT smooth these graphs but show the real variation. (Overlay the smoothed lines if you must, but show readers the truth about variability.) Also, the use of latitude bands masks significant localised variations and should be replaced with regional data. [John McLean, Australia]	Declined. This would be an ureadable graph, providing the actual GHCN values with others smoothed makes this point without trying to plot all the values.
2-1627	2	41	2	41	2	Why is a 20-year climatology being used for rainfall? This should be at least 30 years. [Government of Australia]	see answer to 1626.
2-1628	2	41	2	41	3	Missing closed parenthesis. [Government of Japan]	Editorial, will be corrected
2-1629	2	41	5	41	5	It is surprising that in the tropics the CRU positive anomalies before the reference period 1981-2000 are so much larger than those in the other datasets. Could you comment on that? [Ileana Bladé, Spain]	Yes, will clarify
2-1630	2	41	5	41	5	It should be mentioned here that snowfall measurements are highly sensitive to instruments and siting, which reduces the confidence in observed changes in high-latitude precipitation. A reference for this is Rasmussen R and co-authors, 2012, How well are we measuring snow?, Bull. Amer. Met. Soc., 93, 811-829. [Government of Australia]	Thanks, but this kind of information is specifically left out, the reader is pointed to AR4 for this discussion.
2-1631	2	41	5	41	6	Suggest changing to "spatially averaged precipitation in the tropics has increased over the last decade…" from "precipitation in the tropics has increased over the last". [Government of Canada]	we prefer to keep as is to make clear it is spatially averaged over the tropics.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1632	2	41	5	41	6	Consider qualifying this general statement by mentioning the high spatial and temporal variation in precipitation, as exemplified by reports of exceptional "once-in-a-century" droughts in the Amazon in 2005 and 2010 (Marengo JA et al. 2011. The drought of 2010 in the context of historical droughts in the Amazon region. Geophys Research Letters 38, L12703, doi:10.1029/2011GL047436, 2011; and Lewis SL et al. 2011. The 2010 Amazon drought. Science 331: 554). [Government of Canada]	This section has been extensively revised including new assessments of mid-latitude and high latitude precipitation changes in the NH and SH.
2-1633	2	41	5	41	6	why do you highlight this decadal scale change? [Xuebin Zhang, Canada]	We have enhanced the assessments statements to include more on changes in latitude bands.
2-1634	2	41	5	41	10	I suggest "…precipitation in the tropics has increased over the last 2 decades reversing the drying trend that occurred from the mid-1970s to the early 1990s". Note in general that the Smith et al. analysis shows muted trends owing to its interpolation technique. [David Parker, United Kingdom of Great Britain & Northern Ireland]	This section has been extensively revised including new assessments of mid-latitude and high latitude precipitation changes in the NH and SH.
2-1635	2	41	5			this is for land only and should be stated. [Kevin Trenberth, United States of America]	Done.
2-1636	2	41	6	41	7	What's the point of discussing a trend over a period of 111 years that covers both periods of La Nina and El Nino, both of which impact rainfall? The period of particular relevance is since 1950, but that period also contains El Nino and La Nina events, so that trend likewise cannot be attributed to human activity. [John McLean, Australia]	We now include more on changes since 1950.
2-1637	2	41	7	41	7	"increase in precipitation from 1900-2010" should be "increase in precipitation from 1900 to 2010" [Government of Japan]	Agree, will include this.
2-1638	2	41	8	41	8	"in the results for the early 20th century", what results? Do you mean latitude band precipitation or trend of it? [Xuebin Zhang, Canada]	This section has been extensively revised including new assessments of mid-latitude and high latitude precipitation changes in the NH and SH.
2-1639	2	41	9	41	9	I think you should make the implications of this lack of confidence in early 20th century precipitation more explicit (i.e., “Thus, confidence in those long-term trends is low”). [Ileana Bladé, Spain]	Accepted.
2-1640	2	41	9	41	10	The statement regarding the mid-latitudes of the SH rainfall change has the potential to cause confusion. Please consider adding that "seasonally stratified changes have been observed", for example autumn rainfall over southern Australia (Cai, W., Cowan, T. & Thatcher, M. Rainfall reductions over Southern Hemisphere semi-arid regions: the role of subtropical dry zone expansion. Sci. Rep. 2, 702; DOI:10.1038/srep00702 (2012).). [Government of Australia]	We have chosen not to look at seasonal changes owing to space constraints. This would add a large amount of text for the whole chapter.
2-1641	2	41	9	41	10	This definition of midlatitudes is somewhat misleading, as it more or less combines subtropical and midlatitude regions. At least in the southern hemisphere, opposite trends apply, and the "little evidence of ..change" reflects the cancelling out of trends between about 30-45 and 45-60. [Government of Australia]	There are numerous ways to define the mid-lats, this is our choice.
2-1642	2	41	10	41	10	The very dry last 10-15 years in the southern hemisphere subtropics is significant (see Figure 2.28). There are numerous references including for Australia (e.g., Timbal B and Fawcett R (In press) An historical perspective on South Eastern Australia rainfall since 1865. J. Climate.), and other Southern Hemisphere locations (e.g., Cai, W., Cowan, T. & Thatcher, M. Rainfall reductions over Southern Hemisphere semi-arid regions: the role of subtropical dry zone expansion. Sci. Rep. 2, 702; DOI:10.1038/srep00702 (2012).) [Government of Australia]	This is now included in an assessment statement.
2-1643	2	41	14	41	26	These two paragraphs are supposed to summarize AR4 and AR5 similarities and differences, but they are hard to compare because the first identifies changes over continents and the second discusses changes in latitude bands. More important, the are for vastly different time periods. [Dian Seidel, United States of America]	True, we decided to assess precipitation for larger regions this time, regional is contained in Chapt. 14.
2-1644	2	41	14	41	50	I urge you to rewrite this entire section which to me is one of the crucial ones in this chapter. Currently it is poorly written, too short (compared to the space devoted to other sections) and unsatisfying because it does not read like an assessment of the state of the art knowledge on precipitation changes. There is more emphasis on the regions with opposing short-term and long-term trends than on regions with consistent sustained trends throughout the whole period. One could argue that these are the trends that we can have	This section has been extensively revised including new assessments of mid-latitude and high latitude precipitation changes in the NH and SH. Both the century long, and since 1950 changes are assessed for the NH.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						most confidence in since the recent short-term 30-year trends may mostly reflect multi-decadal variability. Even if that's not the correct interpretation, these issues should be discussed. Also, how large are these trends compared to natural variability? How much variance do they explain? IRI has a nice website that calculates the contribution of long-term trends to the total variance and shows this contribution to be on the order of 10% or lower in most regions (much smaller than for temperature and consistent with the fact that most trends are not significant). This should be pointed out. Finally, if we do not trust precipitation data for the early part of the 20th century is it reasonable to show trends for the 1901-2009 period? Perhaps trends from 1950 onward would be more relevant and a better compromise. [Ileana Bladé, Spain]	
2-1645	2	41	14	41	50	The findings is this part are inconsistent with the conclusion in SPM-3, lines 31-32. First, nowhere medium confidence has been mentioned in this paragraph. In contrast the summary of this paragraph (line 45), low confidence for the whole period since 1900 has been concluded. Secondly, the year 1950 has only been mentioned when referring to AR4 results. All new analyses do not refer to 1950 in this text. Only one analysis mentions increases in precipitation (without likelihood of confidence statement), but refers to the period 1987 – 2006, and for ocean and land together (line 25). We therefore suggest to skip the following part of the conclusion in the SPM-3, lines 31-32: “prior to 1950 and medium afterwards”. If the authors have arguments to maintain the SPM conclusion, the text in paragraph 2.5.1.1 should include these arguments more clearly with a traceable justification of medium confidence. [Government of Netherlands]	This section has been extensively revised including new assessments of mid-latitude and high latitude precipitation changes in the NH and SH.
2-1646	2	41	15	41	16	The phrase "quite a number" is vague. [Government of United States of America]	Agree, will clarify.
2-1647	2	41	17	41	17	Delete the first "and", and the third. [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1648	2	41	21	41	21	For clarity, add comma to read "Since AR4, global satellite observations.." [Government of Canada]	Accepted.
2-1649	2	41	21	41	26	RSS, CMAP and GPCP have ocean precip to present day. Why are these not shown here? [Kate Willett, United Kingdom]	Ocean precipitation is assessed in Chapt 3.
2-1650	2	41	22	41	22	Suggest change to "indicate that spatially averaged precipitation has increased" from "indicate that precipitation has increased…". [Government of Canada]	Editorial, will be corrected
2-1651	2	41	24	41	24	precipitation → globally-averaged precipitation. Since this is a global result, shouldn't it be moved to the previous section? (that would also fix the problem I mention in the next comment) [Ileana Bladé, Spain]	Editorial, will be corrected
2-1652	2	41	24	41	25	The sentence referencing Wentz et al. deals with global changes so is not appropriate to the discussion of regional changes and should be moved to the previous subsection and compared with the much smaller changes shown by GPCP (Fig. 2.28 bottom panel; Adler et al. 2007). It is well known that globally, precipitation is determined by the energy balance and not water vapour, as is incorrectly implied by following on from the Wentz et al. result. The amplification of wet and dry regions is however consistent by increasing water vapour so the rest of this paragraph is reasonable. [Richard Allan, United Kingdom]	The Wentz references is for ocean precipitation. This part has been revised to include a summary from Chapt. 3 on ocean precipitation.
2-1653	2	41	24	41	26	The precipitation increases from 1987-2006 reported by Wentz et al. (2007) are largely due to multidecadal oscillations in tropical Pacific SSTs, and thus not related to GHG-induced global warming (see Gu and Adler, 2012, Climate Dyn.). Wenttz et al. (2007) compared these natural P changes with GHG-induced P changes and made incorrect conclusions. The statements in these lines make no sense. [Aiguo Dai, United States of America]	This section has been replaced with a summary from Chapt 3 that includes long term and shorter term changes in precipitation over the oceans.
2-1654	2	41	24			This section mentions precipitation trends from Wentz et al (2007) as large as humidity trends, but it remains inconsistent with other data shown in the report on precipitation trends as well as the physics: global surface latent heat release controls global precipitation not global specific humidity. So this is not something "consistent" with humidity changes. [Government of United States of America]	This section has been replaced with a summary from Chapt 3 that includes long term and shorter term changes in precipitation over the oceans.
2-1655	2	41	25	41	25	“These patterns”: sentence should be moved up one sentence up, after the sentence actually describing trends. [Ileana Bladé, Spain]	This section has been replaced with a summary from Chapt 3 that includes long term and shorter term changes in precipitation over the oceans.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1656	2	41	28	41	28	Is this figure final? It is not a good figure. The continental borders are too thick to clearly see the color shadings in convoluted regions (i.e., Europe) and the crosses that indicate statistical significance are impossible to see in print. Also, the white regions must correspond to regions with not enough data to compute trends but this is not indicated in the legend. Finally, I cannot exactly reproduce any of the CRU or GPCC plots in Fig. 29 (I was just trying to get a closer look since the details are so hard to see in the figure). Are you using the latest most recent versions of these datasets? Are you doing something not indicated in the legend? [Ileana Bladé, Spain]	We are replotting the maps for clarity.
2-1657	2	41	28	41	28	For the CRU dataset I assumed that CRU TS 3.1.01 is used but this is 0.5x0.5 resolution in its native format, not 5x5. Can you be clear about exactly which datasets and versions are used? It may also be worth thinking about whether some data-products are appropriate for long-term trend fitting? CRU TS was originally designed for using with climate models I believe and so may not be quite so appropriate for long-term trend fitting due to the infilling methods used. It may not be any worse that the other products though if homogeneity has not been assessed in those. [Kate Willett, United Kingdom]	We will clarify data set versions. We have not analysed the specifics of each data set, that is beyond the scope of an assessment.
2-1658	2	41	28	41	29	Stop avoiding the elephant in the room. Show what's happened in the last 15 years. [John McLean, Australia]	It is shown on the graph. Timescales of the last decade or so are discussed as part of the latitude bands plots.
2-1659	2	41	28	41	36	The message of this paragraph is unclear. For example the "although" at L32 does not make sense (unless the unstated intention is to point out that the trends have changed sign). The significant decline in the Sahel at L34 is contradicted directly at L49. The final sentence, asserting that there are many changes of sign between early and recent in Figure 2.29, is not very useful because the reader cannot quantify "many" without further help; give this help by quoting percentages of sign changes. [J. Graham Cogley, Canada]	These paragraphs are being revised. Although this is not a true contradiction since L49 refers to the very last few years.
2-1660	2	41	28	41	43	These two paragraphs are very poorly written. For instance, what does “although” (in line 32) refer to since you indicate agreement with AR4? The comparison between short and long-term trends is completely disordered, with the text skipping between statements referring to the fact that the trends are opposite (line 35), same-signed (implied by line 38), and again opposite (line 42). And why do you need to refer to the areas that showed significant long-term trends in AR4 to talk about regions with opposing short-term trends (line 35)? Finally, are all these results robust (significant) and common to all datasets (again, the figure is so low-quality it is hard to see some of the features alluded to). [Ileana Bladé, Spain]	Agree the paragraphs need to be revised for clarity.
2-1661	2	41	28	41	43	In eastern China, annual and summer (rainy season) precipitation did not show significant long-tern changes over the past 100 years, in spite of the fact that, in more recent decades, North China witnessed a continuous drying trend, and the Yangtze river basin and southeastern China a non-significant decreasing trend (Ren G.Y., H.B. Liu, Z.Y. Chu, L. Zhang, X. Li, W.J. Li, Y. Chen, G. Gao, Y. Zhang, 2011, Climate change over eastern China and implications for South-North Water Diversion Project, Journal of Hydrometeorology, 12 (8): 600-617. DOI: 10.1175/2011JHM1321.1; Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977). [Guoyu Ren, China]	Thanks but we are only assessing annual and large scale precipitation changes.
2-1662	2	41	28			Figure 2.29 - it would be good if they included the other periods: 1901-1950, 1951-2009 [Government of Brazil]	We have included 1951-present.
2-1663	2	41	29	41	29	Delete the word 'and' after GPCC [Government of Australia]	Editorial, will be corrected
2-1664	2	41	30	41	30	"each native data set grid resolution". Many of the datasets are available at multiple resolutions, please specify which ones were used. [Geert Jan van Oldenborgh, Netherlands]	We will specify the versions.
2-1665	2	41	31	41	32	"… and SH. Compared to the same figures in AR4, statistically…" [David Parker, United Kingdom of Great Britain & Northern Ireland]	This sentence is a bit garbled and will be revised.
2-1666	2	41	31	41	33	It is not clear what this sentence is trying to say. It appears that some text was misplaced. [Government of United States of America]	This sentence is a bit garbled and will be revised.
2-1667	2	41	32	41	32	Insert "level" or "scale" after "grid box" (presumably). [J. Graham Cogley, Canada]	This sentence is a bit garbled and will be revised.
2-1668	2	41	32	41	33	Text here doesn't make sense. Do you mean "and compared to the same figures in AR4, statistically	This sentence is a bit garbled and will be revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						significant trends occur in most of the same areas. The same can be said comparing results for the three data sets." ? [James Renwick, New Zealand]	
2-1669	2	41	33	41	34	The precipitation decline over the Sahel seems less pronounced than in the 1901-2005; mention that. [Ileana Bladé, Spain]	Not sure what this means, decline.
2-1670	2	41	33	41	34	The statement that "The decrease in annual precipitation in the Sahel region …" is not supported by the figure and/or a specific reference and should be deleted . If a reference is supplied, suggested change to "The previously reported decrease in annual precipitation in the Sahel region of Africa during 1901-2005 continues to show a significant decline in the updated analysis for 1901-2009". [Government of Canada]	Decline, this statement refers to both the time series and map of trends not just Fig. 2.28 (time series).
2-1671	2	41	33	41	34	It is not clear what "continues" means here. Does it mean AR4 vs. AR5? If so, for the same time periods, or for an extended time period in AR5 relative to AR4? Or does it mean 1901-2009 vs 1979-2009? That would not be a "continuation." This needs more careful wording. [Government of United States of America]	Yes it refers to what appeared in AR4, will clarify.
2-1672	2	41	33	41	34	Why is the Sahel mentioned, but many other more significant features (eg West Africa coast, Western Australia, southeastern S America, eastern US) are not? [Geert Jan van Oldenborgh, Netherlands]	The Sahel is of special interested owing to the long-term drought issues there.
2-1673	2	41	34			Would read better if "a significant decline" were to be replaced by "significant". The opening of the sentence already refers to a decrease. [Adrian Simmons, United Kingdom]	Agree, does sound better.
2-1674	2	41	35	41	35	Suggest changing to "statistically significant trends for the period 1901-2005 in AR4..." from statistically significant long-term trends in AR4…" [Government of Canada]	Agree, this will be revised.
2-1675	2	41	38	41	43	Change the initial "For the shorter-period maps (1979-2009), many" to "Many". As in the previous paragraph, the aim of this paragraph, to judge by its last sentence, seems to be to draw attention to changes in the signs of trends, but the text fails to say so. [J. Graham Cogley, Canada]	Agree, this will be revised.
2-1676	2	41	38	41	43	According to in-situ rainfall data over the whole of the Amazon Basin, there are a significant diminution for the 1975-2003 period (Espinoza et al., 2009 Int. J. of Clim) [Jhan Carlo Espinoza, Peru]	Thanks, but we do not assess smaller region changes.
2-1677	2	41	38	41	43	Precipitation is highly variable in amount and location, and extremes can easily distort any trends. For example, if you included 2010 and 2011 as you do with other climate data, the recent trend might differ markedly because of the shift to La Nina conditions. [John McLean, Australia]	Thanks for the comment, no action requested.
2-1678	2	41	40	41	40	Some quantification is required. GPCC have more station numbers, but does this translate to better coverage. Some of this relates to the degree of spatial infilling - how far does this extend for the precipitation datasets. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Perhaps, but this is the subject of a new, and needed research project, not an assessment.
2-1679	2	41	40	41	41	Since, at the beginning of this section, you do not summarize earlier AR4 results for the 1979-2005 period, it does not seem right to state that the Sahel “continues to show” an increase although “not as strong as in AR4”. [Ileana Bladé, Spain]	Will clarify that this is in reference to AR4.
2-1680	2	41	41	41	41	This section should be rechecked if precipitation trends are updated. 2010-12 have been wet years in the Sahel by recent standards which may affect the trends reported here. [Government of Australia]	We have recalculated all trends and included a new table.
2-1681	2	41	41	41	41	Regarding the phrase "continues to show a shorter-term increase, although not as strong as in AR4". Please specify the short-term period used in AR4 analysis. [Government of Canada]	will clarify the time period.
2-1682	2	41	42	41	43	To what extent does a shift in the sign of these trends represent decadal variability which is common with precipitation time series. [David Sauchyn, Canada]	Could be decadal variability, probably is but since we include these two periods it is worth pointing out the differences.
2-1683	2	41	45	41	46	Do you mean low confidence in the observed changes in the early 20th century or in the time series in the early 20th century? [Xuebin Zhang, Canada]	Both. Owing to a lack of spatial coverage, esp in higher latitudes, the early 20th century increase is uncertain.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1684	2	41	45	41	47	This sentence summarizes findings on global precipitation, but there was no clear previous reference to analysis of global (land+ocean) precipitation. [Government of United States of America]	This refers to land only, will clarify.
2-1685	2	41	45	41	47	This may be a true statement, but it is not a summary of the section. [Dian Seidel, United States of America]	This statement has been substantially revised.
2-1686	2	41	46	41	46	Capital 'C' required for 'century' [Peter Burt, United Kingdom]	Editorial, will be corrected
2-1687	2	41	46	41	48	Suggest the lack of a clear picture of global precipitation is due to large and opposing regional changes which counterbalance. There are however statistically significant regional changes... [European Union]	True, and this will be clarified.
2-1688	2	41	47	41	47	“Where long-term data are available”: what does this mean? Is this related to the non-white areas in the figure (not explained in the legend) ? [Ileana Bladé, Spain]	yes, refers to the areas that have long term data and thus can have a time series to analyze.
2-1689	2	41	47	41	47	I suggest mentioning here two additional and important reasons for this uncertainty, namely the discrepancies between observational databases and the difficulties in measuring precipitation compared with the estimation of other variables such as temperature. I suggest rewording line 47 as "of the globe, to the challenges in measuring global precipitation (Kucera et al. 2012, Tapiador et al. 2012a, 2012b), and to the internal discrepancies between observational databases (Tapiador 2010)." References: Tapiador, F.J., Turk, J., Petersen, W., Hou, A.Y., García-Ortega, E., Machado, L.A.T, Angelis, C.F., Salio, P., Kidd, C., Huffman, G.J. and de Castro, M. 2012a. Global Precipitation Measurement: Methods, Datasets and Applications. Atmospheric Research, 104–105, pp. 70–97. Kucera, P.A., Ebert, E.E., Turk, F.J., Levizzani, V., Kirschbaum, D., Tapiador, F.J., Loew, A., Borsche, M., 2012b. Precipitation from Space: Advancing Earth System Science. Bulletin of the American Meteorological Society (in press) http://dx.doi.org/10.1175/BAMS-D-11-00171.1 Tapiador, F.J., 2010. A Joint Estimate of the Precipitation Climate Signal in Europe using Eight Regional Models and Five Observational Datasets. Journal of Climate, 23, 7, 1719-1738. [FRANCISCO J. TAPIADOR, SPAIN]	Thank you for the references. However we declined to include a large section discussing issues with data owing to space limitations and the fact that much of this is included in AR4 (to which the reader is pointed).
2-1690	2	41	47	41	48	“compared to AR4” seems to indicate a discrepancy with AR4 when what you mean is consistent with AR4. I suggest changing this sentence to “namely, a continuation of the increases … and decreases … seen in AR4”. [Ileana Bladé, Spain]	Disagree, this is a direct comparison to AR4 results.
2-1691	2	41	47	41	48	Please revise the wording. Not all regions with long-term data available saw a significant change in precipitation. [Guoyu Ren, China]	Will clarify this statement.
2-1692	2	41	48	41	48	The statement of "decreases in lower latitudes" is too simple and confusing. Based on the descriptions of line 5-6, 18-19 and 22-23 on the page (2-41), the precipitation change in lower latitudes is quite complicated. [Chia Chou, Taiwan, ROC]	This refers to the latitudinal bands in Fig. 2.28, we agree that for smaller regions it is more complicated.
2-1693	2	41	48	41	49	See previous comment. In the case of the western hemisphere (western US, South America), this change in trend sign could be in response to Pacific SST forcing. [David Sauchyn, Canada]	Could be true, but that is for the attribution chapter.
2-1694	2	41	48	41	50	A difference in linear-trend sign between time period A and time period B, where B is contained in A, is not a "change" in sign of trend, nor is consistency of sign indicative of a "continuation." Furthermore, one cannot use data from the past (which is all we have) to say--in the present tense--that any trend currently continues. [Government of United States of America]	Disagree, this indicates a changepoint in the time series. A longer term period can have one sign and a shorter period withing the longer period can have a different sign.
2-1695	2	41	48	41	50	What "more recent period"? I'm guessing that you are speaking of 1979-2009 and that you are still avoiding the elephant? [John McLean, Australia]	Yes, the 1979-2009 period.
2-1696	2	41	49	41	50	Only 2 grid boxes in one dataset and none in the other show drying in the southwestern US over 1901-2009. Other trends are much more significant. Please replace by a more meaningful example. [Geert Jan van Oldenborgh, Netherlands]	will consider request in revising.
2-1697	2	41	50	41	50	I'm not sure it is correct to say that the southwest USA “continues to get drier”. The GPCC trend seems small and not very widespread for the 1901-2009 period (likewise in the much higher quality AR4 figure). [Ileana	will consider request in revising.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Bladé, Spain]	
2-1698	2	41	52	42	30	The expectations for snow are that there is an increase in midwinter continental extratropical regions because it is warmer and the air holds more moisture, but the season is shorter at each end. Nothing in this analysis allows any of these contrasting aspects to be illuminated. It is poorly done and should be redone. [Kevin Trenberth, United States of America]	That may be true but there is very little in the observed literature that supports this.
2-1699	2	41	53	41	53	In addition to Figure 2.29 a graph showing percentage change per decade scaled to average annual precipitation would be more informative [European Union]	Thanks for the suggestion, we like the figure as is.
2-1700	2	41	55			It is striking that there is not a sub-section devoted to the trends in seasonal rainfall. The sub-section 2.5.1.2 is on an annual basis and the sub-section 2.5.1.3 ('Changes in snowfall') highlights the winter precipitation. It is necessary to add a new sub-section devoted to changes on seasonal precipitation. This is crucial for dry regions, where a seasonal shift on the calendar occurrence of precipitation can modify fluvial regimes, groundwater recharge and water availability strongly, increasing the damages over the crops, the scarcity of water resources, etc. [JAVIER MARTIN-VIDE, SPAIN]	We chose not to include seasonal trends owing to space limitations.
2-1701	2	41	55			Linked with the previous comment, there is literature enough about this subject. In the Mediterranean region there are several articles on changes of seasonal rainfall regimes or on trends of seasonal precipitation. See for example: de Luis, M., Brunetti, M., Gonzalez-Hidalgo, J.C., Longares, L.A. and Martin-Vide, J.(2010): Changes in seasonal precipitation in the Iberian Peninsula during 1946–2005. Global and Planetary Change, 74, 27-33. [JAVIER MARTIN-VIDE, SPAIN]	See answer to 1701.
2-1702	2	41	55			In the article cited, using 2670 complete and homogeneous monthly precipitation series for peninsular Spain, a clearly change on the seasonal precipitation maximum is detected. Spring was the dominant precipitation season in 36.1% of the territory in the 1946–1975 period, whereas in the 1976–2005 period, it is the dominant one in less than half (15.1%) the territory. This contrasts with areas where autumn constituted the main precipitation season, which increased from 10.8% (restricted to the Mediterranean coast) to 41.4% of the territory. [JAVIER MARTIN-VIDE, SPAIN]	Thanks for the comment, no action requested.
2-1703	2	42	1	41	1	A cross-reference to chapter 4, where changes in snow depth are reported, would be useful here. [Government of Australia]	Agree, will include this.
2-1704	2	42	1	42	7	Note that snowfall events and snowfall are two different things. Are the two phases used here by purpose? [Xuebin Zhang, Canada]	We clarify this to snowfall.
2-1705	2	42	1	42	8	This paragraph needs at least a couple of citations. First, for increases in high-latitude precipitation I would recommend citing Rawlins et al., 2010,Journal of Climate, "Analysis of the Arctic System for Freshwater Cycle Intensification: Observations and Expectations." Also, I think a citation is merited in the last sentence of the paragraph, although I'm not sufficiently familiar with the literature in that area to recommend one. [Tamlin Pavelsky, United States of America]	This paragraph summarizes results from AR4, no additional reference is needed in this paragraph.
2-1706	2	42	1			Section 2.5.1.3: The summary at L29-30 is inadequate, consisting of a repetition of the last sentence of the first paragraph. The second and third paragraphs focus on change in the ratio of rainfall to total precipitation, but there is no summary assessment of this important quantity. [J. Graham Cogley, Canada]	This summary has been revised, added more material.
2-1707	2	42	2	42	2	precipitation → snowfall (for clarity). Also, could you include a bit of background/framing on this topic since the impact of warmer temperatures on snowfall can be anticipated to be complex (multiple effects). [Ileana Bladé, Spain]	This is a summary of AR4 and much of the framing is there.
2-1708	2	42	5	42	5	, where warming lead → and warming lead [Ileana Bladé, Spain]	agree, will change.
2-1709	2	42	5	42	6	These two issues - near-zero climatological temperatures and shift of seasons - are semi-independent ideas. Spring can come earlier even in a region where temperatures never approach the freezing point. [Dian Seidel, United States of America]	Thanks for the comment, no action requested.
2-1710	2	42	7	42	8	This last sentence is not supported by a reference and may be redundant with the next paragraph (line 10-15).	We have added some context to this statement.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Considering deleting this sentence. [Government of Canada]	
2-1711	2	42	10	42	15	Are these two studies mostly in agreement? It is a bit hard to tell given the different regional names. If they are in qualitative agreement say so and explicitly point out any discrepancy. [Ileana Bladé, Spain]	yes, more rain less snow means less overall snow.
2-1712	2	42	20	42	20	Need to add the sentences. “Gurung et al. (2011), using satellite data, find a decreasing trend in the snow cover area in Hindukush, in line with the earlier similar trend during 1990-2000. The eastern and western region has an increasing trend of 10% to 12%, while the central region has a declining trend of 12% to 14% for the decade. The authors, however, attribute the changes to interannual circulation changes.”. [Government of India]	Declined. Snow cover is assessed in the Cryosphere chapter.
2-1713	2	42	20	42	20	Valuable to add in to this survey of regional studies the recent investigation of precipitation breakdown in the Arctic Ocean and Canadian Archipelago by Screen, J. A., and I. Simmonds, 2012: Declining summer snowfall in the Arctic: Causes, impacts and feedbacks. Climate Dynamics, 38, 2243-2256. [Ian Simmonds, Australia]	Will consider adding some context here.
2-1714	2	42	21	42	21	in Switzerland : move to after “and rainfall days” or after “since 1961” [Ileana Bladé, Spain]	decline, editorial
2-1715	2	42	22	42	23	What is a trend in a pattern of variability? How much did this pattern explain and what was the trend? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Agree, will clarify.
2-1716	2	42	24	42	24	Dodgey bracketting around van Ommen and Morgan. [Kate Willett, United Kingdom]	Thanks, Endnote problem.
2-1717	2	42	24	42	25	Mentioning this teleconnection here seems unnecessary and the period for this increase is not indicated. [Ileana Bladé, Spain]	Decline, does provide some context.
2-1718	2	42	24			the sentence begins with “(“ [David Sauchyn, Canada]	Thanks, Endnote problem.
2-1719	2	42	29	42	30	This sentence is a poor summary of the corpus of 2.5.1.3. and it is probably wrong. It is at least necessary to add that it is only true in regions with frequent temperatures close to the greezinf point. [Government of France]	This is revised. However, the "probably wrong" statement is pure conjecture.
2-1720	2	42	29	42	30	This sentence almost begs more questions than it answers, especially following the statement about uncertainty in Antarctic snowfall, which probably accounts for a fair fraction of the global snowfall total. It also says nothing about the time period under consideration. As a summary statement, it leaves a lot to be desired. [Dian Seidel, United States of America]	There is not much about Antarctic snowfall in the literature. Also I would be surprised if Antarctic snowfall accounts for a large fraction of global snowfall since Antarctica is essentially a desert.
2-1721	2	42	32	43	18	there is no a figure to specify the changes of the stream-flow or runoff, it is better to add a map to show the changes and distribution of the global runoff. [Jianting Cao, China]	Reject. Page limitation and incompletet information
2-1722	2	42	32	43	18	The arguments on global runoff should be based upon inferences from observations of unregulated and unimpaired catchments around the globe rather than those from regulated rivers or from modelled outcomes (e.g. as those from Dai et al., 2009). The references Stahl et al. (2010) and Stahl and Tallaksen (2012) do account for unregulated rivers but only in the European context. The report will benefit from work of similar nature in other continents. See study by MacMahon et al., 2007; Journal of Hydrology 347, 243-259, Part 1, 2 & 3 that describes stream flow trends from a set of 1221 global rivers. Also see the work of Milly et al. (2005; Nature 438, 347-350, 17 Nov 2005; doi:10.1038/nature04312) which describes trends from observations across 165 river basins and compare these to those from 12 different GCMs. They argue 10-40% increases in runoff in eastern equatorial Africa, the La Plata basin and high-latitude North America and Eurasia, and 10-30% decrease in runoff in southern Africa, southern Europe, the Middle East and mid latitude western North America. More importantly they claim that on the basis of observations from 165 basins and their analysis that a significant part of the twentieth-century hydro-climate as inferred from stream flow responses was externally forced and that larger changes can be expected in the coming decades. [Government of Australia]	Accepted.
2-1723	2	42	32	43	18	Section 2.5.2 Streamflow and Runoff: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted.
2-1724	2	42	32			Section 2.5.2. One can understand the space limitations but the emphasis on large river systems, which are often regulated, does not capture the often complex regional trends. For example, Monk et al 2011, Trends in	Reject. Page limitation and incompletet information

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Indicator Hydrological Variables for Canadian Rivers (Hydrological Processes 25: 3086-3100) examine the often subtle regional differences in the subannual changes in various streamflow metrics. Doubtless there are similar analyses for other regions. Please review. [Government of Canada]	
2-1725	2	42	32			Section 2.5.2: The final sentence of this section should perhaps come earlier, near the start of the second paragraph. The first sentence of the second paragraph makes an important point, but seems inconsistent with the contents of the paragraph. Milliman et al. (2008) is perhaps the keystone reference for this section and has much more to say than what is mentioned-- specifically, on the consistency, and absence thereof, of precipitation and runoff trends. This is relevant both to reduction in streamflow from consumptive water use (which appears not to be mentioned explicitly in this section) and to the high-latitude discussion. Also relevant to the latter are papers by Zhang et al. (2012, doi:10.1038/NCLIMATE1631) and Muskett and Romanovsky (2009, doi:10.1088/1748-9326/4/4/045009). [Government of United States of America]	Noted.
2-1726	2	42	32			Two recent studies in the UK (Hannaford and Buys, 2012) and in France (Giuntoli et al., 2012) found that the sign of the temporal trends in natural streamflows varies with the period studied. Moreover, Giuntoli et al. (2012) found significant (and stable over different periods) correlations between median to low flows and the Atlantic Multidecadal Oscillation (AMO), which shows long quasi-periodic oscillations. It is therefore of the uttermost importance not to overinterpret recent trends in streamflow as a direct result of recent climate change. - Hannaford and Buys (2012) Trends in seasonal river flow regimes in the UK. Journal of Hydrology, 475, 158-174 - Giuntoli et al. (2012) Low flows in France and their relationship to large scale climate indices. Journal of Hydrology, accepted [Jean-Philippe Vidal, France]	Accepted
2-1727	2	42	34	42	44	Lengthy repetition of AR4 conclusions here - are all the regional findings from the AR4 needed here? Why not just provide the AR4 conclusion (last sentence) and then go on to give your new assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
2-1728	2	42	34	43	18	The following work is worth being considered and mentioned: http://orbi.ulg.ac.be/handle/2268/66197 [Benjamin Dewals, Belgium]	Rejected. Not follow IPCC role to cite.
2-1729	2	42	39	42	40	Consider citing original reference regarding decreases in streamflow of many Canadian river basins during the last 30-50 years (cited in AR4 as: Zhang, X., et al., 2001b: Trends in Canadian streamflow. Water Resour. Res., 37, 987–998) [Government of Canada]	AR4 part removed.
2-1730	2	42	41	42	41	more extreme than what? Perhaps you can just delete “more” [Ileana Bladé, Spain]	editoral
2-1731	2	42	43	42	44	It is hard to understand how AR4 came to this conclusion given the ambiguous evidence discussed in the sentences that precede this statement. [Ileana Bladé, Spain]	Noted.
2-1732	2	42	46	42	47	It might be noted that many if not most large rivers, especially those for which a long-term history exists, have been impacted by human influences such as dam construction and land use changes, so results must be interpreted with caution. [Robert Kandel, France]	Accepted.
2-1733	2	42	46	42	57	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]	Noted. Long term trend for discharge extreme is cited.
2-1734	2	42	49	42	49	Consider removing the word "only" in the phrase "only about one-third of the top 200 rivers". [Government of Canada]	Accepted
2-1735	2	42	49	42	50	Not clear what is meant by "top 200 rivers". How are these defined/selected? [Thomas Stocker/ WGI TSU, Switzerland]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1736	2	42	49	42	52	What "recent widespread drying trend" ? At the end of the sentence you repeat "where precipitation has decreased" except now you specify this applies to those particular regions. This sentence should be rewritten. [Ileana Bladé, Spain]	Noted
2-1737	2	42	50	42	50	It would be more informative to indicate which of the listed rivers showed significant upward trends and which showed significant downward trends. [Government of Canada]	Noted
2-1738	2	42	51	42	51	Not clear what quantity these 'downward trends' refers to - discharge? - flow height? [Thomas Stocker/ WGI TSU, Switzerland]	Noted
2-1739	2	42	52	42	52	Can you clarify what is meant by the 'recent widespread drying trend'? Do you mean RH? Precip? Other? Do you mean recent as in last few years or last few decades? Perhaps link to the section where this is discussed. [Kate Willett, United Kingdom]	Noted
2-1740	2	42	52	42	55	The meaning of "recent widespread drying trend" is unclear. Apparently it does not mean decreasing streamflow, since that is what being called consistent. Does it refer to precipitation or to some other water-related observation? [Government of United States of America]	Noted
2-1741	2	42	52	42	55	What is "the effect of surface warming?" How is it consistent with observed decreases in streamflow? [Government of United States of America]	Noted
2-1742	2	42	54	42	54	Citation correction (Dai et al., 2009) [LUCILA CANDELA, Spain]	editoral
2-1743	2	42	54			typing of citation should be (Dai et al., 2009) [David Sauchyn, Canada]	editoral
2-1744	2	42	55	42	55	However ->why is this a contradiction ? Can´t you make a more general statement about the fact that there is a generally consistent relationship between streamflow and precipitation changes (with exceptions?). Or is that not the case? It is hard to tell from this and the next paragraph (page 43). They both seem confusingly written to me with some contradictory statements. Aren´t the downward trends in the global high-latitude ocean (page 43, line 5) inconsistent with the increase in precipitation in northern high latitudes (Fig. 2.28) unlike what is stated in lines 6-7? [Ileana Bladé, Spain]	Noted
2-1745	2	42	55	42	55	"latter half". [J. Graham Cogley, Canada]	editoral
2-1746	2	43	1	43	1	resulted -> resulting [Ileana Bladé, Spain]	editoral
2-1747	2	43	1	43	2	Check grammar "At the higher latitudes, increasing winter base flow and mean annual stream flow resulted from possible permafrost thawing were reported in Northwest Canada" [European Union]	editoral
2-1748	2	43	1			Since permafrost is simply frozen ground, thawing does not necessarily generate runoff. This requires melting of ground ice. [David Sauchyn, Canada]	Noted
2-1749	2	43	2	43	2	This paragraph refers to "higher latitudes" but it is not made clear that the entire previous paragraph refers to mid-to-low latitude (it says so in line 53 but not in sufficiently clear terms). So, does "global" in line 5 mean "global high latitude ocean"? [Ileana Bladé, Spain]	Noted
2-1750	2	43	2	43	2	"resulting", not "resulted". [J. Graham Cogley, Canada]	editoral
2-1751	2	43	2			"resulted" should be "resulting". [Adrian Simmons, United Kingdom]	editoral
2-1752	2	43	4	43	6	Sentence beginning "For ocean basins other than the Arctic .." requires a reference for the statement that the downward trend in the Pacific is -9.4 km3/yr [Government of Canada]	Noted.
2-1753	2	43	5	43	5	Maybe some clarification is needed for non-hydrology experts such as myself: are streamflow, runoff and discharge equivalent terms? [Ileana Bladé, Spain]	Accepted. We included them in Grossary.
2-1754	2	43	7	43	11	I believe this sentence should be rewritten as follows: "However, for the Arctic drainage areas, upward trends in streamflow are not always accompanied by increasing precipitation. . . and recent surface warming and associated downard trends in snow cover, soil ice content, and changes in evaporation over the northern high	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						latitudes may also have contributed. . ." There are many Arctic river basins in which precipitation trends match discharge trends, and the consenus in the literature is that precipitaiton increases are important in driving discharge increses in many areas (see, for example, Pavelsky and Smith, Journal of Geophysical Research, 2006). Also, section 2.5.1 discusses increasing Arctic precipitation at length, and as written this sentence seems to contradict that work. [Tamlin Pavelsky, United States of America]	
2-1755	2	43	7	43	11	Precipitation measurements are sparse and exhibit large cold-season biases in the Arctic drainage areas. There would be large uncertianties using these data to investigate their influence on streamflow. A recent study, using a mass-corrected reanalysis data based on well observed wind and humidity, indicates that the upward trend of the Eurasian Arctic river discharges has been decisively driven by the atmospheric moisutre convergece, i.e., the precipitation minors evapotransporation (Zhang et al. 2012). This also implies a net increase of precipitation. [Xiangdong Zhang, United States of America]	Noted
2-1756	2	43	7	43	11	Zhang, X., J. He, J. Zhang, I. Polaykov, R. Gerdes, J. Inoue, and P. Wu, 2012: Enhanced poleward moisture transport and amplified northern high-latitude wetting trend. Nature Clim. Change, doi: 10.1038/NCLIMATE1631. [Xiangdong Zhang, United States of America]	Accepted.
2-1757	2	43	8	43	18	The writing should be improved: Undoubtedly the increasing streamflow in Siberia comes from somewhere and most likely precipitation: thawing permafrost is an order of magnitude too small. This means the actual preicpitation data is inadequate and this should be the conclusion. Also please state the reasons why earlier analyses were wrong: it is explained in Dai et al 2009 as due to assumptions about missing data and infilling. The newer analysis is clearly superior. [Kevin Trenberth, United States of America]	Accepted
2-1758	2	43	9	43	9	The "although" is not appropriate. Delete it and begin a new sentence with "Recent …". [J. Graham Cogley, Canada]	Accepted
2-1759	2	43	13	43	13	The reference should be Stahl et al 2010 - see next comment [European Union]	editoral
2-1760	2	43	13	43	13	Change “Stahl and Tallaksen (2012)” into “Stahl et al. (2012)” [Chong-Yu Xu, Norway]	editoral
2-1761	2	43	13	43	15	The references Stahl et al. (2010, 2012) should be updated. They are now published in HESS. [Jean-Philippe Vidal, France]	Noted
2-1762	2	43	13	43	18	From hydrological point of view a 50 years are short in length and hydrograph may not reflect streamflow pattern, specially in southern areas where flow is characterised by extreme variability (extreme events). To be conclusive a longuer data set is needed to decrease uncertainty, and this paragraph needs to be nuanced. Include follwing citations: ACCUA (2011) Adaptacions al cànvi climatic en l'us de l'aigua. Catalunya-Caixa, Obra Social. 221 pp [LUCILA CANDELA, Spain]	Noted.
2-1763	2	43	13	43	18	It should be emphasized that climate change and human activities have not only resulted in different changing trends in hydrological processes, but also caused a non-stationary relationship between the annual precipitation and streamflow (Zhang et al. 2011). ((reference: Zhang, ZX, Chen, X, Xu, C-Y, Yuan, LF, Yong, B, Yan, SF, 2011. Evaluating the non-stationary relationship between Precipitation and Streamflow in Nine Major Basins of China during the past 50 years. Journal of Hydrology 409, 81-93. [Chong-Yu Xu, Norway]	Noted.
2-1764	2	43	14	43	14	What are *near-natural* streamflow records? [Ileana Bladé, Spain]	Noted
2-1765	2	43	15	43	15	"regionally". [J. Graham Cogley, Canada]	Noted
2-1766	2	43	18			A clear summary statement should be added here. After the lengthy repetition of the AR4 findings at the beginning of this section, the reader is now left completely unclear what the conclusions of this updated assessment are. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
2-1767	2	43	20	43	31	This section could be significantly expanded, as soil moisture resarch has made significant progress in the last years. First, the original soil moisture database by Alan robok was complemented by the International Soil Moisture network (http://www.ipf.tuwien.ac.at/insitu/) (Dorigo et al., 2011). Further, a novel longterm soil moisture dataset has been released as part of the ESA Climate Change initative. The first version of the dataset is publically available (http://www.esa-soilmoisture-cci.org/). Relevant papers on the data product can be found here: http://www.esa-soilmoisture-cci.org/node/137 In particular Dorigo et al. (2012) analyzed the	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						longterm trends in soil moisture from satellite estimates. REFERENCES: * Dorigo, W.A., Wagner, W., Hohensinn, R., Hahn, S., Paulik, C., Xaver, A., Gruber, A., Drusch, M., Mecklenburg, S., van Oevelen, P., Robock, A., and Jackson, T., Jackson, T. (2011), "The International Soil Moisture Network: A data hosting facility for global in situ soil moisture measurements", Hydrology and Earth System Sciences 15 (5), pp. 1675-1698. * Dorigo, W.A., Wagner, W., Hohensinn, R., Hahn, S., Paulik, C., Xaver, A., Gruber, A., Drusch, M., Mecklenburg, S., van Oevelen, P., Robock, A., and Jackson, T., Jackson, T. (2011), "The International Soil Moisture Network: A data hosting facility for global in situ soil moisture measurements", Hydrology and Earth System Sciences 15 (5), pp. 1675-1698. [Alexander Loew, Germany]	
2-1768	2	43	20			Section 2.5.3: The relative weight given to AR4 in this paragraph should be diminished. TRMM moisture data, as discussed for example by Jung et al. 2010 (cited at L46), should be assessed. [J. Graham Cogley, Canada]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1769	2	43	20			Section 2.5.3: This section on soil moisture seems quite short. Consider citing global trends in modelled soil moisture. For example, the declining trend for 1948-2003 reported in Figure 6 of Fan, Y and van den Dool, H (2004) Climate Prediction Center global monthly soil moisture data set at 0.5 <degrees> resolution for 1948 to present, J. Geophys. Res. 109 D10102, doi:10.1029/2003JD004345 [Government of Canada]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1770	2	43	20			This short 'soil moisture' section lacks any clear assessment, or concluding AR5 statement. The last sentence mentions "long term changes in soil moisture related to drought", but provides no further detail on what these changes are. [Thomas Stocker/ WGI TSU, Switzerland]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1771	2	43	22	43	31	Section 2.5.3 Soil Moisture: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1772	2	43	22	43	31	Section 2.5.3 Surely more can be said about soil moisture - see Soil Moisture section in BAMS 'State of the Climate in 2011' (Blunden et al. 2012) lead by Richard de Jeu - de Jeu, R. A. M., W. A. Dorigo, R. M. Parinussa, W. W. Wagner and D. Chung, 2012: [Global Climate] Soil Moisture [in "State of the Climate in 2011"]. Bulletin of the American Meteorological Society, 93 (7), S30-S34. Also see page 44 lines 20-22 of this Chapter of IPCC. [Kate Willett, United Kingdom]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1773	2	43	22	43	31	The recent diagnostic analysis mentioned above also suggests that the upward increasing rates of atmospheric moisture conveged into the Eurasian river basins is larger that the increasing rate of river discharges, indicating an increase in soil moisture. [Xiangdong Zhang, United States of America]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1774	2	43	25	43	31	Please indicate which decades. This paragraph ends oddly, with no results from LSM soil moisture simulations. [Ileana Bladé, Spain]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1775	2	43	27	43	31	I think that a short summarizing (from section 2.6) sentence on the trend of soil moisture should be included here. [Claudio Cassardo, Italy]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1776	2	43	27	43	31	Doubtful simulations since at soil hydrology is not well simulated in such models and plant-soil interactions are too simplified. Skip this part, not necessary and provides no additional information [European Union]	This section has been removed since little has happened since AR4 examining longer term changes in soil moisture. The rest is in the extremes/drought section.
2-1777	2	43	33	43	33	OK, this is presented here as evapotranspiration from the earth, but does it include evaporation from soil and plants? It would be useful to have some clarification as to what evapotranspiration has been assessed. [Dora	The section includes pan evap and some theoretical discussion on why we see the trends.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Marinova, Australia]	
2-1778	2	43	33	44	22	Section 2.5.4 Evapotranspiration Including Pan Evaporation: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Thank you.
2-1779	2	43	33	44	22	This section confuses pan evaporation with actual evaporation or evapotranspiration. The former depends on the environment (such as how dry the air is flowing over the pan) and energy available, but not moisture availability which is generally critical for ET. In a drought, pan evaporation increases because of dry air but evaporation decreases because of no moisture. This is not dealt with here at all well. An evaluation of ET was given by Mueller, B., et al. (2011), Evaluation of global observations-based evapotranspiration datasets and IPCC AR4 simulations, Geophys. Res. Lett., 38, L06402, doi:10.1029/2010GL046230. and should be discussed further. [Kevin Trenberth, United States of America]	The diff between pan evap. and AET etc. is clarified.
2-1780	2	43	35	43	35	"decreasing trends over recent decades"? State the decades. Why no report of what's happened in the last few years? [John McLean, Australia]	Time periods clarified. There is no literature for the last few years.
2-1781	2	43	35			Section 2.5.4: Statements are confusing that pan evaporation "declines in most regions" yet on global scale evapotranspiration "increased". It is recognized that this ambiguity is likely due to sparse regional sampling of pan evaporation, but it would be helpful to clarify this conflict. [Government of United States of America]	will clarify
2-1782	2	43	35			Section 2.5.4 - This section would benefit from an interpretation of the various observed trends in pan evaporation and evapotranspiration using the concepts articulated in Roderick et al., 2009, including Bouchet's Complementarity Relationship. They point out the importance of distinguishing between energy-limited and water-limited conditions, and that the Earth's land surface is roughly divided in half between those two. Under energy-limited conditions, declining pan evaporation will mean declines in actual evapotranspiration (and increasing runoff and/or soil moisture). Under water-limited conditions, declining pan evaporation will be accompanied by increasing actual evapotranspiration if there is a coincident increase in supply (precipitation), as in a warmer, wetter world. Roderick, M., M. Hobbins and G. Farquhar. 2009. Pan Evapotranspiration Trends and the Terrestrial Water Balance. II. Energy Balance and Interpretation. Geography Compass 3/2 (2009):761-780,10.1111/j.1749-8198.2008.00214.x [Government of United States of America]	will clarify
2-1783	2	43	37			Be clear that "increased humidity" refers to specific/absolute humidity. [Government of Australia]	We will clarify the statement.
2-1784	2	43	42	43	42	Do not write "Fluxnet" in upper case. [J. Graham Cogley, Canada]	FLUXNET is the proper terminology, if you check their website it is all caps.
2-1785	2	43	43	43	46	This sentence is misleading and leads the reader to think that the FLUXNET ET data record goes back to 1982. The cited paper instead says that they used this approach: "We have designed an approach to assessing the temporal behaviour and global spatial distribution of ET over the past 27 years. It integrates point-wise ET measurements at the FLUXNET observing sites with geospatial information from satellite remote sensing and surface meteorological data in a machine-learning algorithm (the model tree ensemble or MTE; ref 4). The approach is data-driven and thus largely independent of theoretical-model assumptions." The sentence needs to be changed to emphasize that it is a data (FLUXNET) and remote sensing-model approach and that the ET increases from 1982-1997 are not based solely on eddy covariance data. [Government of United States of America]	We will clarify the statement.
2-1786	2	43	43	43	46	This sentence states that evapotranspiration has ceased to increase since the late 1990s due to decreased soil surface moisture supply. This begs the question as to why soil moisture supply has decreased. Lower rainfall? Is there evidence for this? Do the regions of decresed soil moisture supply match the regions where observations indicate less rainfall? Could change in the moisture content of the near-surface air into which moisture is transferred from the soil by evapotranspiration play a part? [Adrian Simmons, United Kingdom]	This would be a nice analysis, but this is beyond the scope here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1787	2	43	43			Choice in forcing data produces an average range in global monthly ET of 10.6 Wm-2 for one of the remote sensing algorithms (Badgley et al. in review). [Badgley, G., J. B. Fisher, C. Jiménez, K. P. Tu, and R. K. Vinukollu (in review), On uncertainty in global evapotranspiration estimates from choice of input forcing datasets, Geophysical Research Letters.] [JOSHUA FISHER, United States of America]	Thank you, no action requested.
2-1788	2	43	46	43	46	Adding one sentence to the end of this paragraph: However, its estimates in tropical humidity regions are questionable because this method ignores the impact of available energy on evapotranspiration (Wang and Dickinson, 2012). Reference: Wang, K.C. and Dickinson, R.E., 2012. A review of global terrestrial evapotranspiration: Observation, modeling, climatology, and climatic variability. Reviews of Geophysics, 50(2): RG2005. [Kaicun Wang, China]	will consider this in revising text.
2-1789	2	43	48	43	55	"Tibetan". [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1790	2	43	48	43	56	The level of detail in this regional results is surprising, particularly when compared to other sections (such as precipitation). Can´t all this regional information be synthesized? [Ileana Bladé, Spain]	Since this is pan evap, and obs are scarce we feel this is good context.
2-1791	2	43	48			There is too much review on pan evaporation, especially in regional studies, relative to the amount of discussion of global actual evapotranspiration (e.g., only 2 references given), which needs to be greatly expanded given the contributing role to the global energy balance of ET. For more on uncertainties in global ET, the authors can look to Jimenez et al. (2011) and Vinukollu et al. (2011). [Jiménez, C., C. Prigent, B. Mueller, S. I. Seneviratne, M. F. McCabe, E. F. Wood, W. B. Rossow, G. Balsamo, A. K. Betts, P. A. Dirmeyer, J. B. Fisher, M. Jung, M. Kanamitsu, R. H. Reichle, M. Reichstein, M. Rodell, J. Sheffield, K. Tu, and K. Wang (2011), Global inter-comparison of 12 land surface heat flux estimates, Journal of Geophysical Research, 116(D02102), doi:10.1029/2010JD014545. | Vinukollu, R. K., E. F. Wood, C. R. Ferguson, and J. B. Fisher (2011), Global estimates of evapotranspiration for climate studies using multi-sensor remote sensing data: Evaluation of three process-based approaches, Remote Sensing of Environment, 115, 801-823.] [JOSHUA FISHER, United States of America]	We will revise and consider adding more on global ET.
2-1792	2	43	50	43	50	Jhajharia et al. (2009) citation in Reference list appears to be wrong. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial, will be corrected
2-1793	2	43	50	43	50	Spelling of Prairies has missing 'i' [Kate Willett, United Kingdom]	Editorial, will be corrected
2-1794	2	43	52	43	52	delete “consistent with reported continuous increase in aerosol levels over China”, because the evaporation is mainly related to the temperatiure and wind, and it is something farfetched to link it with aerosel with limitted back of researches. [Qingxiang Li, China]	The aerosol statement is related to global dimming etc.
2-1795	2	43	53	43	53	Note that Roderick et al 2007 noted that the decreases in wind speed referred to here could be either for site-specific reasons (e.g. vegetation growth around the instruments) or more general, and did not attempt to distinguish between the two. [Government of Australia]	Noted.
2-1796	2	43	54	43	56	"concluded". "an overview". "concluded that". [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1797	2	43	56			"an overview" [Richard Allan, United Kingdom]	Editorial, will be corrected
2-1798	2	43	56			2.5.4 Surface evaporation. Insert: "Alexander et al. (2007) found no correlation between pan evaporation and the 21 year Hale cycle in the Southern African region over the last century." [David L. Hagen, United States of America]	Decline, reference not provided, plus the context is unclear.
2-1799	2	43	56			it should read “an overview … concluded that the major” [David Sauchyn, Canada]	Editorial, will be corrected
2-1800	2	44	1	44	2	"…, but that their relative importances vary regionally." [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1801	2	44	4	44	4	"decreasing aerosols" might not be correct. The ISCCCP indicates a late 1990s shift in low, mid and high level cloud cover patterns. Less low and mid level cloud means an increase in incoming shortwave radiation. [John McLean, Australia]	Decline, this is speculation.
2-1802	2	44	4	44	5	Why only in the humid parts of Europe? Why can't increase in shortwave radiation from decreasing aerosols explain positive ET trends anywhere else? [Government of United States of America]	We are limited to assessing the published literature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1803	2	44	5			correct typo: "(Wild et al., 2005)" [Richard Allan, United Kingdom]	Editorial, will be corrected
2-1804	2	44	6	44	6	What direction are these evapotranspiration and precipitation trends from Jung et al.? [Kate Willett, United Kingdom]	Will include more on the trends.
2-1805	2	44	6	44	6	add the following important reference: Gao, G., Chen, DL, Xu, C-Y, Simelton, E., 2007. Trend of estimated actual evapotranspiration over China during 1960-2002. Journal of Geophysical Research – Atmospheres, VOL. 112, D11120, doi:10.1029/2006JD008010, 2007. [Chong-Yu Xu, Norway]	Decline, doesn't add anything.
2-1806	2	44	16	44	22	This concluding paragraph is confusing. Why is it stated that evaporation "continues to decline" when it it later said that, globally, it was increasing until 1997 or 2002 (depending on the study). Is the difference due to the fact that the Jung et al. (2010) study is land-only (in which case it should be clarified earlier, in page 43, line 44) while the Wang (2010) study is truly "global"? And why is this paper not mentioned earlier in page 43? [Ileana Bladé, Spain]	This is pan evaporation that declines, not ET.
2-1807	2	44	16	44	22	The uncertainties in evaporation estimates are still *huge* and available datasets are still very limited. The GEWEX LandFlux and SEAFlux project aim to fill this gap. Recent papers investigated the uncertainties in global land ET products. These should be at least mentioned here. A more critical discussion is needed here. The papers I am referencing to are: (1) Jimenez et al (2011), doi:10.1029/2010JD014545, (2) Mueller et al (2011); doi: 10.1029/2010GL046230 [Alexander Loew, Germany]	Uncertainties are large, but adding much discussion adds too much text.
2-1808	2	44	16			There should be a summary statement on confidence in trends in actual evapotranspiration. [JOSHUA FISHER, United States of America]	Agree, we are adding this to the summary.
2-1809	2	44	16			Section 2.5.4: The "summary" paragraph includes new information that was not mentioned previously. [Government of United States of America]	Will ensure consistency between summary and text.
2-1810	2	44	17	44	17	and is -> which is [Ileana Bladé, Spain]	Editorial, will be corrected
2-1811	2	44	17	44	17	Change "and is possibly related" to "due", and say "to changes in one or more of …". The physics of evaporation is well enough understood to justify being more emphatic. [J. Graham Cogley, Canada]	Noted. Text has changed
2-1812	2	44	17	44	17	Adding one sentence before ".On a global Scale": The decreasing trend reversed after 1998 because of the increased water vapor deficit with a warming environment and a zero trend of global evaporative demand has been found from 1973 to 2008 (Wang et al., 2012, in press). Reference: Wang, K., Dickinson, R.E. and Liang, S., 2012. Global Atmospheric Evaporative Demand over Land from 1973 to 2008. Journal of Climate, in press (This paper can be found at: http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-11-00492.1). [Kaicun Wang, China]	We will investigate the paper and add if aproproate.
2-1813	2	44	19	44	19	evapotranspiration [LUCILA CANDELA, Spain]	Corrected.
2-1814	2	44	19	44	19	"evapotranspiration". [J. Graham Cogley, Canada]	Corrected.
2-1815	2	44	19	44	19	Typo in "evpotransipitation"? [Birgit Hassler, United States of America]	Corrected.
2-1816	2	44	20	44	20	It would be useful if this rate could be compared to the rate of change of net radiation. [J. Graham Cogley, Canada]	Decline, beyond the scope here.
2-1817	2	44	20	47	43	ON THE SIGNIFICANCE OF INCLUDING MENTION OF THE NVAP-M PAPER AND ITS FIG. 4C IN AR5: Positive feedback of water vapor (enhanced evaporation due to warming induced by GHGs) is key to GCMs. This key fact is why the new NVAP-M paper should be discussed and cited in AR5. While the original NVAP work was criticized, this should be considered in light of published problems with all the major global water studies (see, for example, Trenberth, K. E.; Fusillo, J; Smith, L. Trends and variability in column-integrated water vapor. Climate Dynamics 2005, 24 (7-8), 741–758). The 2012 NVAP-M study is a significant improvement and expansion over the original study. It is also the most comprehensive, multi-sourced atmospheric water vapor study to date, for NVAP-M uses data from quality-controlled upper air radiosonde soundings; SSM/I, HIRS, AIRS satellite soundings; and GPS stations. Note that HIRS retrieves the vertical profile of water vapor over land. A timeline of instruments used for the 2012 NVAP-M paper is at ftp://ftp.agu.org/apend/gl/2012GL052094. In conclusion, the initial 2012 NVAP-M paper well deserves	Declined. The NVAP data set continues to have problems for examining temporal trends owing to inhomogeneities. Additionally Vonder Haar didn't look at trends because of the inhomogeneities etc. NVAP is good for looking at spatial patterns, ENSO effects etc. and we will consider adding some text as it relates to spatial patterns, if we decide that is needed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						discussion and citation in AR5. The citation is: Thomas H. Vonder Haar, Janice L. Bytheway and John M. Forsythe. Weather and climate analyses using improved global water vapor observations. GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L15802, 6 PP., 2012. doi:10.1029/2012GL052094. Disclaimer: I do not know and have never met any of the NVAP team. I received one brief e-mail from a member of the team in response to a question about when the 2012 paper would be published. [Forrest Mims, United States of America]	
2-1818	2	44	20			Recent reductions in soil moisture appear qualitatively consistent with decreases in RH shown in the next section (2.5.5) [Richard Allan, United Kingdom]	Noted.
2-1819	2	44	21	44	21	"constraint on". [J. Graham Cogley, Canada]	Editorial, will be corrected
2-1820	2	44	24	45	18	Section 2.5.5 Surface Humidity: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted.
2-1821	2	44	26	44	26	Again, some theoretical framing at the beginning of this paragraph would be nice (I am again thinking of the graduate students reading this report to gain an overview of the topic). You could briefly state our expectations for humidity changes for increasing temperatures under the Clausius-Clapeyron equation and the assumption of constant RH (which appears to hold approximately true empirically, though apparently not in recent years). [Ileana Bladé, Spain]	The report is not intended to provide a review. No change has been made.
2-1822	2	44	26	45	18	Please consider including the result of the following study: Song Yafang, Liu Yanju, and Ding Yihui, 2012: A study of surface humidity changes in China during the recent 50 years. Acta Meteor. Sinica, 26(5), 541–553, doi: 10.1007/s13351-012-0501-9. [Sai Ming Lee, Hong Kong, China]	Thanks for the reference, but we assess only large scale changes.
2-1823	2	44	26	45	18	2.5.5 FAQ 2.1, Figure 2 - HadISDH is included here but not in the text or the figure caption. It's a shame not to include this update to HadCRUH but as it does use a different methodology to HadCRUH it probably does needs it own reference which unfortunately is still under review. So this is just a query - can HadISDH be in or not? I hope so but I understand there have to be deadlines on paper acceptance. [Kate Willett, United Kingdom]	Will inclue HadISH in Figure 2.30 Verify with Peter Thorne.
2-1824	2	44	26	45	18	Section 2.5.5 The Willett et al. 2007 paper could be referenced here that finds the global increase in surface specific humidity attributable to anthropogenic GHG emission at least in part (Willett, K. M., N. P. Gillett, P. D. Jones and P. W. Thorne, 2007: Attribution of observed surface humidity changes to human influence. Nature, 449, 710-713). This can then be fed through to TFE.1 in the technical summary. [Kate Willett, United Kingdom]	Attribution is not part of Chapter 2.
2-1825	2	44	33	44	33	over the more arid regions -> over some arid regions (there is no detectable increase over the Sahara). Please indicate those regions. "Small isolated" and "coherent" seems contradictory. [Ileana Bladé, Spain]	Changed to "some of the more arid regions"
2-1826	2	44	34	44	35	It would be informative to know what these trends represent in relative terms - are they 1% per decade? 5% ? [Dale Hurst, United States of America]	The approximate rate of percentage increases are noted at the end of the paragraph. No change has been made.
2-1827	2	44	34	44	35	Please give error estimates as in the temperature trends section [Geert Jan van Oldenborgh, Netherlands]	Are error estimates available from literature?
2-1828	2	44	34	44	35	By eye, the agreement between the different datasets looks very similar to Fig.2.24b, which was only accorded medium confidence. Whence the difference? [Geert Jan van Oldenborgh, Netherlands]	Medium confidence refers to the rate and vertical structure of temperature changes. Confirm with Peter
2-1829	2	44	36	44	36	Table 2.9 does not distinguish between winter and summer. Do you mean "northern hemisphere" or are you refering to results not shown in the table (clarify, if so). [Ileana Bladé, Spain]	the sentence refers to the warm season in either hemisphere. This will be clarified.
2-1830	2	44	36	44	36	Why are there no confidence/uncertainty intervals in this table? [Ileana Bladé, Spain]	These are trends from various studies that did not provide confidence intervals.
2-1831	2	44	36			Error made in reference to "summer hemisphere", should this be southern or northern hemisphere? [Government of Canada]	the sentence refers to the warm season in either hemisphere. This will be clarified.
2-1832	2	44	37	44	40	Something is wrong/missing in this sentence. It does not make sense. Should there be a stop mark before "averages" ? [Ileana Bladé, Spain]	The paragraph has been rewritten to clarify the meaning.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1833	2	44	37	44	40	Sentence is garbled. Perhaps "show" should be ", showing"? [J. Graham Cogley, Canada]	See response to 2-1832
2-1834	2	44	37	44	40	This sentence is not clear, something seems to be missing [Moira Evelina Doyle, Argentina]	See response to 2-1832
2-1835	2	44	37	44	40	Awkward sentence - needs splitting into two sentences probably. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	See response to 2-1832
2-1836	2	44	37	44	40	This very long and incomplete sentence needs to be rewritten. [Government of United States of America]	See response to 2-1832
2-1837	2	44	37	44	40	This sentence doesn't read very well. You could rewrite as two sentences, removing the 'only' as this implies that no such correlations are found with marine temperature which is not true: "Global specific humidity is sensitive to large scale phenomena such as ENSO (Figure 2.30b-e). It is strongly correlated with land surface temperature averages over the 23 Giorgi and Friancisco (2000) regions for the period 1973–1999 showing mostly increases at or above Clausius- Clapeyron scaling (about 7% K–1) with very high confidence (Willett et al., 2010)." This should be 1973-1999 instead of 1973-2003. [Kate Willett, United Kingdom]	See response to 2-1832
2-1838	2	44	38	44	38	Figure 2.30b-e -> 2.30b-c [Ileana Bladé, Spain]	Corrected.
2-1839	2	44	38	44	38	Reference to Figure 2.30b-e is not consistent with Figure 2.30 on page 174 (there are only Figures a-c) [Birgit Hassler, United States of America]	See response to 2-1838
2-1840	2	44	38	44	39	Change "the 23 Giorgi and Friancisco (2000) regions" to "the 23 regions Giorgi and Francisco (2000)" [Government of Brazil]	See response to 2-1832
2-1841	2	44	38	44	39	A line is missing between the end of 38 and the beginning of 39 [Government of France]	See response to 2-1832
2-1842	2	44	38	44	40	The wording of these lines is awkward. [David Sauchyn, Canada]	See response to 2-1832
2-1843	2	44	38			There are no d and e panels in Fig.2.30 [Government of United States of America]	See response to 2-1838
2-1844	2	44	38			There is no figure 2.30d-e. I believe it should be figure 2.30b-c. [Tamlin Pavelsky, United States of America]	See response to 2-1838
2-1845	2	44	42	44	42	I cannot reproduce the details in Fig. 2.30a either. Again are you doing something different than a simple linear trend (unlike what's stated in the Appendix?). How many missing observations do you accept before masking the region? [Ileana Bladé, Spain]	Noted. The code and data will be made available upon release. This code is the same as that used in many places elsewhere within the chapter as a whole.
2-1846	2	44	43	44	45	It would be worthwhile extending Fig 2.30 in time until 2012. ERA-Interim shows a further recent decline in relative humidity, to close to 2008 values. Geographically, the largest recent anomaly is associated with recent dryness over the USA. It should also be clarified whether the "ERA-Interim" results for 1979-1988 are really from ERA-Interim (as now available) or from ERA-40 (as in Simmons et al., 2010, which was written before ERA-Interim was extended backwards for the 1979-1988 period).The ERA results prior to 1979 must come from ERA-40 not ERA-Interim, contrary to what is stated in the caption. I would advocate showing only the results from ERA-Interim, i.e. results covering the period 1979-2012. I can supply the required values. [Adrian Simmons, United Kingdom]	Noted. Figure has been substantively revised in response to new datasets available since SOD and this comment ahs been accounted for iun those revisions.
2-1847	2	44	45	44	45	HadCRUHext is never introduced. Delete or mention the differneces with HadCRUH. [Geert Jan van Oldenborgh, Netherlands]	HadCRUHExt was discussed in the text. The acroym has been added to the text to clarify the point where HadCRUHExt is being referred to.
2-1848	2	44	48	44	48	Please remove table split in final version! [Peter Burt, United Kingdom]	Agree that this is preferable.
2-1849	2	44	48	45	1	Fig 2.30 includes results from ERA-Interim over land as well as the HadCRUH and NCAR data. Table 2.9, however, shows data only from HadCRUH and NCAR for land areas. Why were the ERA-Interim land values not included in Table 2.9? [Adrian Simmons, United Kingdom]	Noted. Table 2.9 has been completely revised. The revised table concentrates upon periods that precede ERA-Interim so it ahs not been included so as to avoid conflating dataset differences with period of record differences.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1850	2	44	48			Table 2.9. Please add the statistical significance for trends in specific and relative humidity. [Government of United States of America]	Noted. Accounted for in revised table.
2-1851	2	44				Table 2.9 Large scale trends in surface humidity data sets: Table was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted.
2-1852	2	44				Table 2.9 Please give error estimates as in the temperature trends section [Geert Jan van Oldenborgh, Netherlands]	Not sure if they are available?
2-1853	2	44				Table 2-9 Pleaes indicate which trends ar esignificant at p<0.1 (taking serial autocorrelations into account), eg by a bold face. [Geert Jan van Oldenborgh, Netherlands]	Noted. Accounted for in revised table.
2-1854	2	45	3	45	3	Doesn´t ERA-interim begin in 1979? How do you get an ERA curve covering 1975-2010 in panels b and c? [Ileana Bladé, Spain]	Accepted. We now no longer conflate ERA-40 and ERA-Interim so the ERA series starts from 1979
2-1855	2	45	3	45	3	Add 'reanalysis' after ERA-interim? [Kate Willett, United Kingdom]	accepted
2-1856	2	45	4	45	4	"concluded that there was". [J. Graham Cogley, Canada]	accepted
2-1857	2	45	4	45	44	A dataset does not conclude, people do. [Ileana Bladé, Spain]	will revise
2-1858	2	45	5	45	5	ERA-=interim starts in 1979, not 1989. [Geert Jan van Oldenborgh, Netherlands]	will revise
2-1859	2	45	6	45	6	Without the context I suggest earlier, this statement can seem obscure (compatible …). Same for the statement on lines 7-8, which could use some elaboration. [Ileana Bladé, Spain]	will revise
2-1860	2	45	6	45	6	What is a "quick-look" extension? [Ileana Bladé, Spain]	will revise, this is a "quick" update
2-1861	2	45	10	45	10	Bad English. Replace 'like' with 'as with' [Peter Burt, United Kingdom]	not sure why this is bad english.
2-1862	2	45	10	45	13	I think this needs to mention the work of Berry and Kent as they have the only up to date record of marine surface humidity. Could you change this to: "The marine specific humidity (not shown), like that over land, shows widespread increases that correlate strongly with sea surface temperature (Willett et al. 2008; Berry and Kent 2009). However, there is a marked decline in marine relative humidity around 1982. Willett et al. (2008) suggest that this is a non-climatic data issue owing to a change in reporting practice for dewpoint temperature whereas Berry (2009) concludes it to be a response to the shift to a very positive North Atlantic Oscillation at that time." The Berry and Kent ref is already in. Berry 2009 is: 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. If it is not ok to include a doctorate thesis as a reference you could just say: "Willett et al. (2008) suggest that this is a non-climatic data issue owing to a change in reporting practice for dewpoint temperature but this remains inconclusive." [Kate Willett, United Kingdom]	Agree, will include suggested text.
2-1863	2	45	11			I would strongly advocate changing "there is a marked decline in marine relative humidity" to either "observations suggest a marked decline in marine relative humidity" or "analysis of observations suggests a marked decline in marine relative humidity". This would fit in much better with the following sentence. [Adrian Simmons, United Kingdom]	Agree, will revise.
2-1864	2	45	13	45	13	Conventionally, 'dew point' is more common usage [Peter Burt, United Kingdom]	Editorial, will be corrected
2-1865	2	45	15	45	16	"However, over recent years this has abated over land, …", should such short-term behavior be discussed? Can one say global warming has stopped since 1998? [Xuebin Zhang, Canada]	this does provide some context, will leave in.
2-1866	2	45	16	45	16	this -> this increase [Ileana Bladé, Spain]	agree (actually on line 17).
2-1867	2	45	17	45	17	widespread decreases in relative humidity over land -> this is not mentioned earlier, only the global mean is discussed. Or is the word "overall" in line 5 meant to indicate "widespread"? If you use the same word in both sentences then there is no ambiguity. [Ileana Bladé, Spain]	Agree, will revise.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1868	2	45	17	45	18	Delete last sentence. [Government of Australia]	Agree, will revise.
2-1869	2	45	17	45	18	This final sentence is entirely appropriate. The point could be strengthened by noting that the decline in relative humidity from 2000 to 2008 is so rapid compared with behaviour over previous years that it is very unlikely to be indicative only of a climate trend, but rather in some significant part must involve low frequency variability. This view is supported by recent modelling evidence that has come too late to be included in AR5. An AMIP ensemble with a very recent version of the ERA model indicates a tendency towards recent drying (as measured by relative humidity) over land, but the drying that is observed and analysed in ERA-Interim is larger than in the ensemble mean, though just within the spread of the ten-member ensemble. These results are very recent, and have yet to be written up for submission to a journal. So they cannot be used in AR5. But they do support the sentence at the end of section 2.5.5, and perhaps justify a strengthening of the sentence along the lines indicated at the beginning of this comment. [Adrian Simmons, United Kingdom]	Agree, will revise.
2-1870	2	45	24	46	2	This statement does not hold for water vapour at low levels over land, which has not increased consistently with temperature at low levels over land for the last decade, at least if by "consistently" it is meant "consistent with the Clausius-Clapeyron relation and the notion of constant relative humidity". Please see in particular the comments immediately following. [Adrian Simmons, United Kingdom]	The statement specifically refers to water vapor in the troposphere at near-global scales. No change has been made.
2-1871	2	45	25	45	27	The primary reason for widely differing estimates of soil moisture is wide variations in the way models represent soil moisture and evaporation (see e.g. Haddeland et al 2011 and Harding et al 2012). Haddeland, I., and Co-authors, 2011: Multimodel estimate of the global water balance: Setup and first results. J. Hydrometeor., 12, 869–884. Harding R, Best M, Blyth E, Hagemann S, Kabat P, Tallaksen LM, Warnaars T, Wiberg D, Weedon GP, van Lanen H, Ludwig F and Haddeland I, 2011. Preface to the "Water and Global Change (WATCH) special collection: Current knowledge of the terrestrial Global Water Cycle. J Hydrometeorology. Vol. 12, 1149-1156. [European Union]	Wrong section
2-1872	2	45				Table 2.9: The HadCRUH Land and Ocean Combined trends for SH are outside the span of the land-only and ocean-only trends. For land, also quote trends from the new HadISDH dataset (Willett et al., 2012, submitted) if this paper is accepted before the deadline. [David Parker, United Kingdom of Great Britain & Northern Ireland]	That is entirely plausible. HadISDH will be included if the paper meets the acceptance deadline.
2-1873	2	46	6	46	19	Long-term tropospheric humidity (RH, q, PW) from 1970-2008 over China are reported in Zhao, T., A. Dai, and J. Wang, 2012: Trends in tropospheric humidity from 1970-2008 over China from a homogenized radiosonde dataset. J. Climate, 25: 4549-4567. [Aiguo Dai, United States of America]	These results are an analysis of a subset of the Dai et al (2011) product for China which is already referenced. The regional analysis for China is not directly relevant here.
2-1874	2	46	6	46	19	This paragraph is problematic as it is inconsistent with 2.5.5. Moreover, the statement on lines 16 and 17 that there is "no evidence for significant change in tropospheric relative humidity" is incorrect as regards the boundary layer over land, as can be seen by consulting one of the papers referenced. Rewriting is needed. Please see the following three detailed comments. [Adrian Simmons, United Kingdom]	This paragraph has been modified to address the comments from 2-1874 to 2-1877 and note the changes in relative humidity at lower levels.
2-1875	2	46	6	46	19	Firstly, the paper by Simmons et al.(2010) discussed in section 2.5.5 did not look at relative humidity only at the surface. It was shown that the fall in relative humidity over land during the past decade seen in ERA-Interim and in the extended HadCRUH analysis extended throughout the boundary layer in ERA-Interim, and could be seen at the 850hPa level. Simmons et al. also discussed the fit of the ERA-Interim reanalysis to radiosonde data around 850hPa. No significant change in the fit to these data was found over the past decade, indicating that the quite rapid drop in relative humidity was supported by radiosonde data. This should be noted in the paragraph. It may also be remarked that this is also supported by physical reasoning, as consideration of boundary-layer mixing leads one to expect that the near-surface signal would be reproduced over the lowest 100hPa of the atmosphere or more, provided the surface signal was not confined to times of stable, shallow boundary layers. [Adrian Simmons, United Kingdom]	See response to 2-1874
2-1876	2	46	6	46	19	The above result is in fact consistent with the results of McCarthy et al.(2009), one of the three papers cited in paragraph 2.5.6.1. Please consult figures 10 and 11 of McCarthy et al.'s paper. The middle panel of figure 10 shows the relative humidity trend as a function of height. At the surface, the whole of the error bar for daytime data lies on the side of declining relative humidity. Most of the error bar for nighttime data lies on the side of declining data. The latter is true also at 850hPa, for both daytime and nightime data. This is inconsistent with	See response to 2-1874

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the statement in 2.6.5.1 that there is "no evidence for significant change in tropospheric relative humidity". Figure 11 of McCarthy et al.'s paper shows a time series of relative humidity at the surface from the radiosonde dataset that has relative humidity reaching at minimum at the end of the period. It is unfortunate that McCarthy et al.'s study runs only to the end of 2004, as Simmons et al.(2010) show relative humidty continuing to decline substantially until 2008. [Adrian Simmons, United Kingdom]	
2-1877	2	46	6	46	19	The other two papers cited in 2.5.6.1 do not really contribute to the question of boundary-layer relative humidity over land. Dai et al.(2011) is perhaps a bit problematic in any case, since homogenising on the basis of dew-point depression may build in a tendency for relative humidity to be rather uniform. But aside from this, Dai et al predominantly show results at 700hPa and higher. I could not see anything in the paper that would contradict the findings of McCarthy et al. and Simmons et al. indicative of declining relative humidity near the surface and up to at least 850hPa. Durre et al.(2009) discuss precipitable water for the surface to 500hPa layer. They show an upward trend, but one cannot from their results rule out a decrease in relative humidity up to 850hPa over land during the past decade. [Adrian Simmons, United Kingdom]	See response to 2-1874
2-1878	2	46	9	46	19	Duirre et al did not deal with inhomogeneities in the radiosonde record and the results should be discounted compared with Dai et al. On the other hand, it is probably impossible to get a fully homogenous dataset that properly reflects real trends owing to the corrections needed and lack of any standard. [Kevin Trenberth, United States of America]	No change is suggested.
2-1879	2	46	13	46	13	A minority of the artificial trends removed were moistening trends. [David Parker, United Kingdom of Great Britain & Northern Ireland]	This statement has been clarified to indicate that "on average" the impact of the correction procedures is to remove a drying trend.
2-1880	2	46	14	46	14	Change 'grossly' to 'largely' consistent. Using grossly just sounds like bad English. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Changed.
2-1881	2	46	14	46	16	Again, I recommend discussing this expected consistency (at some length, including assumptions) at the beginning of the humidity section (2.5.5) since it keeps coming up. Then you can simply refer to it in much shorter terms whenever is needed rather than the current repetitive multiple allusions (with varying degrees of elaboration). [Ileana Bladé, Spain]	This is unnecessary.
2-1882	2	46	15	46	15	"equilibrium vapour pressure" ? Or "saturation vapour pressure" [Dale Hurst, United States of America]	They are the same, but this has been changed for consistency elsewhere in the chapter.
2-1883	2	46	16	46	17	This evidence is in contrast to the surface? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	See response to 2-1874
2-1884	2	46	32	46	32	The Mears et al. (2011) reference in the bibliography (where it is duplicated!) is not about water vapour. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Correct reference is Mears, C., J. Wang, S. Ho, L. Zhang and X. Zhou, 2010: Total column water vapor, in State of the Climate in 2009. Arndt, D. S., M. O. Baringer, M. R. Johnson eds., Bull. Amer. Meteorol. Soc., 91, S24.
2-1885	2	46	35	46	35	delete “that expected from” [Ileana Bladé, Spain]	Preferred as is.
2-1886	2	46	36	46	36	It would be informative to know what this trend represents in relative terms - 1% per decade? 5% ? [Dale Hurst, United States of America]	Not included, because we refer to the percentage changes with respect to temperature.
2-1887	2	46	37	46	37	Delete 'of' [Peter Burt, United Kingdom]	Deleted.
2-1888	2	46	39	46	39	How about adding “by themselves” before “should not be interpreted”. These GPS-measured trends serve to strength our confidence that there has been an increase in tropospheric water vapor, so adding “by themselves” clarifies, I think, the purpose of this paragraph. [Ileana Bladé, Spain]	Added.
2-1889	2	46	42	47	7	AR4 claimed an increase in water vapour (Trenberth 2005) and this claim is now repeated referring to Wentz 2007. A more recent and updated time series has been published in Vonder Haar, T. H., J. Bytheway, and J. M. Forsythe (2012), Weather and climate analyses using improved global water vapor observations, Geophys. Res. Lett.,doi:10.1029/2012GL052094. Their figure 4 shows a different picture of water vapour trends since 1988, i.e. there doesn't seem to be one. The authors are careful but state in their conclusion:	The water vapor data from NVAP-M does not account for discontinuities intrdouced from the merging of multiple satellite records. As noted by the authors, this must be addressed before the NVAP-M product can be used to estimate decadal trends. Indeed, the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						"Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data." This new dataset (NVAP) calls into question claims that water vapour has been rising during the past 20 years. [Marcel Crok, The Netherlands]	sentence quoted is actually refering to this limitation: "The results of Figures 1 and 4 have not been subjected to detailed global or regional trend analyses, which will be a topic for a forthcoming paper. Such analyses must account for the changes in satellite sampling discussed in the auxiliary material. Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data." Single stream data products (e.g., SSMI only from Wentz 2007) do not suffer from the same level of discontinuity in sampling as do multiple data products like NVAP-M, hence their use in Figure 2.30.
2-1890	2	46	44	47	6	Section 2.5.6.3 Its worth stating that the long-term record of satellite total column water vapour is for ocean only. Also, in the technical summary a value of 3.5% is given for increases in TCWV in the last 40 yrs. I can't find this anywhere in Ch 2. Where does this come from? Also, the technical summary states that tropospheric humidity is attributable to human emissions but this is not mentioned here and probably should be - Santer et al. (2008 PNAS). [Kate Willett, United Kingdom]	The ocean-only is now noted. However, attribution is not part of Chapter 2.
2-1891	2	46	47	46	47	The meaning of the word “globally” keeps changing. Here it appears to mean “vertically-integrated”. While one can eventually figure this out from context it does not seem like good practice. [Ileana Bladé, Spain]	No, It means globally here. The sentence was incomplete in that the microwave measurements are ocean-only. This is now clarified.
2-1892	2	47	1	47	1	add “compare with Fig. 2.16” after “Figure 2.31” to justify this statement. [Ileana Bladé, Spain]	Added.
2-1893	2	47	1	47	2	Earlier in the chapter, tropospheric temperature trends were considered not well known, so it seems odd to be now relying on them to deduce expected moisture trends based on Clausius-Clapeyron. [Dian Seidel, United States of America]	This statement is based on satellite measurements of total column water vapor and SST, not radiosonde measurements.
2-1894	2	47	1	47	3	The sentence spanning these lines is wrong. It is inconsistent with section 2.5.5. Since AR4, it has been shown both initially from ERA-Interim and subsequently from direct analysis of surface measurements (the extended HadCRUH dataset) that relative humidty at the surface has declined over the past decade. The discussion above presents evidence that this decline extends up to at least 850hPa. If AR5 cannot refute the evidence of decline provided in peer-reviewed publications, it should not state the contrary. It may, of course, choose to express low confidence in the results, or (see comment 109) express an opinion concerning it being primarily a low-frequency variation rather than a trend. [Adrian Simmons, United Kingdom]	This has been changed to explicitly refer to changes over ocean. The consistency between ocean total column water vapor and near-surface specific humidity from HadCRUH (comment 2-1895) is also noted.
2-1895	2	47	1			"...at the global scale (Figure 2.31) and interannual anomalies show remarkable agreement with low level specific humidity anomalies from HadCRUH (O'Gorman et al. 2012)." [doi: 10.1007/s10712-011-9159-6, Fig. 8b] [Richard Allan, United Kingdom]	Added.
2-1896	2	47	8	47	11	Figure 2.31 depicts water vapor only over the oceans. Figure 4c from the new 2012 NVAPS-M study (citation given in previous row above) is much more relevant, for it is a time series of water vapor over both land and oceans from 1988 to 2009. The NVAP-M chart in Figure 4c should either replace the current Figure 2.31 or be added as a new figure. [Forrest Mims, United States of America]	See response to 2-1889
2-1897	2	47	8	47	12	See comment above about Vonder Haar (2012). Better to show the updated time series of Vonder Haar or show both Wentz 2007 and Vonder Haar 2012 and explain why Vonder Haar fails to show an increase in global water vapour. [Marcel Crok, The Netherlands]	See response to 2-1889
2-1898	2	47	9	47	11	In view of the correlation between low-frequency variability/trends in TCWV and saturation specific humidity computed using sea-surface temperature, it might be worthwhile adding corresponding plots of qsat(SST) (suitably normalised) to Fig 2.31. The reason for doing this is that we have estimates of SST going back for a century or more, and establishing a strong relationship with TCWV for the recent period for which we have observations of TCWV enables us to derive an estimate of TCWV for the period for which we have reasonably trustworthy SST. [Adrian Simmons, United Kingdom]	We will consider adding this to the figure.
2-1899	2	47	11	47	11	Delete 'a' [Peter Burt, United Kingdom]	Deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1900	2	47	13	47	14	Su et al. (2006) shows that upper troposphere humidity is strongly related to the convective activity and SST over the tropical oceans. The authors may wish to include this reference. Reference: Su, H., W.G. Read, J.H. Jiang, J.W. Waters, D.L. Wu, and E.J. Fetzer, "Enhanced positive water vapor feedback associated with tropical deep convection: New evidence from Aura MLS," Geophys. Res. Lett. 33, L05709, doi:10.1029/2005GL025505, 2006. [Government of United States of America]	The sentence referring to convection and UTH has been removed and this reference is no longer relevant.
2-1901	2	47	14	47	14	regime(Chuang -> regimes (Chuang [Mihai Dima, Romania]	This reference has been removed
2-1902	2	47	16	47	16	Chung et al. (2010) relates to radiative fluxes and Chuang et al. (2010) makes no statement matching this sentence. [David Parker, United Kingdom of Great Britain & Northern Ireland]	This reference has been removed
2-1903	2	47	17	47	17	Define "OLR" [Birgit Hassler, United States of America]	This has been added.
2-1904	2	47	23	47	23	Insert comma after 'However' [Peter Burt, United Kingdom]	Added.
2-1905	2	47	29	47	37	What is this NCEP-reanalysis-based section doing in the “Satellite” section? Besides, this paragraph seems entirely unnecessary given the conclusion that reanalysis products are unsuitable. I suggest keeping only the last sentence (perhaps keep all references to justify it) and moving it at the end of the introductory paragraph of section 2.5.6 (before section 2.5.6.1). [Ileana Bladé, Spain]	This paragraph has been removed and moved to section 2.5.6.4. It now includes references to the Dessler et al paper. The conclusion that reanalyses are unsuitable for the analysis of water vapor trends remains.
2-1906	2	47	29	47	37	If the Paltridge (2009) paper is discussed, then the Dessler et al(2010) expansion and clarification to that work should also be included. While I would agree that water vapor trends in reanalyses are in the unreliable range, they are best taken as a multi-model analysis, than individually (Bosilovich et al. 2009). My interpretation of Dessler's results are that reanalyses still remain faithful to the real physical processes, despite uncertainty in trends - Dessler, A. E. and S. M. Davis (2010), Trends in tropospheric humidity from reanalysis systems, J. Geophys. Res., 115, D19127, doi:10.1029/2010JD014192. - Bosilovich, Michael G., David Mocko, John O. Roads, Alex Ruane, 2009: A Multimodel Analysis for the Coordinated Enhanced Observing Period (CEOP). J. Hydrometeor, 10, 912–934. doi: http://dx.doi.org/10.1175/2009JHM1090.1 [Michael Bosilovich, United States of America]	See response to 2-1905
2-1907	2	47	29	47	37	This paragraph concludes too negatively. Firstly, the only reference given in the final sentence, which states that "reanalysis products are stll considered to be unsuitable for the analysis of water vapour trends" is dated 2009. Reading the reference as listed on page 2-101, lines 24 and 25, I see no discussion of reanalysis in the paper at all! Even if the reference should be to a different paper, a paper published in 2009 cannot have assessed fully the characteristics of the latest generation of reanalyses. Simmons et al.(2010) show (section 2.5.5 and above comments) show that reanalysis can do a good job with low-level humidity over land. See also the next comment. Please also see the general comment 77 about the timescales of the IPCC assessment process and reanalysis production. [Adrian Simmons, United Kingdom]	See response to 2-1905
2-1908	2	47	29	47	37	I am surprised that this paragraph does not include reference to Dessler & Davis (2010, JGR, doi:10.1029/2010JD014192). I quote from the paper "For the ERA--Interim and MERRA, the long-term slopes in Figure 2b are close to the short-term slopes in Figure 2a in the tropical mid and upper troposphere. The JRA and ERA40 also show generally positive slopes here. Only the NCEP/NCAR has negative slopes in the tropical mid and upper troposphere." The NCEP/NCAR results, produced from the oldest generation of reanalysis system, are clearly an outlier, and should not be used to to conclude generally that "reanalysis products are stll considered to be unsuitable for the analysis of water vapour trends". Dessler and Davis further point out that the newer reanalyses, ERA-Interim and MERRA, are in better agreement with theory and observations for both long- and short-term climate fluctuations (for the topic studied - the link between upper-tropospheric humidity and surface temperature for the Tropics). Reanalyses improve from one generation to the next, and past performance is not necessarily a good guide to current (or indeed future) performance. This is of course not to say that the current generation of reanalyses is without flaws when it comes to water vapour trends. But the concluding sentence of the paragraph is much too negative. [Adrian Simmons, United Kingdom]	See response to 2-1905

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1909	2	47	29	47	37	It should be noted that the NCEP/NCAR R1 reanalysis that Paltridge et al is based upon assimilated non-bias corrected radiasonde data and next switched to satellite observations with different biases. [Geert Jan van Oldenborgh, Netherlands]	See response to 2-1905
2-1910	2	47	29	47	37	It may be better to leave tropospheric humidity from reanalyses out - I'm not convinced of the value of reporting the NCEP reanalyses trends from the Paltridge et al. paper in isolation from trends from ERA-Interim, MERRA and JRA, certainly not without also mentioning the Dessler et al. 2010 paper (Dessler, A. E., and S. M. Davis (2010), Trends in tropospheric humidity from reanalysis systems, J. Geophys. Res., 115, D19127, doi:10.1029/2010JD014192.) that discusses why NCEP has decreasing tropospheric humidity and that it is dissimilar to all other more 'state of the art' reanalyses (ERA-Interim, MERRA, JRA). [Kate Willett, United Kingdom]	See response to 2-1905
2-1911	2	47	29			Paltridge et al has no credibility whatsoever because it uses NCEP reanalyses which have completely spurious trends in moisture (see Trenberth et al 2005 in AR4). NCEP R1 does not assimilate any satellite data over the ocean. [Kevin Trenberth, United States of America]	See response to 2-1905
2-1912	2	47	30	47	30	Consider using the more common pressure unit "hPa" instead of "mb" [Birgit Hassler, United States of America]	This has been added.
2-1913	2	47	31	47	31	After “midlatitudes.”, add “However, Dessler et al. (2012) are doubtful that these negative long-term specific humidity trends in the NCEP/NCAR reanalysis are realistic due to, among other issues, non-assimilation of satellite radiances.”. [Government of India]	The Dessler and Davis (2010) study is now referenced.
2-1914	2	47	36	47	37	Ae modern reanalyses such as MERRA, ERA-interim with sophisticated bias adjustement schemes not substantially better? [Geert Jan van Oldenborgh, Netherlands]	There is still widespread disagreement on trends from reanalyses. See Dessler and Davis (2010) which is now referenced.
2-1915	2	47	36	47	37	Might be worth being explicit that this refers to tropospheric water vapour trends as earlier in the Chapter ERA-Interim is used to look at surface humidity over land - and it has been shown to be in good agreement and therefore useful for the assessment of long-term trends at least for surface humidity over land. [Kate Willett, United Kingdom]	Added.
2-1916	2	47	37	47	37	Sherwood et al. (2009) does not address reanalyses. [David Parker, United Kingdom of Great Britain & Northern Ireland]	This has been corrected to Sherwood et al. 2010
2-1917	2	47	38	47	39	The following new subsection is proposed 2.5.6.4 (after the Satellite section 2.5.6.3): 2.5.6.4 Merged Multisensor Land and Ocean The NASA Water Vapor Project-MEaSUREs (Making Earth System Data Records for Use in Research Environments ; NVAP-M) global water vapour dataset (Vonder Haar et al. (2012)) merges passive microwave, hyperspectral and broadband infrared, GPS and radiosonde measurements for a 22 year period (1988-2009). A daily, global land and ocean total column and layered precipitable water vapour record was produced for climate studies. Since data are used from multiple satellites, time-dependent biases are reduced in the dataset due to reprocessing with time-consistent algorithms and intersatellite calibration. At this time a robust trend in global total column water vapour from the merged NVAP-M dataset cannot be disproved. Reference: Vonder Haar, T. H., J. L. Bytheway, and J. M. Forsythe, 2012. Weather and climate analyses using improved global water vapor observations, Geophys. Res. Lett., 39, L15802, doi:10.1029/2012GL052094 [Government of United States of America]	See response to comment 2-1889
2-1918	2	47	39	47	39	Rather than "unreliable" the issue is one of being "fit for purpose". In many cases the products are not designed for such a use. [Government of Australia]	No suggested change is made.
2-1919	2	47	39	47	43	After a positive discussion of various reanalysis studies of radiosonde humidity data, this closing section criticizes all reanalysis products. The obvious concern to this reviewer, who has measured total column water vapor for 22.5 years, is the absence of any mention of the 2012 NVAP-M paper. This paper concludes,	See response to comment 2-1889

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						"Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data." Non-specialist readers must be made aware of this finding and that it is at odds with some earlier papers. Many cited papers in AR5 have yet to be published, but the first NVAP-M paper was published earlier this year (after the FOD reviews) and is definitely worthy of citation: Thomas H. Vonder Haar, Janice L. Bytheway and John M. Forsythe. Weather and climate analyses using improved global water vapor observations. GEOPHYSICAL RESEARCH LETTERS, VOL. 39, L15802, 6 PP., 2012. doi:10.1029/2012GL052094. [Forrest Mims, United States of America]	
2-1920	2	47	39			Same point again. It is unjustified of AR5 to conclude so generally that "reanalysis products of water vapour remain unreliable for trend detection". To justify this there would have to be published evidence of the the general unreliability of the current generation of reanalyses, not previous generations. A correct statement would be that "Previous generations of reanalyses have exhibited some severe problems in representing trends in water vapour. Some significant improvements and cases of reliable representations of low frequency variability and trends in water vapour have been seen in the latest generation of reanalyses, although reanalysis products must still be treated with caution in this regard, as must raw observational data." [Adrian Simmons, United Kingdom]	Dessler and Davis (2010) is now cited as evidence of the widely varying estimates of humidity trends between different reanalysis products.
2-1921	2	47	42	47	43	And again. The statement "Significant trends in tropospheric relative humidity at large spatial scales have not been observed" needs to be qualified. The boundary layer over land is part of the troposphere, and there is evidence there both from direct observational study of surface synoptic data (the HadCRUH extension discussed in Simmons et al., 2010) and radiosonde data (McCarthy et al., 2009; see comment 113) and from reanalysis (ERA-Interim, with supporting evidence from JRA-25, discussed by Simmons et al., 2010). I am sorry to have had to push my own work so much in this and the above comments, but until such time as it may be shown to be wrong or misleading, it should be acknowledged. [Adrian Simmons, United Kingdom]	The summary has been qualified to note that near-surface relative humidity over land has decreased in recent years with reference to section 2.5.5.
2-1922	2	47	43	47	43	It is very likely→ It is thus very likely [Ileana Bladé, Spain]	No change is made.
2-1923	2	47	45	47	45	There is no any graphic plot related to cloud cover changes/trends presented in this chapter, which is a serious negligence considering the important role of cloud played in the climate change. There are at least two long-term satellite cloud products are available now: ISCCP and PATMOS-x cloud products. It would be helpful to show a plot about the time serious of cloud cover from these two datasets. [Xuepeng (Tom) Zhao, United States of America]	There is no consensus on global scale cloud variability. A figure illustrating that was not deemed worthy, given the space limitations.
2-1924	2	47	45	48	29	Section 2.5.7. Clouds are regularly presented in the BAMS annual state of the climate report and several datasets are shown, all with quite different decadal variations and trends, highlighting the spurious changes present associated with changing satellites and sensors. Why is l 15-20 on p 48 included? it was dealt with in AR4 and deals only with data to 1996! Get rid of it. AR4 is much better than this. [Kevin Trenberth, United States of America]	Warren et al. (2007) was not included in AR4. No change has been made. We have added a reference to a recent assesment of multiple satellite products (Stubenrauch et al. 2013) and discus their conflicting variability.
2-1925	2	47	45	49	5	Tokinaga and Xie, 2011, Nature Geoscience, 4, 222-226 is a good reference for SST, cloud cover and precipitation changes over the Equatorial Atlantic over the last 60 years. [Government of France]	This study is more on the physical processes involved rather than documenting the character of trends of this period. It is therefore not included.
2-1926	2	47	50			Plaese add a few introductory sentences that surface observations of vlouds are performed by iobservers estimating the cloud cover by eye, which can be ambiguous in teh case of translucent clouds. In some countries the observaers have been replced by automatised equipment with very different biaes. Homogeneity is therefore a large concern. [Geert Jan van Oldenborgh, Netherlands]	This is not included due to space limitations.
2-1927	2	47	50			There is a strong connection between cloudiness, SSR and DTR. The latter is employed in cliudiness reconstructions such as The CRU TS 3.10 product. Pleaes compare these indirect estimates with the direct ones reported upon here. [Geert Jan van Oldenborgh, Netherlands]	This is a good suggestion, but because of the subtleties involved in interpreting the results it would represent a new analysis of data sets rather than an assesment of existing studies.
2-1928	2	47	54	47	54	A few → Only a few (this way “less globally consistent” in the following sentence makes more sense) [Ileana Bladé, Spain]	Changed
2-1929	2	47	55	47	55	and regional reductions were reported → with regional reductions reported [Ileana Bladé, Spain]	Changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1930	2	47		49		Ground-based, multi-platform and satellite measurements of cloud macro- and micro-physical parameters and associated observations responsible for the formation, development and dissipation of clouds need improvements. The existing long-term data related to cloud morphology show mixed results. Since cloud and aerosol phenomena are strongly inter-linked, their invidual and combined effects on solar radiation needs to be understood for better evaluation of climate and climate change. With the advent of recent technological developments, particularly with satellites, better observations that cover large spatial and temporal scales are hoped in the near future. Such data would be very valuable for representing cloud processes in climate models. [Panuganti, C.S. Devara, India]	No change is suggested.
2-1931	2	48	1	48	13	This reads like a list of results (in no particular order) and not like an assessment. Local results (e.g., for Poland, Tibet) should not even be mentioned. There is also a random attribution statement that does not seem to belong. I think the paragraph should be rewritten in such a way that it clarifies in which way it supports or contradicts conclusions from AR4. [Ileana Bladé, Spain]	The attribution statement was removed.
2-1932	2	48	1	48	13	Quantify the changes, don't just say "decreased", "less frequent", "declined", "increased" etc but provide objectivbe quantification. [John McLean, Australia]	It is difficult to quantify these changes in a consistent manner from different studies and regions.
2-1933	2	48	1	48	18	I suggest condensing this material to: "Work done since AR4 has indicated decreases in cloud occurrence/cover in recent decades over Poland (Wibig 2008), China and Tibet (Xia 2010b; Endo and Yasunari, 2006; Duan and Wu 2006), in particular for upper level clouds (Warren et al. 2007) and also over Africa, Eurasia and in particular South America (Warren et al.2007). Increased frequency of overcast conditions has been reported over some regions, such as Canada from 1953-2002 (Milewska 2004), with no statistically significant trends evident over Australia (Jovanovic et al., 2011) and North America (Warren et al. 2007). In general, low- and mid-level..." [Richard Allan, United Kingdom]	The suggested rewording has been used.
2-1934	2	48	3	48	3	Change "most" to "many". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Changed
2-1935	2	48	5	48	5	Change 1971 to 1951 and delete "(compared to the period 1941-1970)". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Changed
2-1936	2	48	8	48	8	Change "diurnal" to "daytime" because the text could be mis-interpreted as meaning a reduction throughout the diurnal cycle. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Changed
2-1937	2	48	10	48	10	"Warren et al. (2007) noted…" - check all EndNote usage throughout the chapter. [James Renwick, New Zealand]	Changed
2-1938	2	48	12	48	13	The findings of Jovanovic et al are refuted by data from the Australian Bureau of Meteorology (see http://www.bom.gov.au/climate/change/cloud.shtml). [John McLean, Australia]	This result is not peer reviewed.
2-1939	2	48	13	48	13	Capital 'C' required for 'century' [Peter Burt, United Kingdom]	Changed
2-1940	2	48	15	48	20	Why is this paragraph separated from the previous one? Both of them describe “new developments” over land since AR4. If you just want to avoid a very long paragraph at least say “Some other new developments” (line 15), but my previous comment about the lack of assessment and proper comparison with AR4 holds for this paragraph as well. [Ileana Bladé, Spain]	The paragraph has been changed and merged with the previous one. See comment 2-1934.
2-1941	2	48	18	48	18	What does “in general” mean ? Mostly everywhere? Please be more specific. [Ileana Bladé, Spain]	It means broadly speaking and is intended to not be specific.
2-1942	2	48	19	48	20	One has to be careful about a statement for cirrus clouds from surface observations; if lower level clouds increase (as stated in the beginning of the sentence), then they hinder the sight on upper level clouds. [CLAUDIA STUBENRAUCH, France]	This effect is noted in the study.
2-1943	2	48	22	48	22	Move “over the ocean” to the beginning of the paragraph to clarify the subject of this paragraph. [Ileana Bladé, Spain]	Suggested change in wording is not accepted.
2-1944	2	48	27	48	27	The weakening of the Walker circulation has a simple explanation. Strong Walker circulation is associated with La Nina conditions or conditions that are near La Nina, and likewise Hadley circulation with El Nino conditions. With the SOI dominated by conditions on the El Nino side of neutral (i.e. SOI = 0) from 1976 to 2009, one	This is not relevant to this section.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						would expect the Walker Circulation to be less vigorous and the transmeridional Hadley Circulation to be enhanced. (Refer: Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000), and Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures") [John McLean, Australia]	
2-1945	2	48	33			Section 2.5.7.2: Suggest reference to the NASA's A-Train satellite observations (L'Ecuyer and Jiang, 2010). The A-Train satellite observations play a major role in AR5/CMIP5 climate model evaluation and diagnosis processes (e.g. Jiang et al. 2012; Su et al. 2012) especially for clouds observations. References: L'Ecuyer, T.S., and J.H. Jiang, "Touring the atmosphere aboard the A-Train," Physics Today 63, 7, 36-41, 2010. Jiang, J.H., H. Su, C. Zhai, V.S. Perun, A. Del Genio, L.S. Nazarenko, L.J. Donner, L. Horowitz, C. Seman, J. Cole, A. Gettelman, M. Ringer, L. Rotstayn, S. Jeffrey, T. Wu, F. Brient, J-L. Dufresne, H. Kawai, T. Koshiro, M. Watanabe, M. , E.M. Volodin, T. Iversen, H. Drange, M.S. Mesquita, W.G. Read, J.W. Waters, B. Tian, J. Teixeira, and G.L. Stephens, "Evaluation of Cloud and Water Vapor Simulations in CMIP5 Climate Models Using NASA A-Train Satellite Observations," J. Geophys. Res. 117, D1410, 24 PP, 10.1029/2011JD017237, July 2012. Su, H., J.H. Jiang, C. Zhai, V.S. Perun, J.T. Shen, A. Del Genio, L.S. Nazarenko, L.J. Donner, L. Horowitz, C. Seman, C. Morcrette, J. Petch, M. Ringer, J. Cole, M. Mesquita, T. Iversen, J.E. Kristjansson, A. Gettelman, L. Rotstayn, S. Jeffrey, J.L. Dufresne, M. Watanabe, H. Kawai, T. Koshiro, T. Wu, E.M. Volodin, T. L'Ecuyer, J. Teixeira, and G.L. Stephens, "Diagnosis of Regime-dependent Cloud Simulation Errors in CMIP5 Models Using A-Train Satellite Observations," J. Geophys. Res., in press. [Government of United States of America]	We have added a reference to Staubenrach et al. (2013) which includes an analysis of some A-Train data sets. See comment 2-1949
2-1946	2	48	34	48	34	Insert comma after 'However' [Peter Burt, United Kingdom]	Added.
2-1947	2	48	39			2 sections: The authors were not aware of a joint international effort to assess global cloud datasets. The outcome of this project is described in a WCRP report (in press) and summarized by Stubenrauch et al., 2012: Assessment of Global Cloud Datasets from Satellites: Project and Database initiated by the GEWEX Radiation Panel, Bull. Amer. Meteor. Soc., accepted; both available at: http://climserv.ipsl.polytechnique.fr/gewexca/. This ref should replace Evan et al. 2007 and Heidinger and Pavolonis 2009. [CLAUDIA STUBENRAUCH, France]	This reference has been added.
2-1948	2	48	43	48	43	What about HIRS (Wylie et al. 2005)? This has a long record from the 1980s to present as shown in BAMS State of the Climate 2011 (Blunden et al. 2012): Foster, M. J., S. A. Ackerman, A. K. Heidinger and B. C. Maddox, 2012: [Global Climate] Cloudiness [in "State of the Climate in 2011"]. Bulletin of the American Meteorological Society, 93(7), S27-S28. [Kate Willett, United Kingdom]	This is not new and was reported in AR4.
2-1949	2	48	45	48	45	replace ref for PATMOS-x by Heidinger et al. 2012 (from Table 1 in Stubenrauch et al. 2012, see above). [CLAUDIA STUBENRAUCH, France]	This reference has been added.
2-1950	2	48	46	48	50	Please end the 2. sentence of paragraph at end of line 46 and revise rest of the paragraph taking into account the information from GEWEX the cloud assessment: From Fig. 6 of Stubenrauch et al. 2012, including timeseries from 9 satellite datasets, it was concluded: 'On average, the magnitude of the variations corresponds to the global mean interannual variability.' Detailed investigations by Rossow (Annex II of WCRP report (in press) available at http://climserv.ipsl.polytechnique.fr/gewexca/) on possible sources leading to spurious changes in the ISCCP CA time record show that, although they can change the magnitude of the slow CA variations by about one third, they cannot account for all of the variation. The latter study should replace Evan et al. 2007. [CLAUDIA STUBENRAUCH, France]	Results from this study have been added to the paragraph.
2-1951	2	49	1	49	3	The meaning of this sentence is unclear. [Government of United Kingdom of Great Britain & Northern Ireland]	This paragraph has been changed incorporationg suggestions from comments 2-1952, 2-1954 and 2-1955.
2-1952	2	49	1	49	3	Sentence seems confused. Change to "To summarize, there remains substantial ambiguity in surface observations of global-scale cloud variability and trends. What trends do exist are likely to be within the range	See response to 2-1952

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of uncertainties of the surface observations." [James Renwick, New Zealand]	
2-1953	2	49	1	49	5	There are many problems with this paragraph. The first sentence is grammatically wrong (perhaps “while” is not needed). The conclusions come as a total surprise. As far as I can tell, there is no prior discussion of uncertainties and the satellite results you cite do not support the statement that subtropical dry zones have expanded and cloud height has increased between the 1980-2000. Is something missing? [Ileana Bladé, Spain]	See response to 2-1952
2-1954	2	49	1	49	5	This summary does not synthesise the previous two sub-sections. For the first sentence I suggest "To summarize, surface-based observations show region- and height-specific variations and trends in cloudiness. Any global trends are therefore likely to be small and within the range of the uncertainties of the surface observations". The second sentence requires supporting material and citations to be added to Section 2.5.7.2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	See response to 2-1952
2-1955	2	49	1	49	5	please revise this section: A number of factors can produce spurious interannual variations in cloud products, because the size of the actual variations is small. Thus, until a cloud product has been examined to determine whether there have been any systematic changes in space-time sampling and what is the magnitude of change that these induce, its interannual variations have to be treated with caution. [CLAUDIA STUBENRAUCH, France]	See response to 2-1952
2-1956	2	49	3	49	5	Where is the support for the statement about patterns in satellite cloudiness? [Melissa Free, United States of America]	This statement has been removed.
2-1957	2	49	3	49	5	This statement should be referenced. [Government of United States of America]	This statement has been removed.
2-1958	2	49	3	49	5	Which data analysis came to the conclusion that there is an expansion of subtropical dry zones and an increase in cloud height? Are these results conclusive regarding the range of uncertainty? Could you give a reference? [CLAUDIA STUBENRAUCH, France]	This statement has been removed.
2-1959	2	49	3	49	5	The summary of satellite observations mentions two conclsuions (expansion of subtropical dry zones and an increase in cloud height that have not been mentioned before! [Geert Jan van Oldenborgh, Netherlands]	This statement has been removed.
2-1960	2	49	4			The changes in cloud relating to expansion of the Hadley circulation do not seem toi have an associated reference. Is this Evan et al.? [Richard Allan, United Kingdom]	This statement has been removed.
2-1961	2	49	4			Are there references to support the satellite-based findings of expansion of subtropical dry zones and increase in cloud height? Including this finding in the summary section based on this brief and unsupported discussion seems unwarranted. [Government of United States of America]	This statement has been removed.
2-1962	2	49	4			Is the physical change in cloud height due to an increase in upper tropospheric lapse rates the result of warming troposphere and cooling stratosphere? [Government of United States of America]	No change suggested.
2-1963	2	49	7	49	24	This is not a good section and deals only with land precipitation. It needs work. [Kevin Trenberth, United States of America]	Ocean precipitation is assessed in Chapt 3.
2-1964	2	49	9	49	9	The opening statement should be expanded. It states that trends in the hydrological cycle are uncertain, as concluded in AR4. This is not a helpful statement in isolation, as all climate trends are uncertain to some degree or other. What the reader might wish to know is whether trends in hydrological cycle components are any less uncertain as assessed in AR5 than they were in AR4. If yes (and for reanalysis we can identify improvement from one generation to the other), then fine. If no, then it should be clarified whether this is due to continuing data inadequacies, lack of progress of research into the topic, or newly observed behaviour of the atmosphere (such as the low-level relative humidity shift over land) that complicates the picture. [Adrian Simmons, United Kingdom]	We will consider adding a statement to that effect.
2-1965	2	49	9	49	15	It would be adequate to also insert here in this summary 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 page 2-41, lines 33-43). [Alice Grimm, Brazil]	We will consider adding a statement to that effect.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1966	2	49	9	49	15	Please integrate the use of the calibrated uncertainty language into this paragraph. [Thomas Stocker/ WGI TSU, Switzerland]	The new version includes calibrated language.
2-1967	2	49	9	49	24	Section 2.5.8: This section could benefit by including results for all the components mentioned and a discussion of whether they are consistent with one another in a warming climate. [Government of United States of America]	Agree, will revise.
2-1968	2	49	9	49	24	Should mention reference to Chapter 7: Clouds and Aerosols [Jeffrey Taylor, United States of America]	Declined. This was mentioned in an earlier version but removed.
2-1969	2	49	9			"Trends of components of the hydrological cycle are uncertain, as was also concluded in AR4." This seems rather limp and can be removed since it is summarized far better in the next sentence. [Richard Allan, United Kingdom]	Noted. Text has changed
2-1970	2	49	10			There also is large decadal variability. [David Sauchyn, Canada]	Yes, thank you. No action requested.
2-1971	2	49	11	49	12	"Precipitation in the tropics appears to have increased in the last decade…" What last decade? Do you mean 2003 to 2012? The decade in question must be explicitly stated. [John McLean, Australia]	changed to better define period.
2-1972	2	49	11	49	13	Consider qualifying this general statement about precipitation increases in the tropics, given high spatial variation in precipitation trends and recent reports of exceptional "once-in-a-century" droughts in the Amazon in 2005 and 2010 (Marengo JA et al. 2011. The drought of 2010 in the context of historical droughts in the Amazon region. Geophys Research Letters 38, L12703, doi:10.1029/2011GL047436, 2011; Lewis SL et al. 2011. The 2010 Amazon drought. Science 331: 554). [Government of Canada]	will consider this in revising text.
2-1973	2	49	11	49	15	See earlier comments (30ºS-30ºN is not the tropics and I think trends should be computed/discussed only for the period for which records are reliable). [Ileana Bladé, Spain]	There are numerous ways to define the tropics, this is our choice.
2-1974	2	49	13	49	14	Consider qualifying this general statement about precipitation increases in mid latitudes of NH during 1900-2010. Early records may be uncertain (as for high latitudes) and recent exceptional droughts have occurred (e.g., western North America in 2002, Europe in 2003). [Government of Canada]	Accepted. Will add bit about uncertainty in early record and recent droughts.
2-1975	2	49	17	49	17	"...surface, homogeneity-adjusted, radiosonde…" [James Renwick, New Zealand]	Declined. The radiosonde data are homogeneity adjusted and inserting a comma makes it sound like they are not.
2-1976	2	49	17	49	19	The sentence spanning these lines is wrong. It is simply untrue at and near the surface over land. It is inconsistent with what is written section 2.5.5. Enough earlier comments have been made on this point. [Adrian Simmons, United Kingdom]	Accepted, will remove the C-C statement, but leave the assessment of very likely increase.
2-1977	2	49	19	49	20	"Thus water vapour at the surface and through the troposphere has very likely been increasing since the 1970s." This conclusion is contradicted by the 2012 NVAP-M paper discussed in the rows immediately above. The safest conclusion is the conclusion of NVAP-M: "Therefore, at this time, we can neither prove nor disprove a robust trend in the global water vapor data." A suggested revision to the present sentence, which assumes the addition of a mention and citation of the 2012 NVAP-M paper, would be: "In view of the opposing findings from the studies cited here, some of which are only over water, it is not yet possible to assign a trend to the abundance of water vapor in the atmosphere." [Forrest Mims, United States of America]	Declined. The Vonder Haar, NVAP paper did not test for trends, the statement refered to is a caveat in the paper stating that since they didn't even test for trends they cannot prove or disprove a trend. Even though this is an improved version of the dataset it still suffers from inhomogeneity issues, only starts in 1988 and still is not appropriate for detecting long-term temporal changes.
2-1978	2	49	20	49	21	"Clouds observed from the surface also continue to show increases over many land areas (e.g., North America, former USSR, parts of Europe and Australia),…" This seems at odds with the results highlighted in Section 2.5.7.1 (see also #36) [Richard Allan, United Kingdom]	This paragraph has been modified to be consistent with the text in 2.5.7
2-1979	2	49	20	49	21	Warren et al (2007) found no trend for North America during 1971-1996 (page 48, line 18), in contradiction with this statement. Given the evidence it would be cleaner to skip the first part of the sentence and simply say that there does not appear to be a globally consistent (or even latitudinally consistent) trend. [Ileana Bladé, Spain]	See response to comment 2-1978

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-1980	2	49	20	49	21	Warren 2007 indicates no change in North America. Maybe it should read "in parts of North America" to refer to Canada? [Melissa Free, United States of America]	See response to comment 2-1978
2-1981	2	49	20	49	21	For Australia, Jovanovic 2011 says there is no change. [Melissa Free, United States of America]	See response to comment 2-1978
2-1982	2	49	20	49	21	Where is the support for the statement about the former USSR? [Melissa Free, United States of America]	See response to comment 2-1978
2-1983	2	49	23	49	24	Unsupported statements (see earlier comment). [Ileana Bladé, Spain]	This statement has been removed.
2-1984	2	49	23	49	24	Again, no support given for the statement about expansion of tropical dry zones, etc. [Melissa Free, United States of America]	This statement has been removed.
2-1985	2	49	23	49	24	This statement should be referenced. [Government of United States of America]	This statement has been removed.
2-1986	2	49	23	49	24	The expansion of subtropical dry zones and an increase in cloud height are not based on assessed literature in the tekst preceding this statement. [Geert Jan van Oldenborgh, Netherlands]	This statement has been removed.
2-1987	2	49	24	49	24	Again this requires supporting material and citations to be added to Section 2.5.7.2 [David Parker, United Kingdom of Great Britain & Northern Ireland]	This statement has been removed.
2-1988	2	49	26	50	37	We have written an excellent review of statistical methods for extremes which is not incorporated here. Please add: Visser, H. and Petersen A.C., 2012. “Inferences on weather extremes and weather-related disasters: a review of statistical methods”. Climate of the Past 8, p. 265-286. [Hans Visser, The Netherlands]	TAKEN INTO ACCOUNT: This paper has been assessed by the Chapter authors.
2-1989	2	49	26	51	29	Section 2.6 Changes in Extreme Events: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	Noted
2-1990	2	49	26	61	7	It is necessary to assess the overall extremeness. Studies done for the United States and China could be assessed in this section. Karl et al. (Karl, T. R., Knight, R. W. and Easterling, D. R. et al. 1996. Indices of climate change for the United States. Bull. Amer. Meteor. Soc., 77: 279–292) first defined a Climate Extreme Index (CEI) for the contiguous U.S, which took into a comprehensive account the extreme temperature and precipitation indicators. By analyzing the temporal variations of the CEI index, they found that the linear trend is insignificant in 1910-1994, but the decadal fluctuation is noticeable, and a rising trend is quite clear since the mid 1970s (Karl et al., 1996). Later, Gleason et al. (Gleason, K. L., Lawrimore, J. H. and Levinson, D. H. et al. 2008. A revised U.S. climate extremes index. J. Climate, 21(10): 2124–2137.) revised the CEI by increasing data sites and data series length, and by introducing percentile values. The revised CEI series for the contiguous U.S. shows a highly significant rising trend since 1994 and a significant rising trend for period 1910-2006, suggesting that the frequency and intensity of extreme climate events related to temperature and precipitation are increasing over the contiguous U.S. as a whole. The CEI has deficiencies even for the contiguous U.S., of course. For example, local or regional events such as hurricanes and tornados were not taken into consideration. The CEI also gave equal weights to the impacts of the five extreme events. However, the economic and social impacts of different extreme events actually vary from a region or country to another. Abnormally warmness related to winter minimum temperature could cause much less damages to energy, transport and telecommunication sectors than abnormally coldness in the contiguous U.S., for example, and the impacts of extreme climate events related to maximum temperature could not be indifferently treated as those of severe droughts in China. A recent study by Ren et al. (Ren, G.Y., Chen Y., Zou X.K., Zhou Y.Q., Ren Y.Y., Jiang Y., Ren F.M., Zhang Q., Wang X.L., Zhang L., 2011, Change in climatic extremes over mainland China based on an integrated extreme climate index, Climatic Research, 50 (1-2): 113-124. doi: 10.3354/cr01023) developed an integrated extreme climate index (IECI), and analyzed the linear trend of the index series in mainland China over the time period 1956-2009. It was composed of seven individual extreme indicators, i.e. the country-averaged frequencies of high temperature, low temperature, intense precipitation, dust storms and strong wind events, meteorological drought area percentage, and number of land-falling tropical cyclones (TCs). Weights were assigned to the individual country-averaged extreme indices based on the mean losses of economy and casualties induced by the extreme events in recent 5 years. The IECI was then defined as the weighted sums of the standardized individual extreme climatic indices. They showed an insignificant linear trend of the IECI	TAKEN INTO ACCOUNT: A reference to CEI has now been included in Box 2.4.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						series during the past 50 years, in spite of a large inter-annual and inter-decadal variation, implying that the frequencies of the major extreme climate events with potential to cause severe weather disasters in the country witnessed more complicated changes in the period of an accelerated climate warming. [Guoyu Ren, China]	
2-1991	2	49	26	61	9	Section 2.6 is too heavily influenced by a un-published, non-peer reviewed report (SREX). This is unusual for IPCC reports. The IPCC reports are supposed to be based on peer-reviewed original studies, not based on another assessment report. [Aiguo Dai, United States of America]	REJECT: We disagree that this assessment relies too heavily on SREX. In addition we have assessed many publications that have been written after SREX. It is also incorrect to say that SREX is unpublished or non-peer reviewed. This assessment has been more heavily reviewed than most original publications. It seems inappropriate to re-do an assessment where we agree with the assessment that was performed by SREX.
2-1992	2	49	26			Section 2.6: the same definition of “extreme events” as in Chapter 1, p. 11, l. 13 should be used! [Dietrich Feist, Germany]	TAKEN INTO ACCOUNT: Cross chapter discussions with Chapter 1 have led to the removal of this section and reference to Chapter 1 and the glossary have been included.
2-1993	2	49	26			Section 2.6: The datasets in this section are not described well and clearly the quantity and quality of data change over time, but this seems not to get into the analyses. The figures 2.31, 2.32 and 2.33 are too small and impossible to read. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: The dataset descriptions have been revised and figures updated. Information on the datasets and comparisons has now been placed in an Appendix.
2-1994	2	49	26			Section 2.6: Extremes. Overall this section seems rather lengthy and the authors are encouraged to condense the material where possible, without losing necessary detail. This was most notable for temperature extremes (2.6.1). [Thomas Stocker/ WGI TSU, Switzerland]	AGREED: Much of the regional detail has been moved into a table to make this section shorter and more readable.
2-1995	2	49	28	49	37	It would be adequate to insert somewhere in this introduction a comment on the impact of climate variability on the frequency of extreme events, which suggests that also climate change might have impact on this frequency. See, for instance: Gershunov, A. and T. P. Barnett, 1998: ENSO influence on intraseasonal extreme rainfall and temperature frequencies in the contiguous United States: Observations and model results. J. Climate, 11, 1575-1586. Cayan, D. R., K. T. Redmond, and L. G. Riddle, 1999: ENSO and hydrologic extremes in the western United States. J. Climate, 12, 2881-2893. Grimm, A. M. e R. G. Tedeschi, 2009: ENSO and extreme rainfall events in South America. J. Climate, 22, 1589–1609. DOI: 10.1175/2008JCLI2429.1 [Alice Grimm, Brazil]	NOTED. Indeed some of the impact of climate variability e.g. ENSO, PDO and AMO is already discussed in section 2.6.1. Detail on regional changes is also discussed further in Ch 14.
2-1996	2	49	29	49	23	In the previous paragraph it was concluded that "there remains substantial ambiguity in surface observations of global-scale cloud variability and trends and what trends do exist are likely to be within the range of uncertainties of the surface observations", so one should not mention the trends here as being real. [Geert Jan van Oldenborgh, Netherlands]	The discussion of cloud trends in this paragraph has been re-written. See response to 2-1978
2-1997	2	49	40	49	40	The reference to error here is quite different to earlier references. This is spot (random) errors or spikes. Worth making this clear. Random observation errors hardly matter for mean climate, but they matter for extremes, as the authors note. [Government of Australia]	TAKEN INTO ACCOUNT: This sentence has been amended accordingly
2-1998	2	49	40	49	40	"...are likely to especially affect the statistics of extremes and…" [James Renwick, New Zealand]	TAKEN INTO ACCOUNT: This sentence has been amended accordingly
2-1999	2	49	40	49	40	"likely to …", perhaps replace with non-reserved word? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: This sentence has been amended accordingly
2-2000	2	49	41	49	41	Conversely, many real extremes have been considered errors and have been removed in the Quality Control process. [Geert Jan van Oldenborgh, Netherlands]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2001	2	49	53	49	53	We recommend that the definition of return values and return periods in the glossary in annex III is improved and made easier to understand e.g. use language from SREX glossary. [Government of NORWAY]	TAKEN INTO ACCOUNT. Glossary definition has been reassessed.
2-2002	2	49	55	49	55	Add "Wang et al., 2012e; Wang et al., 2012f" after "Schar et al. 2004", because these new studies are about historical changes in temperature extremes as inferred from extreme value distribution analysis (using a GEV-tree that allows choices of stationarity or linear or nonlinear trends in the parameters) [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: These papers has been assessed by the Chapter authors.
2-2003	2	50	5	50	2	there there -> there [Mihai Dima, Romania]	EDITORIAL: amended
2-2004	2	50	5	50	5	It is rather meaningless to seek a "consistent definition" as an extreme event relates to the climate of a particular region and so it must vary with space, time, parameter and sector. Suggest rephrasing this sentence to make this point. [Government of Australia]	TAKEN INTO ACCOUNT: This refers to the ability to be able to compare extremes consistently across regions realising that there will be regional definitions for some extremes. The text has been amended accordingly.
2-2005	2	50	5	50	5	One too many "there" in this sentence. [Birgit Hassler, United States of America]	EDITORIAL: amended
2-2006	2	50	5	50	5	The word “there” is repeated unnecessarily. [Andrew King, Australia]	EDITORIAL: amended
2-2007	2	50	5	50	5	"consistent", perhaps rephase to "unique"? [Xuebin Zhang, Canada]	ACCEPTED: Text amended accordingly
2-2008	2	50	8			Maybe the point is too obvious, but perhaps it should be pointed out that if percentage thresholds are used they need to be defined relative to a fixed past period, say 1961-1990 or 1981-2010, not a period that shifts every thirty (or ten) years, even if for some user-oriented purposes climate statistics do need to be defined for the latest thirty (or ...) years [Adrian Simmons, United Kingdom]	ACCEPTED: Text amended accordingly
2-2009	2	50	17	50	17	which, for example, are investigating → awkward [Ileana Bladé, Spain]	EDITORIAL: amended
2-2010	2	50	17	50	17	The word “of” is required between “use” and “Extreme” [Andrew King, Australia]	EDITORIAL: amended
2-2011	2	50	19	50	19	Add Maraun et al., 2011, Sillmann et al., 2011 to the reference list. They were among the first to use EVT with covariates in climate science! Citations: Maraun, D., et al. (2011): The influence of synoptic airflow on UK daily precipitation extremes. Part I: Observed spatio-temporal relationships. CLIMATE DYNAMICS 36(1-2): 261-275, DOI: 10.1007/s00382-009-0710-9; Sillmann, J., et al. (2011): Extreme Cold Winter Temperatures in Europe under the Influence of North Atlantic Atmospheric Blocking. JOURNAL OF CLIMATE 24(22): 5899-5913, DOI: 10.1175/2011JCLI4075.1 [Douglas Maraun, Germany]	TAKEN INTO ACCOUNT: The authors have assessed these manuscripts for inclusion.
2-2012	2	50	22			Box 2.4, Table 1: Common definitions for extremes indices in the scientific literature: Table was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	NOTED
2-2013	2	50	30	50	30	Something wrong with the reference citation ('G') here and in the reference list. [Peter Burt, United Kingdom]	ACCEPTED: Reference amended.
2-2014	2	50	30	50	30	Fix last name in reference "G. et al., 2011" [Birgit Hassler, United States of America]	ACCEPTED: Reference amended.
2-2015	2	50	30	50	31	Hamza et al. (2012) on sub daily analysis may be added. [Government of India]	TAKEN INTO ACCOUNT: Given the limited information the authors were unable to find this reference.
2-2016	2	50	30	51	29	Section Box 2.4: Extremes Indices: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	NOTED
2-2017	2	50	31	50	31	Citation should be Hardwick Jones et al. 2010. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference amended.
2-2018	2	50	32	50	32	What is meant by 'physical impacts' here? [European Union]	TAKEN INTO ACCOUNT: Text amended
2-2019	2	50	35	50	37	A number of drought indices related to ecological functions have been developed over the past years and used in assessments of regional, continental and hemispheric trends. These include fire weather and behaviour indices. For instance, those studies on the Keetch–Byram Drought Index (KBDI) by Groisman et al.	REJECTED: Beyond the scope of WG1 as it refers to impacts indices which belong in WG2.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2007, Fire Weather Index system components by Amiro et al. (2004), Girardin and Wotton (2009), and Girardin et al. (2009). These indices are better adapted to ecological processes than indices described in this section. Groisman PYa, Sherstyukov BG, Razuvaev VN et al. (2007) Potential forest fire danger over Northern Eurasia: changes during the 20th century. Global and Planetary Change, 56, 371–386. Amiro BD, Logan KA, Wotton BM, Flannigan MD, Todd JB, Stocks BJ, Martell DL (2004) Fire weather index system components of large fires in the Canadian boreal forest. International Journal of Wildland Fire, 13, 391–400. Girardin, M.P., Ali, A.A., Carcaillet, C., Mudelsee, M., Drobyshev, I., Hély, C., Bergeron, Y. 2009. Heterogeneous response of circumboreal wildfire risk to climate change since the early 1900s. Global Change Biology 15, 2751–2769, doi: 10.1111/j.1365-2486.2009.01869.x [Government of Canada]	
2-2020	2	50	35	50	37	Dryness indices based on simulated soil moisture should be mentionned here, like soil moisture percentiles (Sheffield et al., 2009) and Standardized Soil Wetness Index (SSWI, Vidal et al., 2010). - 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: The Chapter authors have assessed the inclusion of these papers. Noted in this box are that indices referenced are those which are very common in the literature and we specifically state that we are excluding indices using other climate variables (except temperature and precipitation).
2-2021	2	50	51	50	51	Orphaned table header [Peter Burt, United Kingdom]	NOTED
2-2022	2	51	2	51	2	Distributed is an odd word to use. Better to say 'not always freely available'. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED. Text amended
2-2023	2	51	2	51	3	While this might be a pragmatic reason to use indices instead of raw data, it is not an "advantage". The results are not subject to tests of reproducibility. This statement merely serves to underscore the stupidity of national policies that do not make environmental data freely available to researchers. [Dian Seidel, United States of America]	NOTED: Text has been amended to take this into account.
2-2024	2	51	4	51	4	Comparability → comparison [Ileana Bladé, Spain]	EDITORIAL
2-2025	2	51	4	51	4	Peterson and Manton 2008. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED.
2-2026	2	51	5	51	6	Referencing needs to be corrected as years are not attached to author names. [Andrew King, Australia]	ACCEPTED.
2-2027	2	51	6	51	6	Year of publication missing from the Peterson and Manton publication referenced here [Government of Australia]	ACCEPTED.
2-2028	2	51	8	51	12	Can you give and example of an impact application for an absolute annual index? [European Union]	REJECTED: Beyond the scope of WG1 which does not discuss particular impacts. This is more appropriate for WG2.
2-2029	2	51	26	51	26	Is panel c of Figure 1, Box 2.4, discussed anywhere? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Yes this sentence refers to all panels of Box 2.4. Figure 1.
2-2030	2	51	27	51	27	Is the low-pass filtering the same as in Figure 2.28? Please indicate so. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: No the filtering process is different.
2-2031	2	51	33	54	7	Please consider including the result of the following study which shows that the return period of extreme minimum (maximum) temperature in Hong Kong has lengthened (shortened) significantly in the past century: - Wong, M. C , H. Y. Mok, and T. C. Lee, 2010: Observed changes in extreme weather indices in Hong Kong. Int. J. Climatol.. Published online in October 2010, doi:10.1002/joc.2238, 12 pp. [Sai Ming Lee, Hong Kong, China]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2032	2	51	35	51	35	There may be scope for acknowledging that most of the extremes results reported are based either on	TAKEN INTO ACCOUNT: While this is not strictly true

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						unhomogenised data (and are hence potentially influenced by the issues described in section 2.4.1.3), or homogenised only at the monthly timescale, which may not fully homogenise extremes (Trewin 2012). Nevertheless the consistency of the results suggests that data homogeneity has only a limited impact on the confidence level which can be placed on changes reported here. [Government of Australia]	(HadEX2 primarily uses either only records that have been identified as homogeneous or data that have mostly been homogenised at daily timescales e.g. Trewin, 2012), we agree that some additional text is required to highlight some of these issues but due to space restrictions this was unable to be added to the main text. Therefore some additional information is added to an Appendix.
2-2033	2	51	44	51	45	Stating that some change has been observed "since about 1950" is so vague and general as to be pointless. Where's the description of extreme events over the last 15 years and the associated discussion of the drivers? The 2003 European heatwave was sensibly described in 4AR so what's the difficulty in describing the specific rather than the general? [John McLean, Australia]	REJECTED: Such a discussion is outside the scope of Chapter 2. We maintain that this statement is a true reflection of the peer-reviewed literature. It would not be possible to describe all of the extreme events that have occurred since the publication of AR4.
2-2034	2	51	46	51	46	Change "also show warming" to "are consistent with warming". [Government of Australia]	EDITORIAL
2-2035	2	51	46	51	46	"...cold and warm days are also consistent with warming…" [James Renwick, New Zealand]	EDITORIAL
2-2036	2	51	46	51	46	Seneviratne et al. 2012b, the SREX chapter 3 has been referenced as 2012a and 2012b [Xuebin Zhang, Canada]	ACCEPTED: Only one reference now appears.
2-2037	2	51	46	51	48	Can you give an example of such regions (where ENSO tends to dominate maximum temperature variability)? [European Union]	TAKEN INTO ACCOUNT: This sentence has been revised.
2-2038	2	51	52	51	54	This agreement over the overlapping period is not surprising. It is, however, important to point out that in discussing these four indices, Alexander et al. (2006, pages 7 & 8) asserted the following: “Changes in all these percentile-based indices seem to have occurred around the mid 1970s which corresponds with changes in mean global temperature [Folland et al., 2001]. Indeed every year since 1979 has been above the long-term average for the annual occurrence of warm nights and every year since 1977 has been below the long-term average for the annual occurrence of cold nights.” [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2039	2	51	52	51	54	This is a clear indication that a structural change occurred in each of these time series and therefore an unsegmented linear regression model for the entire 1951-2003 period is inappropriate. It is important to note that this sudden change in the mean global temperature is generally attributed to the shift of the Pacific Decadal Oscillation from the cold phase to the warm phase in 1976/1977. Recall that there was a slight decreasing trend in mean global temperature from the early 1940s to the mid-1970s. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2040	2	51	52	51	54	Segmented (piecewise) discontinuous regression models are therefore more appropriate for quantifying the trends observed in these four indices. To illustrate the use of a segmented linear regression model, the HadEX data series for TN90p anomalies for the period 1951-2003 is used. (This data set was kindly provided by lead author, Dr. Lisa Alexander). [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2041	2	51	52	51	54	A visual inspection of this time series shows a slight decreasing linear trend until the mid-1970s after which there is a pronounced increasing linear trend. There seems to be a change in both the magnitude and the sign	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of the slopes before and after the mid-1970s. A quick and easy exploratory procedure to check for linearity over the entire 1951-2003 period is through the computation of Tukey's half-slope ratio. If the ratio is reasonably close to 1, then a regression line for the entire period may be appropriate. If not, it is inappropriate. The computed half-slope ratio for TN90p was found to be 4.7 which is obviously far from 1. [Reynold Stone, Trinidad and Tobago]	although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2042	2	51	52	51	54	A visual inspection of the residuals showed a curved pattern and therefore supports the conclusion from Tukey's half-slope ratio. This further indicates that the use of an unsegmented regression model over the entire 1951-2003 period is inappropriate. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2043	2	51	52	51	54	Two change-point tests for a change in slope were conducted to identify the year after which the change in slope occurred. The first test is based on the generalized fluctuation test framework (the CUSUM test) while the second is based on the F test framework (a Chow-type test). These two tests are available in the R package strucchange – a popular package used to test for structural changes in linear regression models. Both tests identified 1976 as the statistically significant ($p<0.001$) change point. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2044	2	51	52	51	54	This confirms the need to use separate linear regression equations for the 1951-1976 and the 1977-2003 periods. For illustrative purposes, the OLS regression equations constituting a segmented discontinuous model were derived although other resistant/robust regression methods could also be used and are expected to yield similar results. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2045	2	51	52	51	54	The OLS equations are: [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2046	2	51	52	51	54	For the period 1951-1976: [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							chapter.
2-2047	2	51	52	51	54	Y1 = 278.9 - 0.1439 X1, (p < 0.10) … (1) [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2048	2	51	52	51	54	For the period1977-2003: [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2049	2	51	52	51	54	Y2 = -1890.0 + 0.9553 X2, (p < 0.001) … (2) [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2050	2	51	52	51	54	Where, Y and X are the warm night anomalies and year respectively. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2051	2	51	52	51	54	Equation 1 shows a slight decreasing linear trend from 1951-1976 which is statistically significant at the 10% level. Equation 2 shows a pronounced increasing linear trend which is statistically significant at the 0.1% level. The Chow test for a change in slope in Equations 1 and 2 yielded a statistically significant F value of 20.8 (p <0.001). [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2052	2	51	52	51	54	The two equations above represent a segmented regression model for TN90p over the period 1951-2003. Several tests of randomness (runs above and below the median test, Bartels test, rank test, turning point test and the difference sign test) were carried out on the residuals. The null hypothesis of randomness could not be rejected at the 5% level. The lag 1 auto correlation of the residuals was found to be -0.067 which is not	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						statistically significant at the 5% level and therefore not significantly different from zero. [Reynold Stone, Trinidad and Tobago]	to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2053	2	51	52	51	54	After analyzing the HadEX TN90p data above, Alexander et al. (2006) reported that globally the number of warm nights increased by about 25 days since 1951 but later emphasized that the change actually occurred from the around the mid-1970s coinciding with the changes in mean global mean temperature. In other words, the increase in the number of warm nights was confined to the mid-1970s to 2003 period. They neglected to explore the trend or variability over the period spanning 1951 to mid-1970s. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2054	2	51	52	51	54	The results of the analyses above show that the number of warm nights decreased by about 4 days for the period 1951-1976, experienced a positive jump of about 3 days in 1976/1977, and subsequently increased by about 26 days over the period 1977-2003. Thus, although the data show a net increase of 25 warm nights over the entire period 1951-2003, in agreement with the finding of Alexander et al. (2006), the details of the distribution of these changes over the entire 1951-2003 period have now been specified for the first time. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2055	2	51	52	51	54	It is therefore advisable that analyses similar to those above on the HadEX TN90p data set be carried out on TN10p, TN90p, TX10p and TX90p for the HadEX2, HadGHCND and GHCNDEX data sets so as to better quantify the distribution of changes in these indices over the 1951-2010 period. This is absolutely necessary so as to provide a better understanding of the trends and variabilities in these indices over the last 60 years. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2056	2	51	57	52	1	The trend estimates of the four indices for the three data sets shown in Table 2.11 are deemed to quantify the changes for the period 1951-2010. However, the calculation of these trends assumes linearity over the entire 1951-2010 period. As was pointed out previously, a visual inspection of Figure 2.32 on page 177 clearly shows this not to be the case. The TN10p and TX10p data series initially seem to fluctuate randomly about some constant mean until the mid-1970s after which they both experience abrupt decreasing linear trends. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.
2-2057	2	51	57	52	1	On the other hand, the TX90p and TX90p series initially seem to exhibit slight decreasing linear trends until the mid-1970s after which they abruptly experience pronounced increasing linear trends. In all four cases, therefore, unsegmented linear regression models over the entire 1951-2010 period are inappropriate because of the presence of structural changes in the data series. The trend estimates in Table 2.11 are therefore misleading. Separate trend estimates need to be determined for the 1951 to mid-1970s and the mid-1970s to 2010 periods. [Reynold Stone, Trinidad and Tobago]	NOTED: The Alexander et al. 2006 paper refers to a different dataset than those being shown here although the reviewer is correct that the datasets generally agree over the overlapping period. However to address the changes in the timeseries noted by the reviewer (in these 20 comments) we also include trend estimates from 1979-2010 which also agrees with the standard periods that we are using in this chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2058	2	51		53		The various suggestions of a significant difference between trends in tmax and tmin extremes is not supported by the data (taking into account the error ranges) nor Figure 2.32. [Government of Australia]	TAKEN INTO ACCOUNT: The suggestion is not that differences are "significant" but rather that differences exist which is clear from Fig. 2.32 and Table 2.11. The paragraph has been amended accordingly.
2-2059	2	51				Section 2.6.1: in the earlier section on land temperature trends (2.4.1) it is noted that there are problems with max and min temperatures leading to a reduction in confidence concerning changes in DTR. Presumably the same considerations - particularly the discussion of problems with minimum temperatures - affect the assessment of extremes, but there is no discussion of this in this section. Homogeneity is not discussed either. It is my understanding that many of the daily series used to calculate extremes are not homogenised, or are homogenised in a very basic way by excluding obviously inhomogeneous stations or sections of data. These uncertainties need to be acknowledged and their effect on the confidence one can have in the conclusions based on these data sets needs to be assessed. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: The section has been amended to reflect the issues surrounding DTR. Some mention is made to the level of homogeneity between datasets (in some cases this is high and is done on daily timescales). Despite differences in the level of quality control and homogeneity between datasets, the level of agreement on global scales is very high.
2-2060	2	52	3	52	3	This is a very weak opening sentence; add “that exceed the uncertainty range” (otherwise there is nothing remarkable about measurement differences). [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: In fact differences are not outside uncertainty ranges but structural uncertainties are important as highlighted in Box 2.1. We have amended this paragraph accordingly.
2-2061	2	52	3	52	47	There needs to be an explanation of how max and min temperatures can be used here but not in the DTR section? [Kevin Trenberth, United States of America]	AGREED: This section, the DTR section and the executive summary have been amended to reflect the issues surrounding the calculation of DTR and differences between this and the use of extremes of maximum and minimum temperatures.
2-2062	2	52	3	53	5	There should be reference to Hansen et al PNAS 2012 "perception of climate change" and consideration given to reproducing Fig 4 and others from that paper. There are also popularized versions of that figure that might serve well in the FAQ.in place of FAQ 2.2, for instance. p 2-60 [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion. However a figure has not been included as this study uses monthly data.
2-2063	2	52	13	52	13	Insert 'in' before 'daytime' [Peter Burt, United Kingdom]	EDITORIAL
2-2064	2	52	17	52	18	Can you briefly explain why this (asymmetrical shift in minimum verses maximum temperatures) is different to the changes in Diurnal Temperature Range discussed in Section 2.4.1.3? [European Union]	TAKEN INTO ACCOUNT: This section, the DTR section and the executive summary have been amended to reflect the issues surrounding the calculation of DTR and differences between this and the use of extremes of maximum and minimum temperatures.
2-2065	2	52	20	52	28	It seems a bit strange to report this level of regional detail for minimum temperatures but say nothing about maxima. If the results for hot and cold days are generally weaker than those for nights (as suggested by earlier discussion), then say so. [Government of Australia]	AGREED: Text amended accordingly
2-2066	2	52	20	52	37	Where's the discussion of causes? The Australian Bureau of Meteorology (BoM) has released media statement about several heat waves in Australia in the last few decades and the common theme is stationary or quasi-stationary pressure cell. Natural causes can account for heatwaves so they should be removed and excluded from any discussion that attempts to attribute heatwaves to human activity. Example statements from the BoM "Perhaps the most important synoptic feature during the month, and the one responsible for Victoria's heatwave, was a blocking high pressure system in the Tasman Sea which developed from about the 26th. The system directed a northerly air flow over the southeast of Australia, moving an extremely hot air mass over the southeast of the continent." (Monthly Weather Review, Victoria, Jan 2009) and "As the high pressure system entered the Tasman Sea it slowed dramatically and became nearly stationary during the next week, 7th to 14th, bringing heatwave conditions over the vast majority of South Australia." (Monthly Weather Review, South Australia, Nov 2009) Also, we shouldn't forget "The 2003 heat wave was associated with a very robust and persistent blocking high-pressure system that may be a manifestation of an exceptional northward extension of the Hadley Cell (Black et al., 2004; Fink et al., 2004)." IPCC 2007 report, WGI contribution, Chapter 3, Section 3.8.4 (Box 3.6) [John	REJECT: Discussion of attribution is outside the scope of Chapter 2

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						McLean, Australia]	
2-2067	2	52	21	52	21	According to the literature from You et. al., all the stations covered by this literature are located within China. It is recommended to replace “China and the Tibetan Plateau (You et al., 2010),…” with “China (You et al., 2010),…”. [Government of China]	TAKEN INTO ACCOUNT: A regional table has been developed to reduce text and reference to the Tibetan Plateau has been removed.
2-2068	2	52	21	52	21	Wang et al. (2012) represented a comprehensive overview of the fact, simulation and projection of extreme climate events over China. This work should be cited after “You et al., 2010”. [References: Wang, H., Sun, J., Chen, H., Zhu, Y., Zhang, Y., Jiang, D., Lang, X., Fan, K., Yu, E., and Yang, S., 2012: Extreme climate in China: Facts, simulation and projection. Meteorologische Zeitschrift, 21: 279–304.] [Dabang Jiang, China]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2069	2	52	21	52	21	Following publications for China would be relevant and you would probably like to cite: 1. Ren, G.Y., Chen Y., Zou X.K., Zhou Y.Q., Ren Y.Y., Jiang Y., Ren F.M., Zhang Q., Wang X.L., Zhang L., 2011, Change in climatic extremes over mainland China based on an integrated extreme climate index, Climatic Research, 50 (1-2): 113-124. doi: 10.3354/cr01023; 2. You, Q., G. Ren, K. Fraedrich, S. Kanga, Y. Ren and P. Wang, 2012, Winter temperature extremes in China and their possible causes, Int. J. Climatol. (2012). DOI: 10.1002/joc.3525; 3. Zhang, H.,P. Zhai. and H. Tang. 2011. Spatial and temporal changes of hourly precipitation over Southwest China during 1961-2000. Advance in Climate Change Research. 7 (1): 8-13; 4. Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053 [Guoyu Ren, China]	TAKEN INTO ACCOUNT: A regional table has now been produced and the authors have assessed these manuscripts for inclusion.
2-2070	2	52	21	52	21	"China and the Tibetan Plateau", but You et al. 2010, 2008 only discussed China portion of the Tibetan Plateau so it is not correct to say China and the Tibetan Plateau. [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: A regional table has been developed to reduce text and reference to the Tibetan Plateau has been removed.
2-2071	2	52	22	52	22	Trewin and Smalley 2012 is a more recent reference for Australia. [Government of Australia]	TAKEN INTO ACCOUNT: This manuscript did not meet the IPCC submission/publication deadline
2-2072	2	52	22	52	22	Add "Wang et al., 2012e" after "Alexander and Arblaster, 2009", because this is a new study on Australian temperature extemes. [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: A regional table has now been produced and the authors have assessed this manuscript for inclusion.
2-2073	2	52	24	52	24	reference "de los Milagros Skansi", Skansi is the surname and de los Milagros is part of her name [Moira Evelina Doyle, Argentina]	ACCEPTED. Text amended
2-2074	2	52	24	52	24	Add "Wang et al., 2012f" after "Peterson and Manton, 2008" because this is a new study about Canadian temperature extremes. [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: This paper did not meet the IPCC paper acceptance deadline.
2-2075	2	52	24	52	25	There is a geographically more comprehensive study on trends in extreme temperature indices for South America that 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: The authors have assessed this manuscript for inclusion. A more recent study by Skansi et al. 2013 now exists.
2-2076	2	52	25	52	25	Add the following or something similar before "Some regions…": For Australia and Canada, an extreme value distribution analysis has also revealed a much stronger and more extensive warming in extreme low minimum temperatures than in extreme high maximum temperatures (20-year return values) over the past century (Wang et al., 2012e and 2012f). [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion although the 2012f paper did not meet the IPCC paper acceptance deadline.
2-2077	2	52	25	52	28	Would suggest adding Caesar et al. 2011 in addition to Choi et al., 2009 for changes in the Asia Pacific region. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: A regional table has now been produced and the authors have assessed this manuscript for inclusion.
2-2078	2	52	25	52	28	Does it need clarifying over what period a doubling is occurring, even approximately e.g. "over the recent observational period" or similar? [John Caesar, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED. Text amended accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2079	2	52	31	52	31	Append the following after line 31 “Over India, during pre-monsoon (March - May), the frequency of occurrence of hot days and hot nights showed widespread increasing trend, while that of cold days and cold nights has shown widespread decreasing trend for the period 1970-2005 (Kothawale, 2011). Revadekar et al., (2012) found that observed trends in annual indices of temperature extremes at low altitude locations in South Asia suggest that these sites can generally expect future changes in temperature extremes that are consistent with broad-scale warming. However, high-elevation sites appear to be more influenced by local factors. Dash and Mamgain (2011) broadly suggest warming trend over India during winter, with significant decrease in the frequency of occurrence of cold nights in the winter months in India and in its homogeneous regions in the north ,except in the western Himalaya; southern regions show a drastic decrease in the frequency of cold nights". [Government of India]	REJECT: This detailed description of regional trends, causes and projections is outside the scope of Chapter 2. These details are considered in Ch 10, Ch 12 and Ch 14. We have however assessed the noted literature for inclusion.
2-2080	2	52	33	52	34	I cannot pass on using Meehl et al in any way. I reconstructed this analysis for the US for two recent congressional hearings, but I did it correctly - using only those stations with at least 80 years of data. The results show the 1930s far exceeded any recent period (even with the summer of 2012) regarding the number of record warm temperatures. Meehl et al. is extremely misleading and should not be cited without stating what happens when the time series is extended back before 1930. (See Christy 2012 Senate EPW 1 Aug 2012 and Christy 2012 House Subcommittee on Energy and Power 20 Sep 2012.) If the comment is left as is, this will be another relatively easy target about which to criticize the IPCC content. [John Christy, United States of America]	TAKEN INTO ACCOUNT: This section has been amended significantly with regional trends and assessments being placed in a new table. While we agree that for the USA the 1930s stands out, this is not necessarily the case for other regions with long records e.g. Europe. Our assessment in the new regional table is based on our assessment of many regional studies although we do highlight that the 1930s dominates the long-term USA record
2-2081	2	52	33	52	34	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. Data ref: http://www.ncdc.noaa.gov/oa/pub/data/special/maxtemps.pdf , http://www.ncdc.noaa.gov/oa/pub/data/special/mintemps.pdf [Richard Keen, United States of America]	TAKEN INTO ACCOUNT: This section has been amended significantly with regional trends and assessments being placed in a new table. While we agree that for the USA the 1930s stands out, this is not necessarily the case for other regions with long records e.g. Europe. Our assessment in the new regional table is based on our assessment of many regional studies although we do highlight that the 1930s dominates the long-term USA record
2-2082	2	52	34	52	34	Trewin and Smalley 2012 is a more recent reference than Trewin and Vermont 2010. The results still hold. [Government of Australia]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion however it did not make the IPCC publication deadline and therefore cannot be included.
2-2083	2	52	39	52	39	The Diurnal Temperature Range has increasing trends over many high altitude stations in South Asia (Revadekar et al., 2012). [Government of India]	TAKEN INTO ACCOUNT: DTR trends are discussed in Section 2.4.1.3.
2-2084	2	52	42	52	43	Does this mean minimum temperatures have increased even at those locations? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Not necessarily but this is clear from the following sentence.
2-2085	2	52	43	52	47	If "the so-called ‘warming hole’ in central North America and eastern USA, where temperatures have cooled relative to the significant warming elsewhere on the continent, has been ascribed to ... decadal variability linked with the Interdecadal Pacific Oscillation", then the "warming hole" over northwestern North America (Alaska) should also be ascribed to the PDO. Then it follows that the PDO is the largest factor affecting climate change over the entire continent. See Hartmann and Wendler references below. [Richard Keen, United States of America]	TAKEN INTO ACCOUNT: The point of this sentence was not to discuss the drivers over the whole continent but rather to provide some explanation of what might be driving the "warming hole" occurring over the region mentioned.
2-2086	2	52	44	52	44	relative to → compared to [Ileana Bladé, Spain]	EDITORIAL
2-2087	2	52	46	52	46	Title of Portmann et al. 2009 is Spatial and seasonal patterns in climate change, temperatures, and precipitation across the United States. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference amended.
2-2088	2	52	53	52	53	Consider hatching the non-significant areas instead (Figure 2.32) [Ileana Bladé, Spain]	NOTED
2-2089	2	52	53	52	53	Avoid splitting legend across two pages [Peter Burt, United Kingdom]	NOTED

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2090	2	53	1	53	53	There should be reference to Hansen et al 2012 in PNAS. [European Union]	ACCEPTED: This paper had not been published at time of writing the Second Order Draft. This paper has now been assessed for inclusion by the chapter authors.
2-2091	2	53	7	53	14	This observation is based on station records. At least part of this change can be explained by the increasing surface development around the stations as shown in a number of studies (see comments on DTR). [John Christy, United States of America]	TAKEN INTO ACCOUNT: This assessment takes account of this increasing surface development.
2-2092	2	53	9			It might be better to say "where frosts occur" or "where frosts can be observed" rather than "where frosts can be defined". The WMO glossary definition of a frost is: "The condition which exists when the temperature near the Earth's surface and Earth-bound objects falls below freezing (0 °C or 32 °F)", and as such is not linked to particular geographic regions. [Adrian Simmons, United Kingdom]	EDITORIAL
2-2093	2	53	10	53	10	For frost days in Asia, you probably hope to cite a recent paper analyzing changes in temperature extremes in mainland China by Zhou and Ren (Zhou, Y.Q. and Ren, G.Y., 2011, Change in extreme temperature events frequency over mainland China during 1961-2008, Climate Research, 50 (1-2): 125-139. doi: 10.3354/cr01053) [Guoyu Ren, China]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2094	2	53	20	53	22	Not sure what point is being made here; perhaps clarification is needed. [Ileana Bladé, Spain]	NOTED
2-2095	2	53	24	53	28	Compo et al. 2011 (Quart. J.) have an excellent study showing no changes in the major climate modes since the 19th century that produce the quasi-stationary features - this should be mentioned. [John Christy, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion - it appears in Section 2.7.
2-2096	2	53	24	53	38	Please consider mentioning the link between atmospheric blocking and extreme weather in mid-latitudes and the evidence of globally increasing blocking occurrence in recent decade. Reference: Francis, J. A., and S. J. Vavrus (2012), Evidence linking Arctic amplification to extreme weather in mid-latitudes, Geophys. Res. Lett., 39, L06801, doi:10.1029/2012GL051000. The Global Increase in Blocking Occurrences, 20th Conference on Climate Variability and Change / 88th Annual Meeting of the American Meteorological Society, 19-24 January 2008, New Orleans. [Sai Ming Lee, Hong Kong, China]	NOTED: This belongs in Section 2.7
2-2097	2	53	24	53	52	This section discusses heat waves / warm spells but some important recent studies are not discussed at all. First of all, there is Hansen et al, PNAS (2012), who showed that seasonal heat extremes have increased strongly since the 1960s. Then there is Duffy and Tebaldi, Clim Ch, 2012 showing greatly increased frequency in extreme summer-temperatures in the US. Next, in Coumou et al, Clim Ch (minor rev) [I have send this paper to the TSU months ago to have it distributed to the authors of the relevant chapters] we show that record-extremes in monthly temperature world wide have strongly increased as well (5-fold). Based on basic statistics one also expects extremes in time-aggregated datasets (ie monthly or seasonal-averages) to increase more strongly than extremes in daily datasets. This is due to improved signal to noise ratio (see eg Rahmstorf and Coumou, PNAS, 2011). This means that multi-days heat extremes should increase more strongly than daily extremes and this is also exactly what is observed. It is therefore very surprising to see that the confidence level in the observed increase in daily extremes is set to "very likely" (p53,line 54) whereas there would be only "medium confidence" that warm spells increased (p. 54, line5). This does not make sense, all the more since in Ch 10, it is stated wrt attribution that (Ch10, p42, line 53-54) "it is very likely that human influence has contributed to the observed changes in temperature extremes". I fully agree with this last statement, but it clashes with the only "medium confidence" given to observed changes in heat waves/warm spells. Thus confidence level in observed increase in frequency of heat wave/warm spell should be set to high, based on the three studies mentioned above and also eg Stott et al, ASL, 2011. [Dim Coumou, Germany]	TAKEN INTO ACCOUNT: The medium confidence assigned to heatwaves is based on a number of factors not least of which is the definitions of heatwaves which vary from place to place and can give different results depending on definition. Also heatwaves are not just 'temperature extremes' as is clearly highlighted in this section. Another factor is that we have very little information for a lot of regions of the world e.g. Africa, South America so to make a global conclusion of anything higher than medium confidence would not be robust. However we agree that there are regions where we could make firmer statements and this is now included in a new Table on regional changes.
2-2098	2	53	28	53	29	Where are these regions of 'transitional climate zones'? Can you give an example? [European Union]	TAKEN INTO ACCOUNT: There are many transitional climate zones as highlighted by the references and to name all regions would make for rather cumbersome text

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2099	2	53	34	53	35	Wind speed or advected air by themselves do not induce higher evapotranspiration. Can you be more specific? Explain also how these processes amplify soil moisture-temperature feedbacks. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Text has been revised and this sentence has been removed.
2-2100	2	53	36	53	38	There is a lot more on drought feedbacks in Lau, W. K. M. and K-M. Kim (2012), The 2010 Pakistan flood and Russian heat wave: Teleconnection of hydrometeorologic extremes, J. Hydromet., 13, 392-403, doi: 10.1175/JHM-D-11-016.1. and also Trenberth, K. E. and J. T. Fasullo, 2012: Climate extremes and climate change: The Russian Heat Wave and other Climate Extremes of 2010. J. Geophys. Res., 117, D17103, doi: 10.1029/2012JD018020. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed these manuscripts for inclusion.
2-2101	2	53	37	53	38	with evidence emerging of a global signature … → unclear to me what this means (in particular, what is "global" here?) [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Text amended for clarity.
2-2102	2	53	42	53	42	Regarding Australian heat wave, please cite "Pezza, A. B., P. van Rensch and W. Cai, (2011). Severe heat waves in southern Australia: synoptic climatology and large scale connections. Climate Dynamics, 38, 209-224." [Government of Australia]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2103	2	53	48	53	48	were → are [Ileana Bladé, Spain]	EDITORIAL: This paragraph has been removed.
2-2104	2	53	52			Add context of the extreme danger of cold waves: However, there is major danger from cold wave. Mongolia experienced its fourth dzud in ten years during the winter of 2009 – 2010, killing 10 million head of livestock (22%), the worst since 1945. Finland lost a third of its population in the Great Famine of 1695-1697." References: " Reference: UNDP/NEMA (2010) "DZUD National Report 2009-2010." "Neumann, J.; Lindgrén, S. (1979). "Great Historical Events That Were Significantly Affected by the Weather: 4, The Great Famines in Finland and Estonia". Bulletin of the American Meteorological Society 60 (7): pp775–787. doi:10.1175/1520-0477(1979)060<0775:GHETWS>2.0.CO;2. ISSN 1520-0477." [David L. Hagen, United States of America]	REJECTED: Discussion of impacts is outside the scope of IPCC Working Group I.
2-2105	2	53	54	53	54	analysis → new analysis [Ileana Bladé, Spain]	NOTED
2-2106	2	53	58	53	58	"medium confidence", it is perhaps better to say "medium confidence due to lack of data". [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: The assessment has been revised based on updated literature.
2-2107	2	53	58			Same comment as the one immediately above. [Adrian Simmons, United Kingdom]	NOTED
2-2108	2	54	6	54	7	I cannot find supporting evidence for this high confidence statement about heat waves in Europe. In fact, the only reference I find is in Page 53, lines 45-46, and it hardly agrees with the statement: "In Europe there is some suggestion that positive trends calculated in earlier studies may have been underestimated". [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: We have significantly revised this section and added a table on regional changes. The literature does support this assessmnet and we have amended the text accordingly.
2-2109	2	54	6	54	7	Which large parts of Europe have high confidence in increasing numbers or length of warm spells? [European Union]	TAKEN INTO ACCOUNT: We have significantly revised this section and added a table on regional changes. The literature does support this assessmnet and we have amended the text accordingly.
2-2110	2	54	7	54	8	Issue: The minimum temperature does not always occur at night, and the maximum temperature does not always occur during the day. In particular, over the polar regions and for the periods without solar radiation, "day" and "night" are not well defined. Therefore, the terms of "cold night, warm night, cold day, warm day" are not very appropriate. At least a note should be given. Suggested Change: add a short paragraph on line 8 of p. 2-54: "It needs to be emphasized that the minimum temperature usually occurs near sunrise and the maximum temperature usually occurs in the early afternoon under clear sky conditions. In general, minimum temperature could occur during the day while the maximum temperature could occur at night, particularly over regions with cloud cover and over polar regions for the periods without solar radiation (e.g., Wang and Zeng 2012). Therefore, the terms of "cold night, warm night, cold day, warm day" are used for convenience rather than to imply that the minimum and maximum temperatures always occur at night and during the day respectively." [Xubin Zeng, United States of America]	TAKEN INTO ACCOUNT: It is now clear from Box 2.4, Table 1 how these indices are being defined.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2111	2	54	9			I suggest changing the title to "Extremes of the Hydrological Cycle" to distinguish from Section 2.5. [Richard Allan, United Kingdom]	ACCEPTED. Title amended.
2-2112	2	54	9			section 2.6.2 does not adequately discuss the issues in dealing with extremes of precipitation (as in Trenberth 2011), such as the need for hourly data to deal with intermittency. Daily data is a poor substitute. This is touched on in the last para but too little too late. [Kevin Trenberth, United States of America]	NOTED. We note the need for sub-daily data but the literature assessing this on large regional to global scales is very limited. In fact WCRP note in their Grand Challenges white papers that lack of high-resolution (space and time) observations are hampering this sort of research. We have amended this section to try and more adequately address this issue.While like the reviewer we accept the need for research into sub-daily data changes, if the research of these sorts of observations does not exist we can do little to make an assessment.
2-2113	2	54	11	54	12	Introductory sentence could be removed - seems not to be crucial as such concepts are introduced in Chapter 1. [Thomas Stocker/ WGI TSU, Switzerland]	AGREED: Text amended accordingly
2-2114	2	54	11			"The hydrological cycle describes the continuous circulation of water between Earth's atmosphere and both surface and subsurface bodies of water." This could be removed if it has already been described in Chapter 1 or if not moved back to Section 2.5. [Richard Allan, United Kingdom]	AGREED: Text amended accordingly
2-2115	2	54	17	55	26	Section 2.6.2.1 Precipitation Extremes: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	NOTED
2-2116	2	54	17			The following is condensed from (Shiu et al., 2012, GRL, 39, L17707, doi:10.1029/2012GL052762) (Apparently Figures 1 and 2 failed to copy. Please refer to Shiu et al., 2012) Major conclusions: (1) Large changes in the precipitation extremes derived from the Global Precipitation Climatology Project (GPCP) data, about 100% increase for the annual top 10% heavy precipitation and about 20% decrease for the light and moderate precipitation for one degree warming in the global temperature, are in agreement with results derived from reanalyses of the European Centre for Medium-Range Weather Forecasts (ECMWF) and the National Centers for Environmental Prediction (NCEP). (2) In comparison, coupled climate models are capable of simulating the shape of the change in precipitation intensity, but underestimate the magnitude of the change by about one order of magnitude. The most likely reason of the underestimation is that the typical spatial resolution of climate models is too coarse to resolve atmospheric convection. Introduction Significant increases of the very heavy precipitation and decreases of light and moderate precipitation have been reported using historical raingauge data over many land areas [Karl and Knight, 1998; Manton et al., 2001; Klein Tank and Können, 2003; Liu et al., 2005; Fujibe et al., 2005; Groisman et al., 2005; Goswami et al., 2006; Lenderink and Meijgaard, 2008]. Over the oceans, analyses of satellite data from 1979-2003 at low latitudes (30oS-30oN) also found similar changes [Lau and Wu, 2007]. The widespread increases of heavy precipitation have been attributed to global warming [Trenberth, 1998; Semenov and Bengtsson, 2002; Allen and Ingram, 2002; Trenberth et al., 2003]. Trenberth et al. [2003] hypothesized that the precipitation intensity should increase at about the same rate as atmospheric moisture which increases at about 7% K-1 according to the Clausius-Clapeyron equation. They argued that the increase in heavy rainfall could even exceed the 7% K-1 because additional latent heat released from the increased water vapor could invigorate the storms. However, they didn't give any quantitative estimate how much over the Clausius-Clapeyron rate or at what intensity of the storms. An invigorated storm could remove moisture by more than 7% K-1 from the atmosphere. On the other hand, global evaporation, which is determined by global surface energy budget, increases at the same rate as global-mean precipitation, about 2-3% K-1 [Cubasch et al. 2001]. This means that in a warmer climate it would take longer for evaporation to replenish atmospheric moisture, thus longer dry spells between storms.	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion. It is outside the scope of Chapter 2 to discuss climate model evaluation which is covered in Chapter 9.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Moreover, the enhanced latent heating from convection would make the atmosphere more stable and thus less likely to precipitate, especially for light and moderate precipitation that requires an unstable large-scale environment. The combined effect is to increase the intensity of heavy precipitation from large storms while suppressing light and moderate precipitation. These thermodynamic arguments are broadly confirmed by an analysis of model-simulated changes by Sun et al. [2007] and an analysis of observational data by Liu et al. [2009], who examined inter-annual variations of observed precipitation from the GPCP [Huffman et al., 2001; Xie et al., 2003]. Both studies find increases in very heavy rainfall exceeding the 7% K-1 (bin 10 of Figure 1), and decreases in moderate precipitation (bins 2 to 5 of Figure 1). In fact, the two studies agree very well in the shape but differ by about one order of magnitude. The analysis of daily data from 14 coupled global climate models (GCMs) by Sun et al. [2007] showed that, under global warming conditions driven by increasing greenhouse gases, the increase of global-mean precipitation intensity (see Supplement for definition of precipitation intensity) could be calculated by averaging the increase in precipitation intensity at each grid point. They obtained an increase of global-mean precipitation intensity of about 2.2% K-1 for the multi-model ensemble average, with a relatively small scattering of 1.4-3.3% K-1 among the models [see Table 2 of Sun et al. 2007]. This intensity increase is significantly higher for top 10% or heavier precipitation, at a rate exceeding the 7% K-1 (Figure 1), in agreement with the hypothesis of Trenberth et al. [2003]. Nevertheless, the mean intensity increase in the model-simulated long-term changes is about one order of magnitude smaller than that derived from the inter-annual variations in the GPCP pentad data [Liu et al., 2009]. Underestimates by climate models for the increase in very heavy precipitation in response to increases of sea-surface temperatures (SST) by a factor of about two over the tropical oceans were noticed by Allan and Soden [2008]. Model-simulated changes in heavy precipitation were also found to be smaller than those seen in limited daily rain gauge data over many Northern Hemisphere land areas [Min et al., 2011]. This underestimation of changes in heavy precipitation by climate models are not unexpected given that current GCMs tend to produce too much light and moderate precipitation while missing most heavy and extreme precipitation events [Dai and Trenberth. 2004; Dai, 2006; Sun et al., 2006]. This large bias results mainly from difficulties in representing small scale (~1-10 km) atmospheric convection in GCMs with a grid size of 100 km or more. Specifically, moist convection tends to start prematurely in most GCMs, leading to weak but frequent convections, an incorrect diurnal cycle, and too much convective precipitation and too little stratiform precipitation in most GCMs [Dai and Trenberth, 2004; Dai, 2006]. Although there are significant differences in data resolution and analysis methods between Sun et al. [2007] and Liu et al. [2009], the large difference in the changes for different precipitation categories between the models and GPCP data still raises a serious concern that the risk of extreme precipitation events under global warming, including floods and droughts, may be substantially greater than that estimated by climate models as of 2007. Concerns about the GPCP data Besides the well-known model deficiencies in simulating convection that could lead to models' underestimation of heavy precipitation changes under global warming conditions, there are other aspects that could contribute to the large difference between the results of Sun et al. [2007] and Liu et al. [2009]. For example, Sun et al. [2007] examined long-term changes induced by increases in greenhouse gases, while Liu et al. [2009] analyzed inter-annual variations (hereafter referred to as inter-annual method, see Supplement for the description of the method) in a relatively short period (1979-2007). Although the basic precipitation processes (e.g., cloud microphysics) are likely to be similar under the two different cases, we cannot rule out the possibility that there are physical processes that could result in different $\Delta P/\Delta T$ ratios at inter-annual and centennial time scales. For example, ENSO may have a much greater impact on the short-term variations than the long-term trends. However, some analyses of long-term linear trends in the 20th century [Fujibe et al., 2005; Lau and Wu, 2007] yield $\Delta P/\Delta T$ ratios similar to results of Liu et al. [2009]. This suggests that at least for the recent (20th century) climate, the $\Delta P/\Delta T$ ratios from inter-annual variations and long-term linear trends are similar. Another concern is that the GPCP pentad data contain biases and errors that enhance the $\Delta P/\Delta T$ ratios. For instance, GPCP data contain a large amount of satellite observations over oceans and parts of land areas, and these satellite observations may contain discontinuities as satellite sensors and orbits have changed over time. For example, the GPCP satellite-based precipitation before 1987 is likely to be less reliable because of	

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the lack of microwave observations, while satellite observations for the last 10 years or so have much improved sampling and thus are more reliable. Other concerns regarding the GPCP data are the coarse resolution (2.5o x 2.5o), 5-day average, and possible long-term drifts in the accuracy of observation instruments. To alleviate these concerns regarding to the GPCP data and analysis methodology, in the following we compare changes in precipitation intensity derived from the GPCP data with results from reanalyses using operational weather forecast models, namely NCEP/NCAR [Kalnay et al., 1996] and ECMWF ERA-40 [Uppala et al., 2005] reanalysis. The comparison is valuable because the precipitation from the reanalyses is a model-calculated quantity that depends on parameterized moist convection and large-scale precipitation processes in the models used in the reanalysis systems. These parameterizations are similar to those of the coupled climate models analyzed by Sun et al. [2007]. However, the atmospheric states (pressure, temperature, humidity, and winds, but not precipitation) were constrained by available observations assimilated in the reanalyses. This is not the case in the coupled climate models. Therefore, the comparison constitutes an independent evaluation of the change in precipitation intensity derived from GPCP. It also provides an estimate of the ΔP/ΔT ratio simulated by model physics similar to that used in the climate models, but under more realistic environmental conditions. Finally, we analyze the short-term and long-term results of four AR4 climate models to evaluate the difference between short-term variations and long-term trends. Data To compare with the ΔP/ΔT ratios derived from GPCP (60oS – 60oN, 1979-2007) by Liu et al. [2009], the daily data of NCEP/NCAR (60oS – 60oN, on T62 Gaussian grid, 1979-2009; Kalnay et al., 1996) and ECMWF ERA-40 (60oS – 60oN, on 2.5ox2.5o grid, 1979-2001; Uppala et al., 2005) reanalysis are analyzed using the inter-annual method, which is described in the Supplement. The domain 60oS – 60oN is chosen because there are many missing data at higher latitudes in the GPCP data. More recent reanalyses, namely CFSR (1979-2009), ERA-Interim (1989-2010), and MERRA (1979-2010) have also been analyzed. Their results are similar to those of NCEP/NCAR and ECMWF ERA-40, but having greater one-standard deviations and hence not shown here. Results and discussion Figure 1 depicts the change in the precipitation amount for each precipitation intensity bin for one degree Kelvin increase in near global-mean temperature (ΔP/ΔT) derived using the inter-annual method from the GPCP pentad data (1979-2007) (yellow bars), and from reanalysis daily precipitation from NCEP/NCAR (1979-2009) (green) and ECMWF ERA-40 (1979-2001) (red). The model-simulated ΔP/ΔT values inferred from long-term changes from Sun et al. [2007] is shown in brown bars (on the right-hand coordinate, the others on the left-hand coordinate). It can be seen that the shapes of the ΔP/ΔT ratio as a function of the precipitation intensity categories are very similar among the four cases, with small changes except the top two or three bins of heavy precipitation. The absolute magnitude of the ΔP/ΔT ratio is comparable among the GPCP, NCEP/NCAR, and ERA-40 cases; whereas the ΔP/ΔT values from climate models are smaller by about a factor of 10. For the GPCP and reanalysis data, the top 2 bins (i.e., the top 20% heavy precipitation) show large increases of about 20-100% K-1, whereas the lower 6 bins (i.e., the bottom 60% moderate and light precipitation) show either little change or relatively small decreases up to 30% K-1. The overall effect of the changes in individual bins constitutes a shift to higher precipitation intensity, i.e. an increase in the overall precipitation intensity. This overall shift to higher precipitation intensity is best measured by changes of bins 10 and 9 which usually have large values and small 1-standard deviation ranges. Using this measure, it can be seen that the two reanalyses are within 50% of GPCP results, i.e. within the uncertainty of the GPCP data. Therefore one can conclude that the GPCP and the two reanalyses are in good agreement, especially considering that they are from independent data sets. On the other hand, the model results are about a factor of 8 lower than GPCP values, substantially above the uncertainty of the GPCP data. To ascertain that the large difference between the model-simulated values derived by Sun et al. [2007] and the other three cases is not due to different analysis methods, we have downloaded output data from the AR4 NCAR CCSM3 model simulation for the period 2000-2099, and analyzed the data with the inter-annual method for the global domain of 90S – 90N. The results are compared with the long-term linear trends induced by increases in greenhouse gases similar to those deduced by Sun et al. [2007]. In addition since this model simulation covers long-term climate change from 2000 to 2099, it offers an opportunity to examine the ΔP/ΔT	

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						ratios over decadal, multi-decadal and centennial time scales for the 21st century. Plotted in Figure 2 are ΔP/ΔT ratios for periods of short-term (ΔT<20 years), the long-term trends between 2000-2029 and 2070-2099 (i.e. ΔT>=70 years), ΔT<100 years, and the ratio for the linear trends of P and T between 2000 and 2099. The last ΔP/ΔT ratio is similar to the calculation of Sun et al. [2007] except they calculated the change in precipitation intensity for each grid first and then made the global averaging. It is clear that within ±1-standard deviation ranges, the four cases agree with one another very well. The agreement shows that the ΔP/ΔT ratios calculated by the inter-annual variation method, either in short term (<20 years) or long term (>70 years), are practically identical to the ΔP/ΔT ratio calculated from long-term (100 years) linear trends of precipitation and temperature. Furthermore, the average increase in precipitation intensity for all four cases is about 3% K-1, agreeing very well with the value of 2.5% K-1 for the CCSM3 (CRES B1 scenario) derived by Sun et al. [2007]. This shows beyond any doubt that the large difference between the model-simulated values derived by Sun et al. [2007] and the other three cases of Figure 1 is not caused by any difference in the data analysis methods or difference between short-term variations and long-term trends. We have also analyzed daily data from three other AR4 model simulations, namely MPI_ECHAM (2001-2100), GFDL_CCM2.1 (1961~2000), and NCAR_CCSM3 (1960~1999), the results are similar (not shown). The use of pentad and daily data produces similar ΔP/ΔT ratios. This is confirmed by similar results found in more recent GPCP data between 1997 and 2007 when daily data are available. These results are also in excellent agreement with the pentad data for the entire period of 1979 to 2007, suggesting improved satellite data since 1987 are consistent with earlier data. In addition, the hourly data from Taiwan show consistent and similar large changes of precipitation intensity when analyzed by hourly, daily, and pentad resolutions [Liu et al., 2009]. There is also other evidence suggesting that large ΔP/ΔT values similar to GPCP of Figure 1 exist over different data records. For example, inter-annual variations in station data over land from USA during 1951-2000, and southeastern China from 1961-2005 also yield changes consistent to those of GPCP [Liu et al., 2009, Supplement]. More importantly, changes in precipitation intensity of the long term rain-gauge data over Japan from 1898-2003 analyzed by [Fujibe et al., 2005], who also categorized the precipitation intensity into 10 bins, are in excellent agreement with those of GPCP. Conclusions Based on the discussions above, we can make the following conclusions. First, the large increase in global average precipitation intensity increase derived from the GPCP data, with the top 10% heavy precipitation increased by about 108% for each degree Kelvin increase in global mean temperature and the bottom 30%-60% bins decreased by about 20% K-1, is credible. Increases in heavy precipitation can lead to more and worse floods and mudslides, while decreases of light and moderate precipitation can increase the risk of droughts. Operational models of NCEP and ECMWF used in the reanalysis mode appear to be capable of simulating realistically the change of precipitation intensity with global temperature seen in the GPCP pentad data, apparently because they have the benefit of assimilating the observed moisture and wind fields around convective storms. In comparison, the climate models are capable of simulating the shape of the change in precipitation intensity, but underestimate the magnitude of the change by about one order of magnitude. The reason of the underestimate is shown to be due to the fact that the typical spatial resolution of climate models is too coarse to resolve convective precipitation. References Allan, R. P., and B. J. Soden (2008), Atmospheric warming and the amplification of precipitation extremes, Science, 321, 1481–1484, doi:10.1126/ science.1160787. Allen, M. R., and W. J. Ingram (2002), Constraints on the future changes in climate and the hydrological cycle, Nature, 419, 224–232. Cubasch, U., and Coauthors (2001), Projections of future climate change. Climate Change 2001: The Scientific Basis, J. T. Houghton et al., Eds., Cambridge University Press, 525–582. Dai, A. (2006), Precipitation characteristics in eighteen coupled climate models. J. Clim., 19, 4605–4630. Dai, A., and K. E. Trenberth (2004), The diurnal cycle and its depiction in the Community Climate System Model. J. Clim., 17, 930–951. 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.	

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						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. Huffman, G.J., R.F. Adler, M. Morrissey, D.T. Bolvin, S. Curtis, R. Joyce, B McGavock, J. Susskind (2001) Global Precipitation at One-Degree Daily Resolution from Multi-Satellite Observations. J. Hydrometeor., 2, 36-50.Kalnay, E., and Coauthors (1996), The NCEP/NCAR 40-year reanalysis project. Bull. Amer. Meteorol. Soc., 77, 437–471. 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, S. C., C. Fu, C.-J. Shiu, J.-P. Chen, and F. Wu (2009), Temperature dependence of global precipitation extremes, Geophys. Res. Lett., 36, L17702. 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, 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. Portmann (2006), How often does it rain? J. Climate, 19, 916–934. Sun, Y., S. Solomon, A. Dai, and R. W. Portmann (2007), How often will it rain? J. Clim., 20, 4801–4818. Trenberth, K. E. (1998), Atmospheric moisture residence times and cycling: Implications for rainfall rates with climate change, Clim. Change, 39, 667–694. 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. Uppala, S. M., et al. (2005), The ERA-40 re-analysis, Q. J. R. Meteorol. Soc., 131, 2961–3012. Kalnay, E., et al. (1999), The NCEP/NCAR 40-year reanalysis project, Bull. Amer. Meteorol. Soc., 77, 437–471. Xie, P., J.E. Janowiak, P.A. Arkin, R.F. Adler, A. Gruber, R.R. Ferraro, G.J. Huffman, S. Curtis (2003), GPCP Pentad Precipitation Analyses: An Experimental Dataset Based on Gauge Observations and Satellite Estimates. J. Clim., 16, 2197–2214. Figure 1 Changes in the precipitation amount falling within each of the ten intensity bins (ΔP, expressed in % of the average precipitation amount of each bin over the time period of data) for one degree increase in global temperature (ΔT). Yellow bars denote the ΔP/ΔT values derived from the GPCP pentad data in 1979-2007. Brown bars are the ΔP/ΔT values derived from an ensemble of 14 climate models as reported by Sun et al. [2007]. Green and red bars are those from NCEP/NCAR R1 and ECMWF ERA40 reanalyses, respectively. The vertical line on top of each bar denotes the ±1-standard deviation range. Figure 2 Same as Figure 1 except for three cases calculated by the inter-annual method with different time intervals and a case calculated from 100 year linear trends of P and T from IPCC-AR4 NCAR CCSM3/SRESB1/day_run2 (2000-2099) (90S-90N). ΔP/ΔT ratio for the 20-year case (yellow) is from 20 consecutive years in 2000-2099, the 70+ years case (green) from the difference between 2000-2029 and 2070-2099 with year difference greater than 70 years, the 100 year case (red) from the straight forward inter-	

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						annual method for the entire 100 years, and the linear trend case is the ratio from the linear trends of P and T between 2000 and 2099 (black). Numbers shown in units of %K-1 are the average increases in precipitation intensity. [Shaw Liu, China]	
2-2117	2	54	19	54	19	disproportionately does not sound very scientific [Ileana Bladé, Spain]	NOTED - this was the term used in AR4 and is repeated here as we are referring to a conclusion from AR4.
2-2118	2	54	19	54	19	Not clear what annual heavy precipitation events means here. Does it mean once a year events? [John Caesar, United Kingdom of Great Britain & Northern Ireland]	NOTED - this was the term used in AR4 and is repeated here as we are referring to a conclusion from AR4.
2-2119	2	54	19	54	24	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: This comment was addressed in the previous draft. 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.
2-2120	2	54	19	55	26	Please consider including the result of the following study which shows that the return period of extreme hourly/2-hourly/3-hourly precipitation in Hong Kong has shortened significantly in the past century: - Wong, M. C , H. Y. Mok, and T. C. Lee, 2010: Observed changes in extreme weather indices in Hong Kong. Int. J. Climatol.. Published online in October 2010, doi:10.1002/joc.2238, 12 pp. [Sai Ming Lee, Hong Kong, China]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2121	2	54	19	55	26	Section 2.6.2.1 This section mentions sub-daily precip scaling with clausius-clapeyron at the end. It would be good to give this a little more context as it hasn't really appeared in Chapter 2 up until now. A few studies have noted that mean precip (tropical oceans - Allan and Soden 2008) has been increasing slower than C-C while heavy precip has been increasing at the rate of C-C. This link between increasing water vapour and increasingly heavy heavy rainfall events seems like something worth drawing attention to in the section on precipitation extremes. [Kate Willett, United Kingdom]	TAKEN INTO ACCOUNT - Due to space constraints the assessment of C-C scaling is discussed in more detail in Chapter 7. However we have amended this section and have made reference to the appropriate section of Ch 7.
2-2122	2	54	21	54	21	Is "rare" the appropriate description for a once a year event? On page 55, line 12, "rarer" is used to describe events which occur once in several decades. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: We think so - this refers to conclusions in AR4.
2-2123	2	54	22	54	22	Shouldn't the 'likely' on this line be italicised? [Government of United Kingdom of Great Britain & Northern Ireland]	NOTED
2-2124	2	54	23	54	23	although noting → but noted [Ileana Bladé, Spain]	EDITORIAL
2-2125	2	54	27	54	27	"indicate that changes". [J. Graham Cogley, Canada]	EDITORIAL
2-2126	2	54	27	54	30	Please explain what kind of changes in precipitation extremes are consistent with a wetter climate. Are these	TAKEN INTO ACCOUNT

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						changes supposed to be spatially uniform as suggested by the following sentence (lack of spatial coherence) ? This sentence seems like to over-simplify the problem. [Ileana Bladé, Spain]	
2-2127	2	54	30	54	32	What does an increasing trend in the wettest day of the year indicate? Why is this important? [Ileana Bladé, Spain]	Noted. This section has been rewritten.
2-2128	2	54	35	54	35	Delete "although", and for clarity add "than decreases" after "increases". [J. Graham Cogley, Canada]	EDITORIAL
2-2129	2	54	39	54	40	Is this very localized exception worth mentioning? Consider reducing to “with some exceptions”, or even better, insert “generally” before “indicate” in line 36. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT. This section has been shortened and a regional table with extremes trends has been added.
2-2130	2	54	40	54	45	There is a geographically more comprehensive study of extreme precipitation trends in South America that 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. Although the two references mentioned in this and in the previous comment are pre-2007, there are other references in the same sections from 2006. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2131	2	54	41	54	41	"evidence". [J. Graham Cogley, Canada]	EDITORIAL
2-2132	2	54	41	54	41	evidence (not eveidence, typo). [Christian-D. Schoenwiese, Germany]	EDITORIAL
2-2133	2	54	47	54	47	Change 'be' to 'been' [Peter Burt, United Kingdom]	EDITORIAL
2-2134	2	54	47	54	47	"have been found". [J. Graham Cogley, Canada]	EDITORIAL
2-2135	2	54	47	54	47	be -> been [Mihai Dima, Romania]	EDITORIAL
2-2136	2	54	47	54	47	Word “be” should be changed to “been”. [Andrew King, Australia]	EDITORIAL
2-2137	2	54	47	54	51	Dubuisson and Moisselin, 2006 have also observed a rise in winter precipitations extremes over the 2/3 north of France during the period 1951-2000. Conversely, a decrease in winter extremes have been noticed over the 1/3 south of France (including Mediterranean area). The reference is the following: DUBUISSON, B., MOISSELIN, J.-M., Evolution des extrêmes climatiques en France à partir des séries observées, La Houille Blanche, N°6, 42-47, 2006. [Ianis Delpla, France]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2138	2	54	49	54	49	Add reference Tromel, S., Schonwiese, C.-D., 2007: Probability change of extreme precipitation observed from 1901 to 2000 in Germany. In this paper a time-tependent PDF analysis (132 monthly time series) shows that extreme percipitation exceeding the 95th percentile has systematically increased, especially in winter. [Christian-D. Schoenwiese, Germany]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2139	2	54	51			This increasing trend of extreme winter precipitation can be explained by a decreasing snow/rain ratio due to warmer winter temperatures (Marty and Balnchet, 2011; DOI 10.1007/s10584-011-0159-9). [Christoph Marty, Switzerland]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2140	2	54	53	54	53	Delete "overall". [J. Graham Cogley, Canada]	EDITORIAL
2-2141	2	54	53	54	54	Are uncertainties larger in southern Europe and the Mediterranean because of greater variability there, poorer observational coverage or for any other reason? [European Union]	TAKEN INTO ACCOUNT: This section has been shortened and a regional table with an assessment of extremes trends has been added.
2-2142	2	54	53	54	54	This statement leaves the reader wondering what the confidence is for the remainder of Europe? It is only provided for Southern Europe and the Mediterranean. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: This section has been shortened and a regional table with an assessment of extremes trends has been added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2143	2	54	54	27	27	presumably the 'change' refers to an increase in extreme precipitation - why not say so. [European Union]	TAKEN INTO ACCOUNT: Not necessarily as both increases and decreases would be expected in a warming climate.
2-2144	2	54	56	55	2	Sentence needs to be re-written. Suggested sentence: ”Zhai et al. (2005) and Wang and Zhou (2005) found significant increases over the second half of the 20th Century in some areas and significant decreases in others whilst some studies find no systematic changes over the region as a whole (Caesar et al., 2011; Choi et al., 2009).” [Andrew King, Australia]	TAKEN INTO ACCOUNT: This section has been shortened and a regional table with an assessment of extremes trends has been added.
2-2145	2	54				Section 2.6.2.1 - I would suggest including "Min et al. (2011) [Nature] show widespread increases in precipitation extremes over the northern Hemisphere, while satellite data over the tropical ocean indicates an increase in the frequency of the heaviest rainfall during warmer years (Allan and Soden, 2008) [Science]" [Richard Allan, United Kingdom]	TAKEN INTO ACCOUNT: The Min et al. 2011 paper uses the HadEX dataset to draw its conclusions and we already make reference to this and more up to date datasets. The Allan and Soden, 2008 paper has been assessed by the authors for inclusion.
2-2146	2	55	1	55	1	"in others, while". "as a whole". [J. Graham Cogley, Canada]	EDITORIAL
2-2147	2	55	1	55	1	an a whole -> as a whole [Mihai Dima, Romania]	EDITORIAL
2-2148	2	55	1	55	9	There is a real need for an assessment here to say why "other studies find no systematic changes": why: is it data, definitions, etc? [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: This section has been shortened and a regional table with an assessment of extremes trends has been added.
2-2149	2	55	4	55	6	Could be worth a second look here. I seem to recall only parts of southern Australia have shown a decrease in extremes (e.g. south-western Australia) while others parts have shown increases. [Government of Australia]	TAKEN INTO ACCOUNT: This section has been shortened and a regional table with an assessment of extremes trends has been added. Studies indicate likely decreases across most of southern Australia but this is seasonal and index dependent as highlighted in the table.
2-2150	2	55	12	55	16	Is a 58-year time series enough to assess changes in the frequency of rare events that occur “once in several decades” ? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: There are now many sophisticated statistical techniques that can assess the changes in rare events with a reasonably high degree of confidence.
2-2151	2	55	15	55	15	The van den Besselaar et al. paper is now available online and has a DOI (10.1002/joc.3619). [Andrew King, Australia]	ACCEPTED: Reference amended.
2-2152	2	55	15	55	16	Citation style wrong [Peter Burt, United Kingdom]	EDITORIAL
2-2153	2	55	17	55	17	Delete "overall". [J. Graham Cogley, Canada]	EDITORIAL
2-2154	2	55	17	55	17	The subregion mentioned here should be precised, as there is a marked difference between northern and southerne areas in Europe for the variation of climate extremes [Ianis Delpla, France]	TAKEN INTO ACCOUNT: We already highlight that there is regional and seasonal dependence.
2-2155	2	55	18	55	18	increase in moderate extremes → increase in the frequency of moderate extremes [Ileana Bladé, Spain]	EDITORIAL: In fact the increase referred to here does not relate soley to frequency.
2-2156	2	55	21	55	26	Westra and Sisson (2011) , doi:10.1016/j.jhydrol.2011.06.014 address sub-daily etxremes in Australia. [Geert Jan van Oldenborgh, Netherlands]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2157	2	55	22	55	22	although we accept → odd [Ileana Bladé, Spain]	NOTED: This section has been amended.
2-2158	2	55	23	55	26	More complex than what? [Ileana Bladé, Spain]	NOTED: This section has been amended.
2-2159	2	55	23	55	26	Again, a consistency with the CC is invoked but never fully explained (see earlier comment) [Ileana Bladé, Spain]	NOTED: This section has been amended.
2-2160	2	55	25	55	25	Delete ' around Clausius-Clapeyron (for consistency elsewhere) [Peter Burt, United Kingdom]	EDITORIAL

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2161	2	55	25	55	25	Something wrong with the reference citation ('G') here and in the reference list. [Peter Burt, United Kingdom]	ACCEPTED: Reference amended.
2-2162	2	55	25	55	25	Please remove the reference "G. et al., 2011". This one in fact refers to the same reference "Lenderink et al., 2011". [Sai Ming Lee, Hong Kong, China]	ACCEPTED: Reference amended.
2-2163	2	55	25	55	25	"Jones" should be "Hardwick Jones". G. et al 2011 is the same as Lenderink et al. 2011. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference amended.
2-2164	2	55	25	55	25	G. et al seems a garbled version of Lenderink et al 2011. [Geert Jan van Oldenborgh, Netherlands]	ACCEPTED: Reference amended.
2-2165	2	55	25	55	25	Lenderink et al, Westra et al & Utsumi et al (some stations) find a scaling at approximately *twice* Clausius-Clapeyron when considering the intensity of hourly precipitation on wet days. This is not correctly reflected by this sentence. [Geert Jan van Oldenborgh, Netherlands]	NOTED: This section has been amended.
2-2166	2	55	25	55	25	"the C-C relation". Expand or remove, otherwise, it is hard to understand. [Xuebin Zhang, Canada]	NOTED
2-2167	2	55	26	55	26	I would have liked to see an overall summary to section 2.6.2.1. [European Union]	NOTED
2-2168	2	55	28	55	45	I am missing the paper Di Baldassarre, G., A. Montanari, H. F. Lins, D. Koutsoyiannis, L. Brandimarte, and G. Blöschl, Flood fatalities in Africa: from diagnosis to mitigation, Geophysical Research Letters, 37, L22402, doi:10.1029/2010GL045467, 2010 in this section. They concluded there is no climatological trend in floods in Africa. Koutsoyiannis presented also global trends at the EGU: Long-term properties of annual maximum daily river discharge worldwide, http://itia.ntua.gr/en/docinfo/1128/, conclusion: "Analysis of trends and of aggregated time series on climatic (30-year) scale does not indicate consistent trends worldwide. Despite common perception, in general, the detected trends are more negative (less intense floods in most recent years) than positive. Similarly, Svensson et al. (2005) and Di Baldassarre et al. (2010) did not find systematical change neither in flood increasing or decreasing numbers nor change in flood magnitudes in their analysis." I would say there is medium to high evidence that at least there is not a positive trend in river run-off and floods. [Marcel Crok, The Netherlands]	REJECTED: The assessments made in this section are based on many more studies than the few picked by the reviewer and based on only a few studies we would not make high confidence statements. In any case floods are seen as an impact of changes in many different variables and as such there are many confounding variables which make assessments of change extremely challenging. In addition changes in flooding can be seen as an 'impact' and therefore this belongs outside the scope of WG1 and belongs withing WG2.
2-2169	2	55	28	57	21	Section 2.6.2.2 is on Floods, drought and extreme weather events, but I see few discussions on floods. The streaflow trends discussed in Dai et al. (2009, J. Climate, pp.2773-2791; also see Fig. Fig.5 of Dai 2011, WIRES) are relevant for both flood changes and drought trends. Although there are differences in the PDSI data due to differences in the forcing data and the way of calibaration/normalization, the precipitation and streamflow trend maps shown in Fig. 5 of Dai (2011, WIRES) provide collaborative support of widespread drying since 1950. Precipitation and runoff trends are other important measures of drought and these are direct observations, in contrast to PDSI or land model-computed soil moisture. There are two reasons for the weaker drying trends since 1950 found by Sheffield et al. and van der Schrier et al.: 1) they all used the CRU precipitation data set that contained much fewer (<1500) raingauges than other products (GPCP and GPCC, >10,000) for the period since 1995 and the CRU P data set shows much wetter conditions than all other precipitation products (see Figs. S12-13 of Sheffield et al. 2012, Nature); and 2) they all included the 1980-present period in the PDSI calibration period, which is not right. The PDSI model calibrates the PDSI values to a pre-defined range (approx. -6 to +6) using the variance of the calibration period. Including the recent warming period in the calibration period increases the variance and reduce the PDSI value range including its long-term changes and the signal to noise (varinace) ratio, making the trend less likely to be significant. This is like to use the 1950-2008 as the reference period for global-mean temperature AND normalize the temperature anomalies (dT) by the standard devidation (s.d.) of dT over the period from1950-2008, and then use the normalized anomaies (dT/s.d.) to claim that the recent warming is insignificant. This is obviously wrong as the s.d. already accounts for much of the warming and the normalization by it removes a large part of warming signal while the s.d. already includes the warming trend (not real natural variability). In contast, Dai et al. (2004) and Dai (2011, JGR) used the standard 30-yr climatology from 1950-1979 as the calibration period for reference and estmated the recent warming effect in comparison to this baseline period. Using the whole analysis period as the reference period is incorrect for assessing long-term changes, especially when the data are normalized by the variance over the whole data period. As shown by Dai (2011, WIRES), the drying patterns from his PDSI data are broadly consistent with independent records of streamflow and also with precipitation data. This section should not ignore these facts and discuss the PDSI differences only. [Aiguo Dai, United States of America]	TAKEN INTO ACCOUNT: Flooding is covered in much more detail by WGII and streamflow is covered in Section 2.5.2. We have substantially revised the text on droughts to take account of the more recent literature that has been published since the SOD. We now assess differences between the different studies commenting on the different forcing precipitation datasets and calibration periods. Based on the current literature we highlight that there are compelling arguments both for and against significant increases in the percentage of area in drought since the middle of the 20th Century. However we also now highlight regions where irrespective of the precipitation dataset or period of calibration, there is high agreement in the literature either for increases or decreases in drought.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2170	2	55	28	57	21	Section 2.6.2.2 Meteorological drought is usually defined in terms of rainfall anomalies or deficiencies. The PDSI (e.g., Dai et al. 2004) attempts to capture both rainfall and evaporative demand in an index, so that the comparison with meteorological drought isn't really appropriate and it would be expect that difference will be seen with sustained warming trends. [Government of Australia]	TAKEN INTO ACCOUNT: This section has been revised accordingly.
2-2171	2	55	28	57	21	Section 2.6.2.2 Floods, Droughts and Severe Local Weather Events: Text was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected with considered text. [Dirk Thielen, Venezuela]	NOTED
2-2172	2	55	28			Section 2.6.2.2: In a comment on the chapter as a whole I have drawn attention to the lack of emphasis on evidence for intensification of the hydrological cycle, and overemphasis on changes in global average precipitation as opposed to regional averages. Similar remarks apply to the incidence of floods. For example at P55 L43 the assessment is that there is "no clear and widespread evidence"; but if what is happening is that regional changes are cancelling out then the assessment might be missing an important change. The widespread evidence might indeed be "clear" as pointing to more floods in wet regions and fewer in dry regions. [J. Graham Cogley, Canada]	TAKEN INTO ACCOUNT: A regional table with an assessment of some regional trends in precipitation extremes is now included.
2-2173	2	55	28			This section deals with floods and droughts. Floods, in particular, have a component related to heavy rains but also depend hugely on infrastructure and the environment: whether drains, culverts, reservoirs, dams etc have been built. The section is quite inadequate in this regard. Please see also major comments on the need to deal with extreme events of note, and also the effects of ENSO and other natural variability vs changes due to extra moisture loading of the atmosphere etc. The comments lines 32-38p 56 fail to take account of changes in the mean precipitation associated with La Nina etc. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: This section has been rewritten to deal with some of the issues raised by the reviewer. However we disagree that there should be a focus on individual events as this would require some cherry picking of events.
2-2174	2	55	30	55	45	It might be worth clarifying the type of flood dealt with here (meteorological/fluvial/alluvial etc.?) just as is done for types of drought (meteorological/hydrological/agricultural) below. [European Union]	TAKEN INTO ACCOUNT: This section has been rewritten and much of the assessments of floods will appear in WG2.
2-2175	2	55	32	55	32	regarding the magnitude or frequency of floods → regarding changes in the magnitude or frequency of floods [Ileana Bladé, Spain]	EDITORIAL
2-2176	2	55	32	55	32	AR4 reported → As for droughts, AR4 reported that [Ileana Bladé, Spain]	EDITORIAL
2-2177	2	55	34	55	34	Please provide references for 'other analyses' [Peter Burt, United Kingdom]	TAKEN INTO ACCOUNT: This section has been rewritten
2-2178	2	55	38	45	45	reference Stahl et al 2010 conclusions about increasing/decreasing low flows across Europe [European Union]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2179	2	55	38	55	38	You do not explicitly indicate the sign of these flood trends. I assume they are positive but it should be stated. [Ileana Bladé, Spain]	NOTED
2-2180	2	55	39	55	39	in some regions → in some northern regions [Ileana Bladé, Spain]	NOTED
2-2181	2	55	41	55	41	Include: Candela L, Tamoh K , Olivares G, Gomez M (2012). Modelling impacts of climate change on water resources in ungauged and data-scarce watersheds. Application to the Siurana catchment (NE Spain). Science of Total Environment. 440:253-260 [LUCILA CANDELA, Spain]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2182	2	55	41	55	45	This is a very awkward sentence; please reword. Also, do you mean *early* spring flow? [Ileana Bladé, Spain]	EDITORIAL
2-2183	2	55	47	56	6	2.6.2.2 re droughts, Add: "The CSIRO climate models predicted increasing drought in Australia. However, by hindcasting/forecasting, Stockwell found decreasing drought instead, with simple mean values showing more skill. Strict validation is needed for drought models." Reference: David R. B. Stockwell (2010) Critique of Drought Models in the Australian Drought Exceptional Circumstances Report (DECR). Energy & Environment, 21:5, 425-436 [David L. Hagen, United States of America]	REJECT: This is outside of the scope of the observations chapter.
2-2184	2	55	48			Section 2.6.2.2: The text is diffuse and perhaps repetitive. The paragraph that describes distinctions among meteorological, agricultural, and hydrological drought doesn't seem to be followed in the subsequent text. For	TAKEN INTO ACCOUNT: Text amended accordingly. We have removed this section and now refer to the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						example, one subsequent paragraph uses the terms "drought proxies" and "hydrological drought proxies," seemingly as mutually exclusive and collectively exhaustive categories. Other passages refer vaguely to "drought," without explaining which type of measure is being used. [Government of United States of America]	glossary definition for drought.
2-2185	2	55	53	55	53	Explain briefly how drought type and complexities of definition "can substantially affect the conclusions". [J. Graham Cogley, Canada]	TAKEN INTO ACCOUNT: Due to space constrainst we rather refer the reviewer to the appropriate section of the SREX report.
2-2186	2	55	56	55	56	Define 'PDSI' [Government of Australia]	REJECTED: This is defined earlier.
2-2187	2	55	56	55	57	For increased readability, begin sentence with “While” and change stop mark in line 57 to a comma. [Ileana Bladé, Spain]	EDITORIAL
2-2188	2	55	56	56	30	Sheffield, J., Wood, M.F. and Roderick, E.L (2012) Little Change in global drought over the past 60 years (Nature 491, 435–438, doi:10.1038/nature11575) present the latest case for a lack in trend in global drought when a more realistic water-balance model is used to assess it. [David Parker, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2189	2	55	56	56	56	There have been a flurry of papers on drought not included in this section. The new paper recently published in Nature by Sheffield et al “Little change in global drought over the past 60 years” [Nature 491 15 Nov 2012 435-440] has done some impressive work. But it should not have been published in Nature. It has 38 pages of dense supplementary material, for heaven’s sake. Was that reviewed? Probably not. The conclusions of the paper are likely wrong. The paper re-examines the Palmer Drought Severity Index (PDSI) in different formulations and how it changes over time. However, this has been done before in references embedded in the paper: 24. Dai, A. Characteristics and trends in various forms of the Palmer Drought Severity Index during 1900–2008. J. Geophys. Res. 116, D12115 (2011). 25. van der Schrier, G., Jones, P. D. & Briffa, K. R. The sensitivity of the PDSI to the Thornthwaite and Penman–Monteith parameterizations for potential evapotranspiration. J. Geophys. Res. 116, D03106 (2011). And dismissed with “Recent studies have claimed that there is little difference between the PDSIs that use the Thornthwaite and PM algorithms (PDSI_Th and PDSI_PM, respectively)24,25 but this can be attributed to inconsistencies in the forcing data sets and simulation configuration(see Supplementary Information).” Yes, but at least some of the supplementary material in this respect is wrong. Ironically, just as Nature is publishing their article, Nature Climate Change is publishing a completely different view: Dai, A., 2012 Increasing drought under global warming in observations and models Nature Climate Change doi:10.1038/nclimate1633 (online only so far). The simple PDSI_th relies on temperature to crudely estimate the atmospheric demand for moisture from the soil and thus evapotranspiration. It can be calibrated to each area and it works reasonably well but it is not perfect. It is known that actual evaporation depends on solar radiation and other energy sources, as well as surface wind speeds and humidity in addition to temperature. The problem is these fields are mostly not available in adequate quality, moreover daily fields are required. The Sheffield et al Figs. S12-S14 suggest that the “little drying” conclusion is likely due to the use of the CRU precipitation data, which has fewer than 1500 raingauges for the recent years and which differs substantially from the GPCC and GPCP precipitation products that have many more gauge data for the last 10-20 years. The authors make a big deal of their findings, but in fact van der Schrier et al. have made similar conclusions in their previously published papers using the same CRU data set as the forcing. There are also major concerns about the reconstruction of the solar radiation data, which depends a lot on how clouds have changed. Accordingly the discrepancies arise from the highly uncertain “forcing” data. In that sense the PDSI_th is much simpler but requires careful interpretation.	TAKEN INTO ACCOUNT: The authors have assessed these manuscripts for inclusion and note the reviewers concerns.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Another important aspect, not explored in the literature but which has emerged in subsequent exchanges, is the baseline period to define and calibrate the PDSI categories. Sheffield et al use a base period of 1950-2008. Dai uses 1950-79. The ideal base period should sample natural variability fully, and the 1950-79 period does not include the dust bowl era of the 1930s, for instance (but then none do). However, there is a major problem in using 1950-2008, because then all the climate change aspects are included. This changes the ranges for defining drought and means that the climate change is included in the spreads, greatly reducing the prospects of finding a climate change signal. [Kevin Trenberth, United States of America]	
2-2190	2	55	57	56	1	Citation style wrong [Peter Burt, United Kingdom]	EDITORIAL
2-2191	2	56	3			A notable case of a region that has experienced more drought is eastern Africa. Climate diagnostic research links this with rising sea surface temperatures in the Indian and western Pacific Oceans, which are in lockstep with rising global temperatures (strongly implying anthropogenic causes). Given the dire human cost of the 2011 famine in the eastern Horn of Africa, and the major role of drought in the crisis, this regional phenomenon deserves attention in the IPCC 5th Assessment Report. References below: Williams, A.P., and C. Funk. 2011. A westward extension of the warm pool leads to a westward extension of the Walker circulation, drying eastern Africa. Climate Dynamics. Funk, C. , 2012. EXCEPTIONAL WARMING IN THE WESTERN PACIFIC–INDIAN OCEAN WARM POOL HAS CONTRIBUTED TO MORE FREQUENT DROUGHTS IN EASTERN AFRICA, in Peterson, T.C., P.A. Stott, and S. Herring, 2012. EXPLAINING EXTREME EVENTS OF 2011 FROM A CLIMATE PERSPECTIVE. BAMS, July 2012. [Government of United States of America]	ACCEPTED: We agree that there should be more discussion of regional changes in drought and have added a new table accordingly and amended the text in this section. However any discussion on the impacts of drought is outside the scope of WGI.
2-2192	2	56	8	56	22	These two paragraph seem disconnected. The opening sentence (line 8) contains a statement about the physics of drought, but then the next sentence skips to ambiguities in drought indices. The example in lines 11-12 is confusing as it is not clear what is being compared (results from a hydrological model versus what?). The second paragraph continues to discuss the sensitivity of results to index definition and variable choice, so perhaps it should be part of the same paragraph discuss problems related to drought definition? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Text amended accordingly for clarity.
2-2193	2	56	9	56	9	Variable → phenomenon [Ileana Bladé, Spain]	EDITORIAL
2-2194	2	56	15	56	22	It will be necessary here to consider Sheffield, J., E.F. Wood and M.L. Roderick, 2012, Little change in global drought over the past 60 years, Nature, 491, 435-438. [J. Graham Cogley, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2195	2	56	15	56	22	A pervasive weakness of indirect drought measures, or proxies, is that many are based on empirical relations that might not hold under changing atmospheric composition Suggest the authors consider findings contained in Milly and Dunne, 2011 (doi:10.1175/2010EI363.1.) [Government of United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2196	2	56	15	56	22	One issue with the PDSI and scPDSI series (and also SPEI) is what PET is used. It doesn't appear to make much difference, but it would be worth mentioning that this has been considered. This was a big issue in AR4. Relevant paper is van der Schrier, G., Jones, P.D. and Briffa, K.R., 2011: The sensitivity of the PDSI to the Thornthwaite and Penman-Monteith parameterizations for potential evapotranspiration, J. Geophys. Res., 116, doi:10.1029/2010JD015001. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This section has been rewritten to take account of these issues.
2-2197	2	56	17	56	17	Delete "conditions". Change "Variable selection" to "The chosen proxy". [J. Graham Cogley, Canada]	ACCEPTED: text amended accordingly
2-2198	2	56	22	56	22	It seems odd to have a section on soil moisture (2.5.3) and then this one result on soil moisture trends here. [Ileana Bladé, Spain]	ACCEPTED: text amended accordingly
2-2199	2	56	24	56	30	The delineation of zones in the SREX report for assessment of trends in drought limited the identification of important regional changes that have occurred over the past century in eastern Canada. For instance, it is stated in the SREX for eastern Canada, Greenland and Iceland (table 3-2, CGI-2) “insufficient evidence” for changes in dryness. However, the area covered by this CGI-2 region is extremely large. Girardin and Wotton (2009) and Girardin et al. (2009) rather report with the analysis of CRU data a north-south dipole, with increasing summer drought in taiga forests and decreasing drought in boreal forests and St-Lawrence river	TAKEN INTO ACCOUNT: The section on drought has been revised and a new table has been introduced to assess regional trends. However, it is outside the scope of chapter 2 to focus on small regional changes or palaeoclimate timescales.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						areas. Declining drought severity in the boreal and St-Lawrence river areas is well supported by proxy evidence, including tree-ring records and stand-replacing fire history studies (Girardin et al. in press). For the boreal, it is a major drought in the 1910-1920s that dominate the trend signal (Girardin et al. 2009, page 2760 with relation to burn areas). In all cases the changes are significant (Girardin et al. 2009, in press). Analysis of scPDSI by van der Schrier (2011) also point to decreasing dryness in southeastern Canada. Hence, for this extremely large area (SREX CGI-2) the statements made in the SREX report may be misleading and consideration should be given to updating them in this current assessment report. Girardin, M.P. and Wotton, B.M. 2009. Summer moisture and wildfire risks across Canada. Journal of Applied Meteorology and Climatology 48: 517-533. Girardin, M.P., Ali, A.A., Carcaillet, C., Mudelsee, M., Drobyshev, I., Hély, C., Bergeron, Y. 2009. Heterogeneous response of circumboreal wildfire risk to climate change since the early 1900s. Global Change Biology 15, 2751–2769, doi: 10.1111/j.1365-2486.2009.01869.x van der Schrier, G., P. D. Jones, and K. R. Briffa (2011), The sensitivity of the PDSI to the Thornthwaite and Penman-Monteith parameterizations for potential evapotranspiration, J. Geophys. Res., 116, D03106, doi:10.1029/2010JD015001 Girardin, M.P., Ali, A.A., Carcaillet, C., Gauthier, S., Hély, C., Le Goff, H., Terrier, A., Bergeron, Y. . In press. Fire in managed forests of eastern Canada: risks and options, Forest Ecology and Management, http://dx.doi.org/10.1016/j.foreco.2012.07.005 [Government of Canada]	
2-2200	2	56	24	56	30	It is stated: “in North and Central America an overall slight decrease in dryness has been observed since 1950 (Figure 2.33b) although regional variability and the 1930s drought in the USA and Canadian Prairies dominate the signal…" These changes are not apparent on Figure 2.33b, possibly because of the poor quality. Also, these trends were tested on the period 1951-2010. How can the 1930s’ drought affect this trend? This analysis (Figure 2.33b) and paragraph requires revision. [Government of Canada]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly and a new table has been introduced to assess regional trends.
2-2201	2	56	25	56	26	‘For example, in North and Central America an overall slight decrease in dryness has been observed since 1950 (Figure 2.33b)…’. I cannot see this overall decrease in Fig. 2.33b. I can see a very large white area in North America and predominantly grey in Central America, and neither of these colours indicates decrease of dryness. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly.
2-2202	2	56	25	56	30	Sentence is grammatically incorrect. Split in two sentences. [Ileana Bladé, Spain]	EDITORIAL: In any case this section has been rewritten
2-2203	2	56	28	56	30	‘…while in Africa drought indices have generally increased (Figure 2.33b), the 1970s prolonged Sahel drought dominates the signal (Dai, 2011a; Dai, 2011b; Sheffield and Wood, 2008).’ I could not see these features in Fig. 2.33b. There are no data (grey color) over the Sahel and over almost over all Africa. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly.
2-2204	2	56	32	56	33	The data do not show what is happening now, but what has happened in the past, so "to be declining" should instead be "to have declined." [Government of United States of America]	EDITORIAL: sentence amended
2-2205	2	56	32	56	33	‘On the whole the annual maximum number of consecutive dry days appears to be declining in most regions since the 1950s (Figure 2.33b).’ It seems to me that the number of grid boxes in Fig. 2.33b with positive and negative trend is very similar. Therefore, I do not see that this figure supports this conclusion. As a matter of fact, this figure does not show any significant trend and the regions with data are very scattered. Therefore, it should be withdrawn. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly.
2-2206	2	56	32	56	38	I find this paragraph confusing. Figure 2.33b shows very few areas with statistical significance. Why is the conclusion that “the annual maximum number of consecutive dry days appears to be declining in most regions”? What does an increase in hydroclimatic intensity mean? Are both results consistent? Are they consistent with other studies on drought trends? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly.
2-2207	2	56	33	56	37	This result is based on a single paper. Should it be given such prominence in this report (a figure) ? At any rate this “hydroclimatic intensity” variable should be defined/interpreted (its meaning is not readily obvious). [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly. Hydroclimatic intensity appears in Box 2.4 and is referred to here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2208	2	56	34	56	34	It should be made clearer that the Giorgi et al analysis only covers the period 1960-2000. [Government of Australia]	TAKEN INTO ACCOUNT: It is clear from the figure that results are only shown from 1976 to 2000.
2-2209	2	56	36	56	36	Change to either “trends are negative” or “hydroclimatic intensity has decreased”. [Ileana Bladé, Spain]	EDITORIAL
2-2210	2	56	37	56	37	Insert, after “(Fig. 2.33c).”, “Goswami et al. (2006) indicate that extreme rainfall events are increasing in the central homogeneous India at the expense of weaker events.” [Government of India]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2211	2	56	37	56	38	One would be stronger here and say the poor availability of data and analysis prevents drawing conclusions at a global scale. [European Union]	NOTED
2-2212	2	56	41	56	41	Is the legend for panel (b) correct? It seems that “frequency of” should be removed or something else is wrong. And is the scale correct? Has the maximum number of consecutive dry days really varied by 1-10 days per year? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: The figure and text have been amended accordingly.
2-2213	2	56	45	56	45	Capital 'C' required for 'century' [Peter Burt, United Kingdom]	EDITORIAL
2-2214	2	56	49	56	49	Are hail or thunderstorms considered as hydrological cycle? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: Yes
2-2215	2	56	49	56	51	Although thunderstorms may indeed be poorly detected by surface meteorological networks, we are seeing the emergence of lightning imagers on satellites. Some mention of this could be made in this paragraph. [Adrian Simmons, United Kingdom]	TAKEN INTO ACCOUNT: Some additional text has been added to this section.
2-2216	2	56	49	57	6	It seems like a lot of space is devoted to discussing hail changes even though the results are regionally-dependent and therefore inconclusive. Just because there have been studies on this topic since AR4 does not mean they need to be mentioned. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Given that we cannot make 'global' conclusions it seems appropriate to at least mention some of the large scale regional studies that exist. We have however amended the text to try and draw out a more consistent assessment.
2-2217	2	56	49	57	6	It would be useful to compare the assessment of small-scale severe weather with AR4. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	NOTED
2-2218	2	56	49	57	6	Tornadoes were mentioned in AR4 but not in this section, although the Verbout and Doswell references are relevant. They are mentioned specifically in the executive summary. [John Caesar, United Kingdom of Great Britain & Northern Ireland]	NOTED: This section and the Executive Summary have been revised
2-2219	2	56	49	57	6	No mention is made of hail studies for South America. The paper R.N. Mezher et al. / Atmospheric Research 114–115 (2012) 70–82 analyzes hail climatology and trends for Argentina [Moira Evelina Doyle, Argentina]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2220	2	56	49	57	6	Ren et al. (2012) recently summarized the changes in frequencies of extreme climate events in mainland China over the time period 1951-2008, and showed that thunder storms have significantly decreased in all of the areas investigated in eastern China during 1961-2008 (Ren, G., Y. Ding, Z. Zhao, J. Zheng, T. Wu, G. Tang, and Y. Xu, 2012, Recent progress in studies of climate change in China, Advance in Atmospheric Sciences, 29 (5): 958-977). [Guoyu Ren, China]	TAKEN INTO ACCOUNT: The authors cannot find this manuscript.
2-2221	2	56	49	57	6	Can this be related to changes in sub-daily precipitation intesnity? [Geert Jan van Oldenborgh, Netherlands]	NOTED
2-2222	2	56	49	57	21	As of 19 April 2012, a review of climate change and severe thunderstorms has been available online (http://dx.doi.org/10.1016/j.atmosres.2012.04.002) Brooks, H. E., 2012: Severe thunderstorms and climate change. Atmos. Res., 112, in press. [Harold Brooks, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2223	2	56	50	56	50	observed → monitored? [Ileana Bladé, Spain]	EDITORIAL
2-2224	2	56	51	56	52	Doswell et al. (2009) and Verbout et al. (2006) 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, United States of America]	AGREED: Text amended accordingly
2-2225	2	56	54	56	54	Change "associated with very high uncertainty" to "very uncertain". [J. Graham Cogley, Canada]	EDITORIAL
2-2226	2	56	56	32	33	Check again this conclusion I don't see strong evidence of consecutive dry days (CCD) declining in most	TAKEN INTO ACCOUNT: The figure and text have

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						regions. [European Union]	been amended accordingly. The figure has been improved to show that CDD is indeed declining in most regions.
2-2227	2	57	8	57	8	"In summary, analyses …", are these NEW analyses? [Xuebin Zhang, Canada]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2228	2	57	8	57	11	Bad grammar (illogical usage of “more than”). Not readily fixable. Perhaps change to "Regionally, there have been more increases than decreases in the number of heavy precipitation events”. [Ileana Bladé, Spain]	EDITORIAL
2-2229	2	57	9	57	10	"more statistically significant regional increases than regional decreases in …, but". [J. Graham Cogley, Canada]	EDITORIAL
2-2230	2	57	11	57	12	You already said that there are regions where heavy precipitation has decreased. I suggest shortening to “but many of the trends are not statistically significant and there are strong regional and subregional variations in the trends. Additionally, there are variations between seasons (e.g., more consistent trends in winter than in summer in Europe)." [Ileana Bladé, Spain]	EDITORIAL
2-2231	2	57	13	57	13	Delete "overall". [J. Graham Cogley, Canada]	EDITORIAL
2-2232	2	57	13	57	14	About the sentence: "The overall most consistent trends towards heavier precipitation events are found in North America (likely increase over the continent)." Actually, North America is a sub-continent and form, with Cental and South America and Caribe, the American continent. [Rubén D Piacentini, Argentina]	NOTED
2-2233	2	57	16	57	19	A negative statement concerning high confidence is unusual in this document. If it is specifically meant to negate an AR4 statement, but this could be made clear. [Government of United States of America]	ACCEPTED: The text in this section has been amended accordingly and use of the negative statement has been removed.
2-2234	2	57	16	57	19	This sentence: ‘The current assessment does not support the AR4 conclusions regarding global increasing trends in droughts but rather concludes that there is not enough evidence at present to suggest high confidence in observed trends in dryness due to lack of direct observations, some geographical inconsistencies in the trends, and some dependencies of inferred trends on the index choice.’ should be written in another way, perhaps: ‘The current assessment does not support the AR4 conclusions regarding global increasing trends in droughts but rather concludes that there is low confidence in trends in dryness due to lack of direct observations, some geographical inconsistencies in the trends, and some dependencies of inferred trends on the index choice.’ [Alice Grimm, Brazil]	NOTED
2-2235	2	57	16	57	19	I agree with the revision of the IPCC AR4 assessment on historical drought changes, especially since this assessment was mostly addressing global-scale changes in drought (which is not meaningful, given that there are both regions with increasing and decreasing drought trends, beside regions with low confidence in the trends). However, a more recent study has brought more light on this issue (Sheffield et al. 2012, Nature). It argues that some of the discrepancies found in the literature regarding global-scale drought trends can be explained with issues with a common version of the model used to compute the Palmer-Drought Severity Index (using the Thornthwaite-based formulation for potential evaporation). Their results suggest that these issues strongly affect global-scale drought trends. However, regional trends in drought in that study agree well with the IPCC SREX assessment of medium confidence of drying (southern Europe, West Africa) or wetting (central North America, northwestern Australia) trends in some regions (see also Seneviratne 2012 for a discussion), as well as with a range of studies using different indices and methods (e.g. see IPCC SREX chapter 3, Table 3.2; Dai 2011; Dai 2012). I would recommend that the chapter authors possibly consider providing assessments regarding observed drought changes in larger regions, setting the confidence level at "medium" for those regions in which all available studies agree on the trend sign independently of methodology, input dataset, and index choice. References: 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575; 2) Seneviratne, S.I, Nature, 491, 338-339; 3) Dai, A. Wiley Interdisc. Rev. Clim. Change 2, 45–65 (2011); 4) 1) Dai, A. Nature Clim. Change http://dx.doi.org/10.1038/NCLIMATE1633 (2012). [Sonia Seneviratne, Switzerland]	ACCEPTED: It is agreed that some refinement is reqired to the drought section to give more robust statements on regional trends. To this end we have rewritten this section and included a new table which focuses on continental changes in regional extremes.
2-2236	2	57	16	57	21	Very good. The reassessment of trends in droughts is necessary, and it has been done objectively and in a balanced manner. It is also good to have differentiated agricultural drought, hydrological drought and	NOTED

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						meteorological drought, and to have pointed out the limits of any meteorological drought indices. Many thanks. [Guoyu Ren, China]	
2-2237	2	57	17	57	18	This is an odd formulation to state as a conclusion what you are NOT able to do. As written, you have not ruled-out that you might have medium confidence (or low confidence) in globally observed trends of dryness, but this is left to the readers own interpretation. Strongly suggest rewriting as a positive statement. egg, "In contrast to AR4, the available evidence currently provides only XXX confidence in observed trends in dryness due to....." [Thomas Stocker/ WGI TSU, Switzerland]	ACCEPTED: The text in this section has been amended accordingly and use of the negative statement has been removed
2-2238	2	57	17	57	20	From this current concluding statement for droughts, it is left unclear whether or not the AR5 continues to support the SREX findings of medium confidence of increasing dryness in some regions (egg, southern Europe, West Africa) and decreasing dryness in other regions (NW Australia, North America). This needs to be stated, with relevant regions listed. [Thomas Stocker/ WGI TSU, Switzerland]	ACCEPTED: The text in this section has been amended accordingly and use of the negative statement has been removed
2-2239	2	57	18	57	19	The wording is difficult here - suddenly we are talking about 'dryness' not droughts and we don't have high confidence in observed trends of drying but what do we have? AR4 concluded it was likely droughts had increased - how does this compare to 'not high confidence'? please be explicit. [European Union]	ACCEPTED: The text in this section has been amended accordingly and use of the negative statement has been removed
2-2240	2	57	20	57	21	The "very low confidence" for the "trends in small-scale severe weather phenomena" seems to me even exhaggerated: compared to other phenomena or variables, this seems better defined by 'very low', or inexistent [Claudio Cassardo, Italy]	REJECTED: This uses the pre-defined IPCC uncertainty language.
2-2241	2	57	23			The authors switch between Hurricanes, Tropical Storms, Typhoons and Tropical Cyclones. Would be preferable to use just one term and then advise the reader that these are broadly interchangeable. [Government of Australia]	NOTED. We try to amend text and figure captions to make definitions more consistent. However in some cases the different terminologies are appropriate.
2-2242	2	57	31	57	33	This comment is not justified, in fact AR4 statements all still apply and quite well, I might add. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: We have revised this introductory statement to clarify that our statement refers to the pre-satellite era.
2-2243	2	57	35	57	35	Box 14.2 is meant [Ileana Bladé, Spain]	Noted. This aseessment now appears in Section 14.6.1
2-2244	2	57	35	57	43	No mention of time periods in these statements. [Dian Seidel, United States of America]	ACCEPTED: Text amended accordingly
2-2245	2	57	35	57	48	We have constructed a homogeneous record of cyclone activity since 1923 based on instrumental data which does not suffer the temporal bias problems of hurdat. This is basically a measure of atlantic tropical cyclone activity based on tide gauge data. In this we see a significant rising trend. Please add this paper to the discussion in this paragraph. Aslak Grinsted, John C. Moore, and Svetlana Jevrejeva (2012), Homogeneous record of Atlantic hurricane surge threat since 1923, PNAS, doi:10.1073/pnas.1209542109 [Aslak Grinsted, Denmark]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion. Most of the discussion of tropical cyclone trends is contained in Ch 14.
2-2246	2	57	35	58	48	This section fails to deal with changes in strong hurricanes greater than cat 3, say, which Holland, Emanuel, etc have shown are increasing and also are expected to increase. There is work in press on this. Also this topic is extensively dealt with in Chapter 14 and needs to be reconciled. The same comment applies to "extratopical cyclones" sec 2.6.4 which is also in Ch 14. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: As noted by the reviewer Chapter 14 covers most of the assessment of observed changes in tropical cyclones. Currently we see no conflict between the amended text in Chapter 2 and what appears in Chapter 14. In fact we refer to Chapter 14 in this section for more detail.
2-2247	2	57	38	57	42	Besides the trends, the abrupt increase of intense tropical cyclones in early summer is also found in the western Pacific (Tu et al. 2001; Environ. Res. Lett., 6, 034013) and the India Ocean (Evan et al. 2011; Nature, 479, 94-97). [Chia Chou, Taiwan, ROC]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion. Most of the discussion of tropical cyclone trends is contained in Ch 14. We only summarise their conclusions here.
2-2248	2	57	38	57	48	Please include the assessment results by the UN ESCAP/WMO Typhoon Committee Expert Team on the impacts of tropical cyclone (TC) activity in the Typhoon Committee region (western North Pacific basin) with a focus on the possible changes in TC track and impact areas (Lee et al., 2012a) and the recent study by Lee et	NOTED. Data used in these publications come from government publication (CMA, 2011) which we reference as the original source.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						al. (2012b) which examines the long-term variations of TC frequency and intensity in the South China Sea and the vicinity of Hong Kong from 1961 to 2010 based on the best track data of four main weather agencies in the western North Pacific. References : - Lee, T. C., T. R. Knutson, H. Kamahori, and, M. Ying, 2012a: Impacts of Climate Change on Tropical Cyclones in the Western North Pacific Basin. Part I : Past Observations. Tropical Cyclone Res. Rev. 1, 213-230. http://tcrr.typhoon.gov.cn/EN/abstract/abstract30.shtml - Lee T. C., Y. Y. Leung, M. H. Kok and H.S. Chan, 2012b: The long term variations of tropical cyclone activity in the South China Sea and the vicinity of Hong Kong, accepted for publication in Tropical Cyclone Res. Rev. http://tcrr.typhoon.gov.cn/EN/abstract/abstract20.shtml [Sai Ming Lee, Hong Kong, China]	
2-2249	2	57	41	57	41	To the list of references starting with Holland and Webster, Deo et al. (2012) may be added. [Government of India]	TAKEN INTO ACCOUNT: We presume that the reviewer was referring to the Deo et al. 2011 paper in Natural Hazards.The authors have assessed this manuscript for inclusion. The trends assessed in this paper are much shorter than the long-term trends referred to in the sentence and therefore we do not think that this reference belongs here. Most of the discussion of tropical cyclone trends is contained in Ch 14.
2-2250	2	57	45	57	47	Weinkle 2012 is mentioned (Historical global tropical cyclone landfalls, Jessica Weinkle, Ryan Maue and Roger Pielke, Jr., Journal of Climate http://dx.doi.org/10.1175/JCLI-D-11-00719.1) referring to Australia. However as the title suggests this is not a regional but a global trend analysis. I think their figure 2 (see http://rogerpielkejr.blogspot.nl/2012/04/historical-tropical-cyclone-landfalls.html for a quick link) deserves to be shown in the final report. As far as I know this is the most recent and extensive analysis of global cyclone trends. The official figure 2 is here and shows the data for the different ocean basins as well: http://journals.ametsoc.org/na101/home/literatum/publisher/ams/journals/content/clim/2012/15200442-25.13/jcli-d-11-00719.1/production/images/large/jcli-d-11-00719.1-f2.jpeg Their main conclusion: "Our analysis does not indicate significant long-period global or individual basin trends in the frequency or intensity of landfalling TCs of minor or major hurricane strength. This evidence provides strong support for the conclusion that increasing damage around the world during the past several decades can be explained entirely by increasing wealth in locations prone to TC landfalls, which adds confidence to the fidelity of economic normalization analyses." [Marcel Crok, The Netherlands]	REJECTED: It is clear from the text that we are not referring to Australia when we reference this paper. We do not agree that their figure 2 belongs in the text . This paper adds to our assessment and is one of many studies on which we have based our assessment. More extensive assessment of TCs is found in Ch 14.
2-2251	2	57	46	57	47	Reference Lee and McPhaden (2010) do not mention tropical cyclones, should not be mentioned here. [Fabrice Chauvin, France]	ACCEPTED: reference removed
2-2252	2	57	46	57	47	One more reference land-falling tropical cyclones, Tu et al. (2009; J. Climate, 22, 3617-3628) can be added here. [Chia Chou, Taiwan, ROC]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2253	2	57	46	57	47	Lee and McPhaden 2010 does not discuss tropical storms. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: reference removed
2-2254	2	57	47	57	47	summarizes → shows [Ileana Bladé, Spain]	EDITORIAL
2-2255	2	57	47	57	47	After “Weinkle et al. 2012.”, insert (“The tropical cyclone frequency in the Bay of Bengal has weakened in the recent decades (Rao et al., 2004, Deo et al., 2011), while the Arabian sea is seen to be more cyclogenic (Deo et al., 2011) [Government of India]	TAKEN INTO ACCOUNT: The authors have assessed these manuscripts for inclusion. Most of the discussion of tropical cyclone trends is contained in Ch 14. We feel that our previous sentence makes it clear that there is no clear evidence of long term trends in land falling tropical cyclones in most ocean basins.
2-2256	2	57				Figure 2.34: The Y-Axes of "Normalized Units" is difficult to interpret. Please add a more descruiptive title. [Jeffrey Taylor, United States of America]	TAKEN INTO ACCOUNT: The figure has been amended.
2-2257	2	58	1	58	6	There was an increase in the most intense cyclones (Cat. 4-5) in the Western Pacific Region from 1990 -	TAKEN INTO ACCOUNT: The authors have assessed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2009, but this came at the expense of Cat (1-2) storms, so there was no overall trend in the region. Ref: Lupo, A.R., 2011: Interannual and Interdecadal variability in hurricane activity. Hurricane Research, ISBN 978-953-307-238-8, Intech Publishers, Vienna. (Chapter 1) Lupo, A.R. Book editor, Recent Hurricane Research: Climate, Dynamics, and Societal Impacts, 616 pp. [Anthony Lupo, United States of America]	this manuscript for inclusion.
2-2258	2	58	2	58	3	Elsner et al., 2008 show also increases in the lifetime-maximum wind speeds of the strongest storms for northern and southern Indian Ocean cyclones [Ianis Delpla, France]	NOTED
2-2259	2	58	3	58	3	Suggest reference to Kuleshov, Y., R. Fawcett, L. Qi, B. Trewin, D. Jones, J. McBride, and H. Ramsay (2010), Trends in tropical cyclones in the South Indian Ocean and the South Pacific Ocean, J. Geophys. Res., 115, D01101, doi:10.1029/2009JD012372. [Government of Australia]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2260	2	58	5	58	6	This assessment will be published in 2013, and the phrase "over the past 25 years" might be interpreted by the reader as meaning 1988-2012. But the reference is to Emanuel(2007) who presumably was discussing a somewhat earlier 25-year period. [Adrian Simmons, United Kingdom]	NOTED: We have amended the text to avoid confusion.
2-2261	2	58	8	58	8	which further elucidated the scope of uncertainties → further to what? This is too verbose anyway. [Ileana Bladé, Spain]	EDITORIAL: In any case the text has been amended so this phrase no longer appears
2-2262	2	58	8	58	11	Knutson et al concluded: that given the large amplitude of fluctuations observed in the frequency and intensity of tropical cyclones it remains uncertain whether past changes have exceeded the variability caused by natural causes - I think it more useful to quote Knutson et al directly rather than this convoluted and ambiguous statement. [European Union]	REJECT: This is outside of the scope of Chapter 2 which focuses on observations not causes. However we have amended the text to make this clearer.
2-2263	2	58	8	58	14	Please, express the conclusions in this paragraph in a clearer and more direct way. [Alice Grimm, Brazil]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2264	2	58	9	58	9	It is bizarre to not conclude something. Since this conclusion is not what was actually concluded in AR4 either, I can't even suggest that it be changed to “no longer concludes”. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2265	2	58	9	58	10	Description of Knutson result is strange. Instead of telling us what they do not conclude, please tell us what they do conclude :) [Paul Matthews, United Kingdom]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2266	2	58	9	58	12	"do not conclude": good to underline the differences with respect to AR4, but I would like also to see the "conclusions" for the annual number of tropical storms. This sentence is a summary, and in my opinion 'repetita juvant' [Claudio Cassardo, Italy]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2267	2	58	9			The wording 'do not conclude that it is likely that ' is awkward and likely to be misinterpreted. Consider rewording to say what the study concluded. [Government of United States of America]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2268	2	58	11	58	11	This is supported by our independent homogeneous data published here: Aslak Grinsted, John C. Moore, and Svetlana Jevrejeva (2012), Homogeneous record of Atlantic hurricane surge threat since 1923, PNAS, doi:10.1073/pnas.1209542109 [Aslak Grinsted, Denmark]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2269	2	58	11	58	12	This is not the same as what is stated in line 1 (increase in intensity of the strongest storms) and suggests, instead, that the number of very intense storms has increased. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2270	2	58	11	58	12	Please add a confidence level to this important statement regarding an increase in the intense North Atlantic tropical storms. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2271	2	58	12	58	12	does not revise → endorses? Maintains? [Ileana Bladé, Spain]	EDITORIAL
2-2272	2	58	12	58	14	There is evidence of a likely increase in tropical storms since 1970s so I find this sentence too negative - I would suggest leaving out altogether. [European Union]	TAKEN INTO ACCOUNT: This section has been rewritten to clarify what is meant.
2-2273	2	58	16			2.6.4: Extratropical storms: These paragraphs are problematic, since the reader gets the impression that there may be a minor problem in the Donat et al. (2011) analysis OVER LAND using the 20th century reanalysis	TAKEN INTO ACCOUNT: Text amended accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						data. However, most authors in the peer-reviewed literature agree that the main problem is found over the ocean, in this case the Atlantic (and extending to the North Sea), which appears logical, since it much easier to "miss" a cyclone over the ocean than over land when going back in time. I therefore suggest to consider something of the following: [Martin Stendel, Denmark]	
2-2274	2	58	16			(cont.) As direct wind observations usually cover periods too short to be suitable for analysis and/or suffer from inhomogeneities, several studies have relied on reanalyses or other reconstructions based on pressure observations. Independent of the method applied, these reconstructions show a rather high agreement with respect to long-term variations of storminess, and conclude that storminess over the European-Atlantic sector is dominated by multidecadal variations rather than long-term trends (Krueger and von Storch, 2011a, 2011b; Matulla et al., 2008; Wang et al., 2009c). [Martin Stendel, Denmark]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2275	2	58	16			(cont.) In contrast, Donat et al. (2011), using sea level pressure, sea surface temperature and sea ice extent from historical observations (Compo et al., 2011), find a strong increase in winter storminess since 1871 that goes along with a strong increase in the number of assimilated observations over time and likely is an artefact of changing station density comparable to other spurious trends in reanalysis data (e.g., Bengtsson et al., 2004; Paltridge et al., 2009; Dessler and Davis, 2010). [Martin Stendel, Denmark]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2276	2	58	16			(cont.) References: Krueger, O. and H. von Storch (2011a): Evaluation of an air pressure based proxy for storm activity. J. Climate 24, 2612-2619. - Krueger, O. and H. von Storch (2011b): The informational value of pressure-based single-station proxies for storm activity. J. Atmos. Oceanic Technol., 29, 569–580, doi: 10.1175/JTECH-D-11-00163.1. - Dessler, A.E. & S. M. Davis (2010): Trends in tropospheric humidity from reanalysis systems. J. Geophys. Res. 115, doi:10.1029/2010JD014192. [Martin Stendel, Denmark]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2277	2	58	16			This section is not discriminating about use of reanalyses: some should not be used at all, and some are much better than others. There is no confidence in this assessment. [Kevin Trenberth, United States of America]	ACCEPTED: It is agreed that as currently written there is little discrimination between reanalyses products and the section has been reworked to take this into account.
2-2278	2	58	24	58	24	Insert after '...work is required to consolidate findings for future assessments.': "A broad intercomparison study (Neu et al. 2012) has shown that the use of different algorithms to assess extratropical cyclones leads to rather large differences in the resulting frequency of total number of cyclones while for strong cyclones the frequency is much more robust between algorithms. Moreover, regions with strong (positive or negative) trends (reflecting e.g. the shift of strom tracks) are quit robust among methods, while trends averaged over ocean basins, continents or hemispheres are less consistent." Reference: Neu U., M.G. Akperov, N. Bellenbaum, R. Benestad, R. Blender, R. Caballero, A. Cocozza, H.F. Dacre, Y. Feng, K. Fraedrich, J. Grieger, S. Gulev, J. Hanley, T. Hewson, M. Inatsu, K. Keay, S.F. Kew, I. Kindem, G.C. Leckebusch, M.L.R. Liberato, P. Lionello, I.I. Mokhov, J.G. Pinto, C.C. Raible, M. Reale, I. Rudeva, M. Schuster, I. Simmonds, M. Sinclair, M. Sprenger, N.D. Tilinina, I.F. Trigo, S. Ulbrich, U. Ulbrich, X.L. Wang, H. Wernli, 2012: IMILAST – a community effort to intercompare extratropical cyclone detection and tracking algorithms: assessing method-related uncertainties. Bull. Am. Met. Soc, doi 10.1175/BAMS-D-11-00154.1 (published online 19 Sep 2012; http://journals.ametsoc.org/doi/pdf/10.1175/BAMS-D-11-00154.1) [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2279	2	58	24	58	39	The most recent study using the century-long 20CR reanalyses (Wang et al. 2012d) is completely missing in this section. I think this is the only existing in-depth study of global cyclone activity using century-long reanalyses with careful analysis and treatment of inhomogeneities and thus it is an important reference for this section. Therefore, I have the next few comments on this section. [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2280	2	58	26	58	26	Add "Wang et al., 2012d" after "Vilibic and Sepic, 2010". [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2281	2	58	27	58	27	Before the sentence "Some studies...", add the following or something similar: These changes in the North Atlantic cyclone activity are also reported by Wang et al. (2012d) using the ensemble of the Twentieth Century Reanalyses (20CR; Compo et al., 2011) with all identified inhomogeneities being accounted for. [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2282	2	58	29	58	30	These results belong to the following paragraph (where they are indeed found again). [Ileana Bladé, Spain]	ACCEPTED: Text amended accordingly
2-2283	2	58	29	58	30	Zhang et al. (2004) shows a decadal-scale variability of intensity and number of North Pacific and North American cyclones. [Xiangdong Zhang, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2284	2	58	29	58	30	Zhang, X., J. E. Walsh, J. Zhang, U. S. Bhatt, and M. Ikeda, 2004: Climatology and interannual variability of Arctic cyclone activity, 1948-2002. J. Climate, 17, 2300-2317. [Xiangdong Zhang, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2285	2	58	31	58	31	Replace references '(Leckebusch et al., 2006; Pinto et al., 2006)' by '(Ulbrich et al., 2009; Neu et al., 2012). Reason: More recent literature. (refs: Ulbrich U., G. C. Leckebusch, and J. G. Pinto, 2009: Extra-tropical cyclones in the present and future climate: a review. Theor. Appl. Clim., 96, 117-13; Neu U., M.G. Akperov, N. Bellenbaum, R. Benestad, R. Blender, R. Caballero, A. Cocozza, H.F. Dacre, Y. Feng, K. Fraedrich, J. Grieger, S. Gulev, J. Hanley, T. Hewson, M. Inatsu, K. Keay, S.F. Kew, I. Kindem, G.C. Leckebusch, M.L.R. Liberato, P. Lionello, I.I. Mokhov, J.G. Pinto, C.C. Raible, M. Reale, I. Rudeva, M. Schuster, I. Simmonds, M. Sinclair, M. Sprenger, N.D. Tilinina, I.F. Trigo, S. Ulbrich, U. Ulbrich, X.L. Wang, H. Wernli, 2012: IMILAST – a community effort to intercompare extratropical cyclone detection and tracking algorithms: assessing method-related uncertainties. Bull. Am. Met. Soc, doi 10.1175/BAMS-D-11-00154.1 (published online 19 Sep 2012; http://journals.ametsoc.org/doi/pdf/10.1175/BAMS-D-11-00154.1). [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: The authors have assessed these manuscripts for inclusion. This section has been rewritten.
2-2286	2	58	31	58	31	A very important point is made here, in that the nature of the changes in cyclone behavior is, at least in part, associated with the precise technique use to identify and quantify cyclones. It would be very valauble here to cite a recent international collaborative effort on this very topic **** Neu, U., M. G. Akperov, N. Bellenbaum, R. Benestad, R. Blender, R. Caballero, A. Cocozza, H. F. Dacre, Y. Feng, K. Fraedrich, J. Grieger, S. Gulev, J. Hanley, T. Hewson, M. Inatsu, K. Keay, S. F. Kew, I. Kindem, G. C. Leckebusch, M. L. R. Liberato, P. Lionello, I. I. Mokhov, J. G. Pinto, C. C. Raible, M. Reale, I. Rudeva, M. Schuster, I. Simmonds, M. Sinclair, M. Sprenger, N. D. Tilinina, I. F. Trigo, S. Ulbrich, U. Ulbrich, X. L. Wang and H. Wernli, 2012: IMILAST: A community effort to intercompare extratropical cyclone detection and tracking algorithms: Assessing method-related uncertainties. Bulletin of the American Meteorological Society, doi: 10.1175/BAMS-D-11-00154.1 (in press). [Ian Simmonds, Australia]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2287	2	58	33	58	35	Do you mean cyclonic activity in general, as opposed to intense cyclones only? Then perhaps the general result should be quoted first and the result on intense cyclones second. [Ileana Bladé, Spain]	NOTED: This sentence has been removed.
2-2288	2	58	33			Would be better to start the line "For the North Pacific" not "In the North Pacific". [Adrian Simmons, United Kingdom]	EDITORIAL: Section rewritten
2-2289	2	58	36	58	36	also in this region -> delete. Even then, this in awkward sentence. [Ileana Bladé, Spain]	EDITORIAL: Section rewritten
2-2290	2	58	38	58	38	Delete Geng and Sugi 2001 which deals with the North Atlantic. [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Section rewritten and reference deleted
2-2291	2	58	41	58	49	How does this section of decreasing extra-tropical storms over Europe link up with the section on surface winds which include the findings of stilling over land? Are the extremes changing in concert with the mean here? This is covered to some extent in page 59 lines 28-36 but again the distinction between mean and extremes is not made. [Kate Willett, United Kingdom]	TAKEN INTO ACCOUNT: Some amendments have been made to the text to clarify the difference between mean and extremes.
2-2292	2	58	44	58	44	but with some regional and seasonal trends -> and only a few regional and seasonal trends (otherwise sentence seems contradictory) [Ileana Bladé, Spain]	EDITORIAL
2-2293	2	58	45	58	45	Reference of Wang et al. 2011 should be Climate Dynamics vol. 37 pages 2355-2371, doi 10.1007/s00382-011-1107-0 [David Parker, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference amended
2-2294	2	58	45	58	45	Replace "(2009c); Wang et al. (2011)" with "2009c; Wang et al., 2011)" because the parentheses are not properly matched. [Xiaolan Wang, Canada]	EDITORIAL
2-2295	2	58	46	58	46	As far as I can tell, there are only 2(1) regions with a statistically significant downward (upward) trends in Fig. 2.35, which, presumably shows the most remarkable results. Can you really conclude that "decreasing trends outnumber increasing trends" ? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This sentence has been amended.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2296	2	58	46	58	49	Please add the following reference supporting this statement: Krüger, O., F. Schenk, F. Feser and R. Weisse 2012: Inconsistencies between long-term trends in storminess derived from the 20CR reanalysis and observations, J. Climate, doi:10.1175/JCLI-D-12-00309.1 [Ralf Weisse, Germany]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2297	2	58	49	58	49	cite here in addition to Cornes and Jones (2011a): Krueger et al., 2012 (Inconsistencies between long-term trends in storminess derived from the 20CR reanalysis and observations, 2012: Oliver Krueger, Frederik Schenk, Frauke Feser, and Ralf Weisse, J. Climate, doi: http://dx.doi.org/10.1175/JCLI-D-12-00309.1) [Frauke Feser, Germany]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2298	2	58	49	58	49	Add "Wang et al., 2012d" after "Cornes and Jones, 2011a". [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2299	2	58	49	58	49	Add to the end of this paragraph the following or something similar: "However, after accounting for all identified inhomogeneities, Wang et al. (2012d) found that cyclone activity over northern Europe has increased signficantly since 1871 but has no significant trend since 1951. For the North Sea region, Wang et al. (2012d) found that the 20CR cyclone trends are in agreement with trends in geostrophic wind extremes derived from in-situ surface pressure observations, and in the mean duration of wet spells derived from rain gauge data in Europe." [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: This paragraph has been rewritten.
2-2300	2	58	49	59	49	Cornes and Jones (2011a and b) is the same paper! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: Reference amended
2-2301	2	58	56	58	57	"… trends of at least 5% significance." This is a sloppy formulation. Please use correct statistical langauge. [Ralf Weisse, Germany]	ACCEPTED: Text amended accordingly
2-2302	2	59	2	59	5	Something is grammatically wrong with this sentence. [Ileana Bladé, Spain]	EDITORIAL: Sentence amended
2-2303	2	59	4	59	5	When you note both a northward shift in cyclone activity and increased cyclone frequency at high latitudes (here and elsewhere), it is not clear to me whether the latter is the manifestation of the former, or the latter is in addition to the former. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This sentence has been revised.
2-2304	2	59	5	59	5	Should 50 N read 60 N here? [Government of Australia]	TAKEN INTO ACCOUNT: This section has been revised.
2-2305	2	59	5	59	5	Before the sentence "The decreases ...", please add the following or something similar: "Analyzing the Twentieth Century Reanalyses with all identified inhomogeneities being accounted for, Wang et al. (2012d) found that cyclone activity has decreased notably in east Asia (30N-45N), especially in summer over the period 1951-2010." [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: This section has been revised.
2-2306	2	59	9	59	9	The sentence beginning "those changes" is somewhat ambiguous - it is unclear whether it refers to "a few exceptions" (end of previous sentence) or the changes in most parts of the globe referred to earlier in the previous sentence. Please reword to clarify. [David Wratt, New Zealand]	Noted: Think numbering is incorrect - these sentences do not appear here
2-2307	2	59	9	59	16	What reanalyses? Some of this is quite dated. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: Some discrimination is now made between reanalyses products.
2-2308	2	59	16	40	22	FAQ 2.1 (Evidence for warming): The two figures are good. However I think the text would be improved by making it more quantitative - quoting numbers out of the chapter text for the mangtude of trends and changes, rather than just making qualitative statements. I've made suggestions below relating to individual paragraphs where I think this could be done. [David Wratt, New Zealand]	Noted: This is referring to FAQ 2.1 so think numbering is incorrect here.
2-2309	2	59	18	59	18	Add 'over south-eastern Australia' after 'storminess'. [Government of Australia]	ACCEPTED: Text amended accordingly
2-2310	2	59	18	59	19	Where has this decrease been observed? Near 30ºS ? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2311	2	59	18	59	19	The results in Alexander and Power 2009 and Alexander et al 2011 are specifically for the southeast Australian region, and do not apply to the whole Southern Hemisphere as implied here. [Government of Australia]	ACCEPTED: Text amended accordingly
2-2312	2	59	18			where in the SH is the decline? [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: Text amended accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2313	2	59	20	59	20	Please add after this sentence the following or something similar: "Using the 20CR ensemble (Compo et al., 2011) with all identified inhomogeneities being accounted for, Wang et al. (2012d) found that extratropical cyclone activity has increased substantially in the SH (20S-90S), and that, for southeast Australia, the 20CR cyclone trends are in agreement with trends in geostrophic wind extremes derived from in-situ surface pressure observations." [Xiaolan Wang, Canada]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2314	2	59	24	59	25	" … studies tend to aggree …" This needs to be specified and discussed in more detail. In particular, the SH is particularly data sparse in earlier years. Increasing availability and density of data could be expected to produce such effects. Please discuss to what extent these contributions have been discounted for in the previous statement or rewrite the statement accordingly. [Ralf Weisse, Germany]	TAKEN INTO ACCOUNT: Text amended accordingly
2-2315	2	59	28	59	29	Does "wind extremes" refer to intensity or frequency ? Line 50 (FAQ 2.2) explicitly states "wind frequency" in reference to this result. Is this correct? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This refers to both frequency and intensity
2-2316	2	59	28	59	36	This paragraph (like others in Chapter 2) discusses reanalysis and differences between products without distinguishing between the results of different generations of reanalysis. There is no paper referenced in this paragraph that has a publication date later than 2008, so presumably none of what is discussed relates to the latest generation of reanalyses. It's hard to know how to deal with this issue, but I would draw attention to my earlier comment 77. [Adrian Simmons, United Kingdom]	TAKEN INTO ACCOUNT: We agree that some discrimination between products is required. We have tried to do this by some changes to the text in section 2.6.4 and we refer the reader to Box 2.4 where reanalyses products are discussed in more detail.
2-2317	2	59	30	59	31	A most recent study, using the high resolution 3-hourly Norht American Regional Reanalysis (NARR) from 1979-2009, found an increase in both monthly mean and exstreme winds over the Chukchi and Beaufort Seas of Arctic Ocean (Stegall and Zhang 2012). [Xiangdong Zhang, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2318	2	59	30	59	31	Stegall, S. T., and J. Zhang, 2012: Wind field climatology, changes, and extremes in the Chukchi-Beaufort Seas and Alaska North Slope during 1979-2009. J. Climate, doi: 10.1175/JCLI-D-11-00532.1 [Xiangdong Zhang, United States of America]	TAKEN INTO ACCOUNT: The authors have assessed this manuscript for inclusion.
2-2319	2	59	34	59	36	I would suggest shortening this sentence to "Trends extracted from reanalysis products must be treated with caution (Box 2.3)." The rest of what is in the sentence is already covered by Box 2.3, Bengtsson et al.(2004) is a rather old reference and it's not the best one, and the problem is not fundamentally in the data assimilation methods, but rather in significant changes in observational coverage for variables for which there is either observational bias or bias in the model used for data assimilation system. Changes in observational coverage are a problem for climate analysis in general, not just for reanalysis. An example is provided earlier in the chapter, for the analysis of upper stratospheric temperature trends, for which there is an absence of datasets that unite the SSU and AMSU-A satellite records. Reanalysis in fact comes closest to doing a reasonable job for this. [Adrian Simmons, United Kingdom]	ACCEPTED: Text amended accordingly
2-2320	2	59	34	59	36	what period? There are many other references on reanalyses. [Kevin Trenberth, United States of America]	ACCEPTED: Text has been revised
2-2321	2	59	35	59	35	I think instead of "data assimilation methods" the author means "assimilation of the changing observing system". Data assimilation methods are generally fixed in a reanalysis, but when an observing system enters of leaves the time series, trends can be inlfuenced. [Michael Bosilovich, United States of America]	ACCEPTED: Text amended accordingly
2-2322	2	59	38	52	42	In the summary it should refer to the northward movement of cyclones in the N Atlantic (supported later by evidence of SLP and storm tracks). [European Union]	TAKEN INTO ACCOUNT: This section has been rewritten.
2-2323	2	59	43	61	9	FAQ 2.2 (Climate Extremes): I suggest this could be improved by being more specific about what has changed (e.g. rather than making a general comment that "studies … indicate changes associated with warming …" (lines 6-13 of page 60) say that "it is very likely that the overall number of cold days and nights has decreased and the overall number of warm days and warm nights has increased on the global scale between 1951 and 2010". [David Wratt, New Zealand]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2324	2	59	46	61	9	General comment: language of FAQ 2.2. generally is more 'complicated' than of other FAQs. I have made some suggestions. [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2325	2	59	46			FAQ 2.2: The intention was that the formal uncertainty language would not be used in the FAQs. But we see that it is really difficult to answer this FAQ without using this language. We would however recommend that the	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						instances of 'likely' (line 50, line 36) are avoided in the text as the reader will not be familiar with the quantitative basis for these terms. [Thomas Stocker/ WGI TSU, Switzerland]	
2-2326	2	59	48	59	49	As noted earlier, a major portion of this observation, which is based on station records, is the influence of surface development on the nocturnal boundary layer coupling/decoupling phenomenon (Christy et al. 2009 last section, McNider et al. 2012). [John Christy, United States of America]	REJECTED: The majority of literature does not agree with this as a global scale assessment and there is only limited regional studies that suggest otherwise.
2-2327	2	59	48	59	52	It should be emphasize that this is a global average [Government of Australia]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2328	2	59	48	59	52	This italicized statement is not a good summary. How is this compatible with DTR? There is excellent confidence in changes in tropical cyclones over the N Atlantic. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2329	2	59	50	59	50	Replace "Heavy precipitation events show likely, but regionally dependent, increases" by "Increases in heavy precipitation events are likely, but regionally different". Reason: easier to understand. [Urs Neu, Switzerland]	EDITORIAL
2-2330	2	59	54	59	54	Replace "snaps, droughts" by "...snaps or droughts...". Reason: reading the sentence becomes easier. [Urs Neu, Switzerland]	EDITORIAL
2-2331	2	59	55	59	56	Replace "not just because of the intrinsically rare nature of these events, but because, they invariably happen in conjunction with disruptive conditions" by "not just because these events are rare, but because they happen in exceptional conditions". Reason: Keep language as easy as possible. [Urs Neu, Switzerland]	EDITORIAL
2-2332	2	59	57	59	57	Replace "complicates global assessment" by "complicates a comparative global assessment". Reason: more clear. [Urs Neu, Switzerland]	EDITORIAL
2-2333	2	60	2	60	2	Replace "while" by "although" [Urs Neu, Switzerland]	EDITORIAL
2-2334	2	60	3	60	3	will be a different temperature to a hot day -> at a different temperature than a hot day [Ileana Bladé, Spain]	EDITORIAL
2-2335	2	60	3	60	3	Suggest deleting "collaborative" - I think that is implicit in "international". [Francis Zwiers, Canada]	EDITORIAL
2-2336	2	60	3	60	4	Replace "to monitor extremes" by "to create comparable descriptions of extremes". Reason: original sentence makes no sense. [Urs Neu, Switzerland]	EDITORIAL
2-2337	2	60	3			I suggest changing "will" to "may" or otherwise rewording the sentence. Remember that Death Valley in California, which (after recent judgment) now holds the record for hottest reliably-reported temperature, is located at 36N. But there are some places that are cold for much of the year at this latitude. [Adrian Simmons, United Kingdom]	EDITORIAL
2-2338	2	60	6	60	13	Paragraph is quite complicated and difficult to understand, and contains no expllicit result (as e.g. the number of cold days has decreased and the number of warm days has increased). The following is suggested: "For example, studies using consistent definitions for cold (<10th percentile) and warm (>90th percentile) days and nights indicate a decrease of cold days and nights and an increase of warm days and nights for most regions of the globe, except for daytime temperatures in central and eastern North America, and southern South America. Changes are generally most apparent in minimum temperature extremes. Limited data availability makes it difficult to establish a causal link of these changes to increases in average temperatures, but data in FAQ 2.2, Figure 1 indicates that daily global temperature extremes have indeed changed." (Comment: in FAQ 2.2, Fig.1 a change of spread and shape is not really visible). [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2339	2	60	9	60	9	minimum temperature extremes' - suggest providing an example to help reader understand what these include, egg, cold days, cold nights. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2340	2	60	10	60	13	These claims are not statistically tested. The fact that the two frequency distribution shown on page 2-181 are slightly different, does not proof that the difference is statistically significant. Please apply a Kolmogorov-Smirnov test. [Hans Visser, The Netherlands]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2341	2	60	11	60	13	The change in spread in both panels is pretty subtle. Perhaps the figure could be improved to show this more clearly? Also, is this a for the global average? [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2342	2	60	11			The authors should take care when discussing changes in the "shape" and "spread" of the distribution: it is not a peer-reviewed source, but Tamino's blog has an insightful post (http://tamino.wordpress.com/2012/07/21/increased-variability/) which fairly convincingly demonstrates that if something like temperature is calculated as the average of, say, two weather stations, and each weather station has a temperature distribution, and the temp distributions of both stations stay constant but one weather station has an increase in mean temperature, then the calculated average of the two stations will have both a change in the mean AND the shape/spread even though the underlying weather only changed in the mean. [Government of United States of America]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2343	2	60	12	60	13	I think this will be difficult for readers to see from FAQ 2.2, Fig 1, if they have not been trained to know what to look for, since the changes and shape and width appear to be small when presented in this way. Further, what region is depicted in FAQ 2.2, Fig 1? Mixing observations from climatologically different regions could distort whatever information there may be about changes in shape and spread, as would comparing distributions that are calculated from differing numbers of stations. I assume that units are degrees C. [Francis Zwiers, Canada]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2344	2	60	15	60	15	Replace "Warm spells or heat waves containing consecutive extremely hot days or nights have also been assessed" by "Warm spells or heat waves, i.e. periods containing consecutive extremely hot days or nights, have also been assessed" [Urs Neu, Switzerland]	EDITORIAL
2-2345	2	60	18	60	19	How can changes in frequency and duration show "cooling"? Do you mean they decreased? [Dian Seidel, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2346	2	60	19	60	19	Replace "cooling" by "a decrease". Frequency and duration cannot 'cool' [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2347	2	60	19	60	19	Replace "a so-called ‘warming hole’" by "less warming". Reason: avoid 'insider's' expressions if possible. [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2348	2	60	20	60	20	Reasoning is unclear. Why should heat wave frequency and/or duration be linked to extreme precipitation? A link to precipitation frequency seems much more comprehensible. Maybe delete "extreme" before precipitation. [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2349	2	60	22	60	22	What other regions beside Europe have such long records? Why not discuss them explicitly? [Dian Seidel, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2350	2	60	22	60	25	This paragraph refers to an unsupported statement made earlier in Page 54. [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2351	2	60	22	60	25	In the light of recent media attention this paragraph could probably do with either a reference or link to a point in Ch 2 where this (2001-2010 hot summers warmer than any decade since 1500) is demonstrated. [Kate Willett, United Kingdom]	REJECTED: FAQS SHOULD BE STAND ALONE WITH NO REFERENECES OR REFERENCE TO CHAPTER TEXT.
2-2352	2	60	23	60	23	that some -> that some regions [Ileana Bladé, Spain]	EDITORIAL
2-2353	2	60	23	60	23	"indications are that some...." countries? Sub-regions? [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2354	2	60	28	60	28	FAQ2.2, Figure 1 caption: Is this data global or regional? Insert correspondingly after "distribution of .... " [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2355	2	60	28			FAQ, see comment earlier about possible use of Hansen's work. That is not daily data but a different aspect of extremes. [Kevin Trenberth, United States of America]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2356	2	60	34	60	34	Say increases rather than changes. [European Union]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2357	2	60	35	60	35	Please indicate whether the "increases in more extreme precipitation events .." refers to increases in the number of such events, or in the rainfall during such events, or both. [David Wratt, New Zealand]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2358	2	60	35	60	36	Replace "but results are very regionally and seasonally dependent" by "but results vary strongly with regions	TAKEN INTO ACCOUNT: This FAQ has been

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						and seasons". [Urs Neu, Switzerland]	rewritten for simplicity and clarity.
2-2359	2	60	36	60	36	Insert "are" before "seasonally dependent". [Francis Zwiers, Canada]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2360	2	60	37	60	37	Insert "some" before "other regions". [Francis Zwiers, Canada]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2361	2	60	37	60	38	FAQ2.2: Western Asia, along with southern Australia, are singled out here as regions where heavy precipitation may have decreased. Only southern Australia is called out for decreasing heavy precipitation in the associated Figure (Figure FAQ2.2. Figure 2). Revision is required for consistency. [Government of Canada]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2362	2	60	38	60	38	leave out 'likewise'. [European Union]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2363	2	60	41	60	41	Delete "such as tropical cyclones," Reason: is unnecessary and only confusing in reading the sentence. [Urs Neu, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2364	2	60	43	60	43	Please indicate whether the "intensification" of the most extreme storms refers to an intensification in wind speed, or an intensification of rainfall rate, or both. (This relates to a problem I have also raised regarding some places where "intensification of storms" is used in the SPM without clarifying for the general reader what is meant by storm intensification). [David Wratt, New Zealand]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2365	2	60	43	60	44	This sentence could perhaps specifically refer to the observed pattern of some increase in the intensity of the strongest tropical cyclones in the North Atlantic. [Government of United Kingdom of Great Britain & Northern Ireland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2366	2	60	43	60	44	"There is some evidence....." Is this sentence referring to the increase in intense storms in the North Atlantic? If so, then it seems to be worded rather weaker than the corresponding conclusion of Section 2.6.3 which refers to 'robust evidence'. In addition, 'very short' is probably a bit unfair given the record extends since 1970. If this sentence is referring to a global intensification then it is unclear how this is supported by the conclusions of 2.6.3. [Thomas Stocker/ WGI TSU, Switzerland]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2367	2	60	47	60	47	This does not reflect the conclusions on Page 57, where all that is stated about landfalling tropical cyclones is a decrease in Eastern Australia. Figure 2.34 does indicate a negative trend also in the United States but the legend says "unadjusted" (does this refer to uncertainties in observing methods, as I assume?) and does not indicate if the trend is statistically significant (as is required for a result to be emphasized here in the FAQ). [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2368	2	60	48	60	48	I think the sentence that beings with "Little evidence …" should be rephrased slightly to explain why there is little evidence (trends may exist, but the limited available observations are not adequate to determine whether that is the case). [Francis Zwiers, Canada]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2369	2	60	51	60	51	intensity -> intensity of extratropical storms [Ileana Bladé, Spain]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2370	2	60	51	60	51	Is the increase in intensity described here just for wind speed, or also for rainfall rate in mid-latitude storms ? [David Wratt, New Zealand]	TAKEN INTO ACCOUNT: This FAQ has been rewritten for simplicity and clarity.
2-2371	2	60	53	61	2	FAQ 2.2 Figure 2 - Can this figure be improved? The map in the middle seems irrelevant really as all the information is above and below it. The 'except in these regions' could be overlaid on the map. Is there more to be added? [Kate Willett, United Kingdom]	Noted. This figure has been significantly revised.
2-2372	2	60	56	60	57	FAQ Figure 2 on the top line shows a white arrow pointing down indicating a decrease in tropical cyclone frequency. The confidence is low but there no evidence of a decrease. - suggest a horizontal arrow. [European Union]	Noted. This figure has been significantly revised.
2-2373	2	60	56			FAQ 2.2: Figure 2: We strongly believe that this figure has potential to be misinterpreted and introduces a level	Noted. This figure has been significantly revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						of simplification that could be problematic. In this current version we have already noted some scientific inconsistencies with the underlying chapter assessment. In addition, the main graphical components of the figure are largely redundant, and as such, we believe this information can be much more effectively and accurately presented in a Table. Please consider replacing this figure with a version of the TS/SPM table that is focussed on the observed changes but expanded to include some regional detail. Likelihood terms should include in ( ) the probability range, and the caption should explain why a probability is not provided for instances of low and medium confidence. Because the FAQ's need to be stand alone, it must be assumed that the readers have no knowledge of the uncertainty language used by the IPCC. [Thomas Stocker/ WGI TSU, Switzerland]	
2-2374	2	60	57	60	57	Figure 2 of Faq 2.2 seems to oversimplify the evidence and contradict the summarizing lines 4-7 in Page 61. The figure ascribes a sign to the change in the frequency and intensity of tropical cyclones everywhere except Australia (albeit with low confidence), which does not match the statements in section 2.6.3 (Page 57, lines 35-37). Changes in heavy precipitation, as discussed in Page 25, are also much less spatially uniform and more ambiguous than suggested by this figure, as acknowleged in Page 61, lines 5-6. [Ileana Bladé, Spain]	Noted. This figure has been significantly revised.
2-2375	2	60	57	61	2	Consideration should be given to rewording Figure 2 of FAQ 2.2. It currently states "These events are happening everywhere" and then talks about changes in frequency and intensity of tropical cyclones, which do not happen everywhere. [Adrian Simmons, United Kingdom]	Noted. This figure has been significantly revised.
2-2376	2	60	57			FAQ 2.2, Figure 2 is very misleading when it states "These events are happening everywhere" and it includes tropical cyclones and droughts. Saying trends 'are happening' and with low confidence are not compatible. [Stephen Gaalema, United States of America]	Noted. This figure has been significantly revised.
2-2377	2	61	5	61	6	FAQ2.2: The statement that precipitation extremes show "large spatial inconsistency' seems to contradict what is shown in Figure 2 of this FAQ where it is stated that heavy precipitation increases are seen everywhere except in southern Australia. [Government of Canada]	Noted. This figure has been significantly revised.
2-2378	2	61	6	61	6	I don"t think "imconsistency" is the right word here - inconsistentency with what ? I suggest instead: " … but there is large spatial VARIABILITY, and observed trends …" [David Wratt, New Zealand]	Accepted. Text amended accordingly.
2-2379	2	61	6	61	7	The sentence beginning with "There is little evidence …" should explain why there is little evidence - presumably because other other climate variables have not been studied as extensively, or data are not available. [Francis Zwiers, Canada]	Taken into account. We think this is clear from the preceding text.
2-2380	2	61	11	61	11	Suggest adding reference to Cai, W., Cowan, T. & Thatcher, M. Rainfall reductions over Southern Hemisphere semi-arid regions: the role of subtropical dry zone expansion. Sci. Rep. 2, 702; DOI:10.1038/srep00702 (2012) . [Government of Australia]	reference added
2-2381	2	61	11			Section 2.7: The trend calculation here is troublesome as there is no good reason for a linear trend. Even in the SH where there are noticeable trends, they are not linear and ozone depletion, a major cause was evident in the 1970s but has been reversed in recent times. The whole section is much in need of an assessment being performed. [Kevin Trenberth, United States of America]	Noted. - Linear trend calculation is justified in Box 2.2 (now 2.1). Notes of caution were added at several places (e.g., caption of Fig. 2.37, now 2.38)
2-2382	2	61	13	61	14	Cloud cover has been discussed in section 2.5.7 and si not discussed here. [Geert Jan van Oldenborgh, Netherlands]	Accepted. - Cloud cover is omitted.
2-2383	2	61	24			I do not agree with the sentence `The observational…AR4'. I think the basis of observations remains the same but probably, it is the use of new tools and techniques in various studies which are included in AR5. [Umesh Kulshrestha, India]	Accepted - Changed to "empirical basis" to make sure it is more than just the observations that have changed.
2-2384	2	61	27	61	27	delete the word 'start' from this sentence [Government of Australia]	Editorial
2-2385	2	61	27	61	27	Remove word “start” from sentence. [Andrew King, Australia]	Editorial
2-2386	2	61	43	61	43	Change 'onboard' to 'on board' [Peter Burt, United Kingdom]	Editorial
2-2387	2	61	45	61	45	HadSLP2t isas far as I know not the most widely used one, there are many other estimates of historical SLP, both observation al (UCAR SLP for the NH, E-OBS for Europe) and re-analyses, which are in principel very	Accepted - Sentence added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						suited for interpolating SLP taking the dynamics of the atmospherei into account and are in my experience much more widely used than HadSPLP2r (eg 20CR for long-term, NCEP/NCATR1 from 1948 (also used for the last part of HadSLP2r), and ERA-interim from 1979). [Geert Jan van Oldenborgh, Netherlands]	
2-2388	2	61	53	61	54	Thje 20CRand HadSLP2r estimate of SLP trends do not find incraese in the tropics in the annual mean (see attached file trends_slp.pdf). The discrepancies between the various datasets are huge, lessening confidence in these SLP trends. [Geert Jan van Oldenborgh, Netherlands]	Accepted - Sentence added.
2-2389	2	61	55	61	55	Seasonal trends -> Indeed, seasonal trends (to make the point of the sentence before this one clear). [Ileana Bladé, Spain]	Accepted - Previous sentence changed.
2-2390	2	61	55	61	55	"Seasonal trends" it is not mentioned which season is considered. [Geert Jan van Oldenborgh, Netherlands]	Accepted - Sentence added.
2-2391	2	61	56	61	56	Are these decreasing trends observed in all seasons? [Ileana Bladé, Spain]	Accepted - Sentence added.
2-2392	2	61	56	61	56	"Indian Ocean, and adjacent land regions" according to HadSLP2r, pressure inclreased over 1979 in East Africa and over India, which are adjacent land areas to teh Indian Ocean.They do decraese in the 20CR reanalysis. Unless an errors in any of these datasets is pointed out I think one should not mention these trends that disagree between datasets. (attachmenet: trends_slp_1979.pdf) [Geert Jan van Oldenborgh, Netherlands]	Accepted - Sentence is change (adjacent land regions is removed).
2-2393	2	61	56			The words "decreasing trends" should be avoided due to ambiguity. They could mean "trends for SLP to decrease" but could also mean that the trends of SLP (of whatever sign) are becoming smaller in magnitude. [Adrian Simmons, United Kingdom]	Accepted - Changed throughout the Section.
2-2394	2	61	57	61	57	There has been a strong decrease in the pressure over the Mediterranean in winter (especially JFM), which has led to an incfrease in westerly flow over continental Europe (van Haren et al, doi:10.1007/s00382-012-1401-5, Fig.14). This should be mentioned here. [Geert Jan van Oldenborgh, Netherlands]	Accepted - Reference added
2-2395	2	62	1	62	1	See Fig. 2.A.3 would be directly more helpful. [Ileana Bladé, Spain]	Accepted. Changed and Figure moved to main text.
2-2396	2	62	1	62	1	Figure FAQ 2.1: additional effects could be added to strengthen the argument, e.g from biology, such as "rise of timber lines", "elongation of vegetation growth period", "appearance of new species originally restricted to warmer regions". [Urs Neu, Switzerland]	Misappropriated comment. Rejected. None of these exist in regularized form for anything approximating global or hemispheric coverage necessary with multiple independent estimates to our knowledge.
2-2397	2	62	2	62	2	Change 'Sect.' to 'Section' [Peter Burt, United Kingdom]	Editorial
2-2398	2	62	2	62	2	2.7.5 not 2.7.4. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-2399	2	62	4	62	13	To what extent are these analyses influenced by the very different trends in 20CR from other SLP reconstrctions such as HadSLP2r? [Geert Jan van Oldenborgh, Netherlands]	Accepted. The analyses are robust (the figure in the ZOD had all data sets). Sentence is changed.
2-2400	2	62	5	62	7	These decadal fluctuations in strength are hard to see in Fig. 2.36. [Ileana Bladé, Spain]	Noted.
2-2401	2	62	10	62	10	Researches also found that, in recent decades, the Siberian high has intensified and northwestward expansed (e.g., Zhang et al. 2008; Zhang et al. 2012). [Xiangdong Zhang, United States of America]	Accepted - second reference added.
2-2402	2	62	10	62	10	Zhang, X., A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso, 2008: Recent radical shifts in atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi:10.1029/2008GL035607. [Xiangdong Zhang, United States of America]	Rejected. - Reference does not add substantially to those already cited
2-2403	2	62	10	62	10	Zhang, X., C. Lu, and Z. Guan, 2012: Weakened cyclones, intensified anticyclones, and recent extreme cold winter weather events in Eurasia. Environ. Res. Lett., accepted. [Xiangdong Zhang, United States of America]	Accepted - reference added.
2-2404	2	62	13	62	13	2.7.8 not 2.7.9. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-2405	2	62	25	62	27	Although the increase in anemometer heights is important, the change from visual to anemometer winds has a larger effect on marine wind speed trends [Elizabeth Kent, United Kingdom]	Accepted
2-2406	2	62	29	62	29	Thomas et al. (2008) reference repeated [Elizabeth Kent, United Kingdom]	Editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2407	2	62	33	62	41	One more reference for surface wind changes, Young et al. (2011; Science, 332, 451-455). [Chia Chou, Taiwan, ROC]	Not used because of a shorter data set period than is used in this section. Review by Bourassa et al., 2010 discusses different types of satellite-based marine surface wind data sets, including altimeter-based ones.
2-2408	2	62	36	62	39	This sentence is technically complex and the explanation given is not clear to me (the confusing part begins with "used either"). [Ileana Bladé, Spain]	Editorial, re-worded.
2-2409	2	62	39	62	39	CCMP does use reanalysis wind as a background field, however OAFlux uses 3 different renalysis as input data sources. Indeed, prior to 1985 OAFlux winds are based on reanalyses alone. [Elizabeth Kent, United Kingdom]	Rejected: these details do not contradict the sentence as written.
2-2410	2	62	43			Section 2.7.2: In referencing Figure 2.37, the authors are encouraged to discuss the apparent increase in the trade winds in the central southern Pacific. This is missing from the text, despite most of the panels in Fig. 2.37 showing this increasing trend across this sub-region of the Pacific. [Government of United States of America]	Taken into account: discussion of Southern Ocean winds and a reference to the Chapter 3 are added.
2-2411	2	62	45	62	45	Provide reference Kent et al. 2012 in bibliography. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2412	2	62	45	62	45	I can't find the Kent et al. 2012 reference unless you mean the one about HadNMAT2 marine air temperature? [Kate Willett, United Kingdom]	Accepted:corrected
2-2413	2	62	47	62	47	Perhaps you can elaborate a little more on this North Atlantic pattern ? [Ileana Bladé, Spain]	Rejected: Reference is present to Sec 2.7.6.2.
2-2414	2	62	48	62	49	To avoid confusion, it should be mentioned here that these are short-term trends associated with the considerably decadal v raibility in these areas and not long-term trends. [Geert Jan van Oldenborgh, Netherlands]	Accepted: Text added.
2-2415	2	62	51	62	52	Is it the absolute quality of the wind measurement over land that is in question, or is it more a question of whether the wind measurements made at synoptic stations in a particular region are sufficiently representative of the region as a whole? [Adrian Simmons, United Kingdom]	Accepted. Sentence changed and more details on possible problems are given in the following sentences.
2-2416	2	62	51	63	18	A recent paper that should probably be assessed here is McVicar T.R., M.L. Roderick , R.J. Donohue, L.T. Li, T.G. Van Niel, A. Thomas, J. Grieser, D. Jhajharia, Y. Himri, N.M. Mahowald, A.V. Mescherskaya, A.C. Kruger, S. Rehman and Y. Dinpashoh, 2012, Global review and synthesis of trends in observed terrestrial near-surface wind speeds: implications for evaporation, Journal of Hydrology, 416/417, 182–205. [J. Graham Cogley, Canada]	Accepted. - Reference added
2-2417	2	62	56	62	56	it seems that there is a consistent picture of increasing winds in the southern ocean which is not mentioned. [European Union]	Accepted: text added.
2-2418	2	62	56	62	65	The following two paragraphs suggest a higher than low confidence that wind speeds have decreases over land - discuss. [European Union]	Taken into account: We now make the main cause, the lack of important meta information in global data sets, more explicit.
2-2419	2	63	1	63	1	"weakening of seasonal and annual winds" (mean ?) unclear [Government of France]	Accepted. - Sentence changed.
2-2420	2	63	1	63	8	Recent publication of Earl et al. 2012 (J. Clim) showed decreases in wind speed over the UK too. [Kate Willett, United Kingdom]	Accepted. - Reference added
2-2421	2	63	6	63	7	The Canadian Arctic is a big area, is it possible to qualify what parts of the Canadian Arctic, even on a regional basis, have experienced increased wind speeds? [Government of Canada]	Taken into account: Reference to McMivcar et al. (2012) is added, who reviewed 148 observational studies and provide the requested regional detail.
2-2422	2	63	12	63	12	Perhaps mention that Vautard's result is consistent with trends from reanalysis (Fig. 2.37). [Ileana Bladé, Spain]	Rejected - the trend period is different.
2-2423	2	63	13	63	13	Again, I thought this section was steering clean of attribution statements, but if you choose to include one	Accepted. - Sentence omitted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						please make sure it does not sound so speculative. [Ileana Bladé, Spain]	
2-2424	2	63	15	63	15	change to 'to underestimate systematically' to remove split infinitive [Peter Burt, United Kingdom]	Editorial
2-2425	2	63	20	63	27	The figure 2.37 is poor and includes some products with no credibility. Thes sensitivity to the number of missing months and years should be assessed. Using annual values fails to allow for seasonality. [Kevin Trenberth, United States of America]	Editorial
2-2426	2	63	29	63	29	ananometer [Elizabeth Kent, United Kingdom]	Editorial
2-2427	2	63	31	63	32	Style change for citing degrees [Peter Burt, United Kingdom]	Editorial
2-2428	2	63	39	63	59	2.7.3 Upper-Air Winds: Zhang et al. (Zhang, A.Y., G.Y. Ren, J. Guo and Y. Wang. 2009, Change trend analyses on upper air wind speed over China in past 30 years. Plateau Meteorology, 28 (3): 680-687) analyzed the changes in upper air winds for mainland China based on a dataset of monthly mean wind speed from 1980 to 2006 at 119 radiosonde stations, and they found that, in mid- to lower troposphere (850-400hPa) and upper troposphere (300-150hPa), annual mean wind speed decreased, with change rates of -0.10 m/s per decade and -0.17 m/s per decade respectively (insignificant at 0.05 confidence level). They also found that lower stratospheric (100-50hPa) wind speed increased with a rate of 0.24 m/s per decade (insignificant at 0.05 confidence level). Contrast to the upper atmosphere, surface wind speed significantly decreased with a rate of -0.16 m/s per decade (significant at 0.05 confidence level). They suggested that the large decrease of surface wind speed widely reported is not only associated with the large-scale circulation change, but also with the urbanization-induced change in observational settings around the meteorological stations. [Guoyu Ren, China]	Accepted - Reference added
2-2429	2	63	41	63	59	2.7.3 Can you quantify what is meant by upper-air winds? Is this anything from 10m upwards or above the boundary layer or other? I think this might be important for people reading this interested in wind turbines/renewable energy policy. [Kate Willett, United Kingdom]	Accepted - we add a note on the definition (winds above the planetary boundary layer)
2-2430	2	63	43			why would trends in radionde winds be less common? This relates to causes of lost balloons and missing data. It relates to documented problems with using small balloons, which burst prematurely, balloons that burst in cold conditions, balloons that are carried out of sight of a radar by strong winds (creating a light wind bias), and changing tracking devices, especially radar vs gps. [Kevin Trenberth, United States of America]	Rejected. - This results is the outcome of a study using objective homogenisation algorithms.
2-2431	2	63	46			It could be pointed out here that reanalyses also assimiilate wind information from profilers and aircraft, and also infer incremental wind information from thermal observations via balance relationships applied to differences between background fields and observations. [Adrian Simmons, United Kingdom]	Accepted. - Information is added.
2-2432	2	63	50	63	57	Aren´t rawinsonde wind observations and balloon-based observations the same? Then I don´t understand why line 50 says "no (decreasing) or much weaker trends were found for lower tropospheric winds from balloon data" whereas lines 55-56 say "Vautard et al. (2010) find increasing wind speed in rawinsonde observations in the lower and middle troposphere from 1979-2008 over Europe and North America". [Ileana Bladé, Spain]	Accepted. - Sentence on l. 50 is omitted. Yes, they are the same and we now use only "radiosonde".
2-2433	2	63	51	63	51	What kind of data is the Allen and Sherwood result based on? [Ileana Bladé, Spain]	Accepted. Is is based on radiosonde data; the sentence is changed
2-2434	2	63	51	63	54	Allen and Sherwood (2008) were mainly focused on deducing temperature trends from the thermal wind relationship, not on assessing wind trends, so I'm not sure it's fair to include these results. Also, it's important to specify the direction of the wind in addition to the sign of the trend. So are the negative trends in the upper troposphere a decrease in easterly wind? [Dian Seidel, United States of America]	Taken into account: Sentence now reflects the original goal of Allen and Sherwood (2008)
2-2435	2	63	53	63	53	After line 53, add “Rao et al (2012) also identify, analyzing the NCEP/NCAR reanalysis, find a weakening trend in the tropical easterly jet over India during the summer monsoon for the period 1958-1998". [Government of India]	Rejected - NCEP/NCAR not suited for this analysis.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2436	2	63	54	63	55	Wouldn't it be more useful to say that the trends in wind shear are consistent with trends in temperature (as opposed to this vague statement about implications) and doesn´t this apply to the extratropics as well? [Ileana Bladé, Spain]	Taken into account: Sentence omitted.
2-2437	2	64	8	64	10	There is a big contradiction between this result and conclusions from AR4 which deserves some comment/explanation. [Ileana Bladé, Spain]	Accepted - Explanation added (the main cause is the time period analysed).
2-2438	2	64	8	64	10	Provide a reference e.g. State of the Climate ijn Bull. Amer. Met. Soc. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected. - The sentence refers to the cited figure (which is now moved to the main text); no reference is therefore required.
2-2439	2	64	19	64	20	It could be pointed out that this remark applies to trends from reanalyses as well as those from radiosonde data [Adrian Simmons, United Kingdom]	Accepted - Sentence changed and a reference is added.
2-2440	2	64	20	64	20	Confidence' and 'low' should be in italics [Peter Burt, United Kingdom]	Editorial
2-2441	2	64	22			This section should do an assessment and not treat all reanalyses as equally credible. [Kevin Trenberth, United States of America]	Accepted - Some reanalyses (ERA-40 and NCEP2) are now excluded from two of the figures.
2-2442	2	64	26	64	26	Additionally, the tropical circulation -> Additionally, changes in the tropical circulation [Ileana Bladé, Spain]	Editorial
2-2443	2	64	27	64	28	Can you elaborate a bit? This background information is so succint it is hardly useful. [Ileana Bladé, Spain]	Rejected - space is too tight to elaborate, but the two references are useful.
2-2444	2	64	27			This is not correct. It applies if one ignores the intermittent changes in precipitation and the changes in frequency and intensity. It applies only if one ignores transients and deals with monthly means. [Kevin Trenberth, United States of America]	Taken into account - sentence changed to make clear that it is the average state of the circulation only that is constrained.
2-2445	2	64	37	64	37	monsoon circulation -> Asian monsoon circulation [Ileana Bladé, Spain]	Editorial
2-2446	2	64	38	64	38	Citation of Tianjun et al. 2008 should be Zhou, T., L. Zhang, and H. Li (2008), Changes in global land monsoon area and total rainfall accumulation over the last half century, Geophys. Res. Lett., 35, L16707, doi:10.1029/2008GL034881. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - problem with Endnote software
2-2447	2	64	41	64	42	Xu et al citation should be 2006b [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-2448	2	64	42	64	45	Can you tie these circulation changes to the weakening of the monsoon a little more explicitly? This part of the paragraph feels disconnected. [Ileana Bladé, Spain]	Accepted - sentence rephreased.
2-2449	2	64	44	64	44	The western Pacific subtropical high is a middle tropospheric feature (it is clearly not present in Fig. 2.36), but the transitional words "In the upper level" (when referring to the South Asian high) seems to imply that it is a surface feature. [Ileana Bladé, Spain]	Accepted - sentence rephreased.
2-2450	2	64	44	64	44	Delete "Hong and Ho, 2002" which does not address the zonal expansion of the south Asian high. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - reference deleted.
2-2451	2	64	46	64	46	After Zhou et al. (2009a), add “Krishnan et al. (2012) noted a weakening of the South Asian summer monsoon circulation during the last few decades as evidenced from two sets of reanalysis data and from projections of a high resolution model”. [Government of India]	Rejected - reanalysis data not considered suitable for this purpose.
2-2452	2	64	46	64	47	Another cautionary note could be that the precipitation datasets do not in fact show a decrease in precipitation in the EASM region (see Fig.2.29 for annual mean trends from 1901, but the same holds for summer half year trends since 1950 in the GPCC v6 and CRU TS 3.10.01 datasets). [Geert Jan van Oldenborgh, Netherlands]	Accepted - A sentence on this is added.
2-2453	2	64	51	64	52	The strengths … are -> The strength … is [Ileana Bladé, Spain]	Editorial
2-2454	2	64	54	64	57	True, but the NNR and ERA-40 analyses are not from the latest generation of reanalyses. They were discussed in AR4. For AR5 it should be pointed out that ERA-Interim improves on ERA-40 in several ways in the representation of the Tropics. Dee et al.(2011) is one reference. In relation to what is stated in the paragraph, there can be hardly any doubt that increases in Hadley circulation since the 1970s seen in ERA-40	Accepted - Sentence is rephrased to make explicit that later generation reanalyses such as ERA-Interim also show a strengthening.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						are artificial, but for the period since the 1970s, ERA-Interim (available from 1979 onwards) should be used, not ERA-40. ERA-Interim is certainly not perfect, but it is much better than ERA-40. [Adrian Simmons, United Kingdom]	
2-2455	2	64	55	64	57	The increase in Hadley Circulation would be entirely consistent with ENSO conditions since 1977 averaging on the El Nino side of absolutely neutral (I.e. SOI = 0), which is easily demonstrated by averaging the monthly average SOI. I see that this is eventually mentioned on page 70, lines 3 to 6. [John McLean, Australia]	Noted.
2-2456	2	64	57			Wittenberg (2004, J. Climate, 17, 2526-2540) was among the first to demonstrate the tropical wind stress shortcomings of the NNR. [Andrew Wittenberg, United States of America]	Rejected. The text has sufficient and specific references.
2-2457	2	65	6	65	18	Fig 2.38 contains ERA-40 and NCEP2 which have no credibility. [Kevin Trenberth, United States of America]	Accepted. - Both are now exlcuded.
2-2458	2	65	7	65	18	This is a poor figure. It shows different factors (Hadley and Walker circulations) over different months. It should either show the same months in its top and bottom parts or be split into two separate figures. [John McLean, Australia]	Rejected - the two figures are thematically closely related and show the same data sets over the same timeperiods.
2-2459	2	65	15	65	16	Please do NOT use all reanalyses when many are known to have spurious variations and no credibility [Kevin Trenberth, United States of America]	Accepted. ERA-40 and NCEP2 are now excluded
2-2460	2	65	20	65	36	One more reference for changes in the Pacific Walker circulation, Luo et al. (2012; PNAS, doi:10.1073/pnas.1210239). [Chia Chou, Taiwan, ROC]	Accepted. - Reference is added.
2-2461	2	65	20	65	36	A recent study (Tokinaga et al. 2012b) in Nature shows that the observed Walker circulation slowdown is largely forced by the changes in zonal SST gadient across the Indo-Pacific Oceans, instead of due to the muted hydrological cycle response that can take place even with spatial-uniform SST increase. This study also resolved the confusing issue of the observed SST warming pattern (VEcchi and Soden 2007), suggesting that the Indo-western Pacific warming is less than that in the eastern tropical Pacific. Reference: Vecchi, G. A., and B. J. Soden, 2007: Global warming and the weakening of the tropical circulation. J. Climate, 20, 4316–4340. Tokinaga, H., S.-P. Xie, C. Deser, Y. Kosaka and Y. M. Okumura, 2012: Slowdown of the Walker circulation driven by tropical Indo-Pacific warming. Nature, 491, 439-443 [Nathaniel Johnson, United States of America]	Rejected - no attribution is done.
2-2462	2	65	20	65	36	The decrease in Walker Circulation is consistent with ENSO conditions since 1977 averaging on the El Nino side of absolutely neutral (ie. SOI = 0), which is easily demonstrated by averaging the monthly SOI. Under these conditions Hadley Circulation increases and Walker Circulation decreases. [John McLean, Australia]	Noted. - This is discussed in Sec. 2.7.8 and many corresponding figures/tables, which are referenced.
2-2463	2	65	23	65	24	If "Boreal spring and summer contribute most strongly to the centennial trend" in the Walker circulation, why is Figure 2.38b based on indices from September to January (winter)? [Ileana Bladé, Spain]	Accepted. - Sentence changed (Sep.-Jan is the season when the Walker circulation is strongest).
2-2464	2	65	26	65	26	Figure 2.38 is meant (also in line 34) [Ileana Bladé, Spain]	Editorial
2-2465	2	65	26	65	35	Several references to Figure 2.39 should be to Figure 2.38, and a reference to Table 2.8 should be to Table 2.12. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-2466	2	65	28	65	29	Again discrepancies between the NNR and ERA-40 are mentioned. Please see the immediately preceding comment. [Adrian Simmons, United Kingdom]	Accepted. - Sentence changed.
2-2467	2	65	29	65	31	This seems backwards. I think what you mean is that Deser et al. find their indirect observations of the Walker circulation (temperature and cloud cover) to be consistent with a weakening. [Ileana Bladé, Spain]	Accepted. - Sentence changed.
2-2468	2	65	31	65	32	Tokinaga et al. (2012) find robust evidence for a weakening of the Walker circulation "from 1950 to 2008" based on a series of observations. [Hiroki Tokinaga, United States of America]	Accepted. - Sentence changed.
2-2469	2	65	32	65	32	Tokinaga et al. (2012)'s analysis begins in 1950. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. - Sentence changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2470	2	65	34	65	34	Could add that recent strengthening of the Walker circulation is also consistent with the tropical Pacific surface temperature trend patterns from 1979-2011 shown in Fig 2.22 [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted. - Sentence is added (including a reference to the Fig. 2.22)
2-2471	2	65	35	65	35	Table 2.8 does not show SOI trends. I think you mean 2.12. [Ileana Bladé, Spain]	Changed
2-2472	2	65	36	65	36	Append “Notwithstanding this, the Walker circulation in the tropical pacific was uniquely disrupted, with upward motion everywhere, in the boreal summer of 2009 (Ashok et al., 2012). Further, occurrence of anomalous twin Walker cells over the tropical Pacific is seen in the recent two decades associated with the occurrence of the ENSO Modoki events (Ashok et al., 2007a, Weng et al., 2007)". [Government of India]	Rejected - Does not contribute sufficient new material on changes in Walker circulation.
2-2473	2	65	38	66	12	Recommend updating your reference list and estimates of tropical expansion to include the recent paper by Allen et al (2012), titled "Recent Northern Hemisphere tropical expansion primarily driven by black carbon and tropospheric ozone." [William Landuyt, United States of America]	Rejected - no attribution is done in this chapter
2-2474	2	65	48	65	48	Tracked down Hudson (2011) which should be Hudson (2012) at Atmos. Chem. Phys., 12, 7797–7808, 2012 doi:10.5194/acp-12-7797-2012 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-2475	2	65	56	65	56	Add Davis and Rosenlof (2011) to citations. [Dian Seidel, United States of America]	Accepted.
2-2476	2	65				Figure 2.39: While I entirely agree that the tropics are widening, I find it difficult to draw that conclusion from this figure. Please modify the caption to make this figure more demonstrative of this effect. [Jeffrey Taylor, United States of America]	Accepted - Figure changed
2-2477	2	66	1	66	3	The green curves in Fig. 2.39, corresponding to jet-stream based metrics, seem completely flat (no trend) but the text says this metric indicates widening? [Ileana Bladé, Spain]	Accepted - The figure is redrawn
2-2478	2	66	3	66	7	These sentences seem contradictory. The first implies widening is disputed, the second, not (but both cite some of the same references) [Peter Burt, United Kingdom]	Rejected. - There is no contradiction. The methods (indices used) are disputed, but the same authors find significant widening in other measures.
2-2479	2	66	5	66	6	The statement "subtropical high-pressure regions also indicate widening" seems in contradiction with an earlier statement on Page 62, line 5 that "The position and strength of semi-permanent pressure centres show no clear evidence for trends since 1951". [Ileana Bladé, Spain]	Taken into account - Sentence is changed to make clear that the observed change in subtropical highs is small.
2-2480	2	66	9	66	12	These are consistent with the change in the ENSO. [John McLean, Australia]	Noted.
2-2481	2	66	18	66	18	and zonally [Ileana Bladé, Spain]	Editorial
2-2482	2	66	30	66	30	After ENSO, add” the IOD, ENSO Modoki, and the southern annular mode”. [Government of India]	Accepted - Other modes of variability are added.
2-2483	2	66	30	66	30	In relation to the above suggestion, cite (after with strong and Davis 2008b) “Nakamura and Sampe 2004; Ashok et al 2007b, 2009”. [Government of India]	Rejected. - Existing references are sufficient.
2-2484	2	66	32	66	32	Various analyses -> Various new analyses; indicate -> confirm [Ileana Bladé, Spain]	Editorial
2-2485	2	66	32			Please do NOT use all reanalyses when many are known to have spurious variations and no credibility [Kevin Trenberth, United States of America]	Accepted. - ERA40 and NCEP2 are excluded.
2-2486	2	66	34	66	34	Archer and Caldiera reference should be 2008a and the Fu et al. refference should be Fu and Lin 2011. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
2-2487	2	66	34	66	36	Inconsistency may also arise from the choice of analysis period especially through inhomogenity of data input to reanalyses (Archer and Caldiera 2008b) [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. - Sentence changed.
2-2488	2	66	37	66	37	Delete comma after 'Atlantic' [Peter Burt, United Kingdom]	Editorial
2-2489	2	66	37	66	37	Replace "Canada" by "the western and central Pacific, eastern Canada" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. - Sentence changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2490	2	66	38	66	38	trends that are coupled with -> concomitant with [Ileana Bladé, Spain]	Editorial
2-2491	2	66	43	67	19	Shouldn't this section on "Frequency of Cyclones" be consolidated with the earlier section 2.6.4. on "Extratropical storms" ? [Ileana Bladé, Spain]	Accepted. - Sections are better consolidated, 2.6.4 is expanded while this section is condensed.
2-2492	2	66	43			Section 2.7.6.2 should be merged with 2.6.4, which covers the sme material. [Geert Jan van Oldenborgh, Netherlands]	Accepted. - Sections are better consolidated, 2.6.4 is expanded while this section is condensed.
2-2493	2	67	1	67	2	Regarding the poleward shift of storm track and intensification of storm activity in northern high latitudes, the following two papers should be cited: [Xiangdong Zhang, United States of America]	Rejected - rather old references
2-2494	2	67	1	67	2	McCabe, G. J., M. P. Clark, and M. Serreze, 2001: Trends in Northern Hemisphere surface cyclone frequency and intensity. J. Climate, 14, 2763–2768. [Xiangdong Zhang, United States of America]	Rejected - rather old references
2-2495	2	67	1	67	2	Zhang, X., J. E. Walsh, J. Zhang, U. S. Bhatt, and M. Ikeda, 2004: Climatology and interannual variability of Arctic cyclone activity, 1948-2002. J. Climate, 17, 2300-2317. [Xiangdong Zhang, United States of America]	Rejected - rather old references
2-2496	2	67	5	67	5	Add a comment along the lines "In contrast to studies suggesting a weakening of the Walker Circulation, Meng et al. (2012) find evidence for a strengthening of the Walker Circulation in the 20th century that is consistent with an enhancement of the SST gradient across the Equatorial Pacific." Citation: Meng, Q., Latif, M., Park, W., Keenlyside, N.S., Semenov, V.A., Martin, T. (2012): Twentieth Century Walker Circulation Change: Data Analysis and Model Experiments, Climate Dynamics 38, 1757-1773, doi: 10.1007/s00382-011-1047-8. [Douglas Maraun, Germany]	Accepted - Reference is added (p. 65), but comment is off-placed and refers to p. 65
2-2497	2	67	6	67	6	"…equatorward shift…" Considering the period and the pronounced decadal variability this is a brave statement hardly supported by the studies cited in the same sentence. Please adjust the statement accordingly. [Ralf Weisse, Germany]	Accepted. - Sentence deleted.
2-2498	2	67	12	67	12	Change "frequent" to "pronounced". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial
2-2499	2	67	15	67	19	The time period for the Australian study is shown as 1865 to 2009 but that's far too long to be of any value. Describe what's happened in the last 15 or 30 years. [John McLean, Australia]	Accepted - Sentence is omitted.
2-2500	2	67	19			Apologies for being repetitive, but Trenberth et al. (2005) and Wang et al.(2006b) are rather old references, and their conclusions do not necessarily apply to the latest generation of reanalyses. [Adrian Simmons, United Kingdom]	Accepted - Sentence is omitted.
2-2501	2	67	21	67	41	This is not an assessment but a list of papers. Can we conclude that in all the regions not mentioned (eg Europe, North America) blocking has not changed? Are all studies mentioned uncontroversial? [Geert Jan van Oldenborgh, Netherlands]	Accepted. - No we cannot conclude that. The sentence is changed.
2-2502	2	67	21			Section 2.6.7.3 This section is missing anything on the SH: please check with Jim Renwick (LA in Chap 14).This is in section 14.6.3 not 14.2.11 which does not exist. [Kevin Trenberth, United States of America]	Noted. There was one statement and reference on SH blocking (Dong et al. 2008); the same that was in Sect. 14.6.3. The sentence is slightly rephrased.
2-2503	2	67	23	67	26	The following study corroborates the Dole (2011) study of the Russian Heatwave of 2010 being caused by blocking. (Our goal was to examine the dynamics involved, but in doing so, we corroborated much of Dole's findings) Ref: Lupo, A.R., I.I. Mokhov, M.G. Akperov, A.V. Chernokulsky, and A. Hussain, 2012: A dynamic analysis of the role of the planetary and synoptic scale in the summer of 2010 blocking episodes over the European part of Russia. Advances in Meteteorology, Vol. 2012 (2012), Article ID 584257, 11 pages [Anthony Lupo, United States of America]	Rejected - The reference does not give substantial additional information.
2-2504	2	67	23			Sec 2.7.6.3 : It is worthwhile to point out that blocking frequencies (and hence trends) depend quite sensitively on the definition of the blocking index. No generally accepted definition exists. Most important differences occur between anomaly-based blocking indices and full-field indices such as the Tibaldi-Molteni index. The latter is inherently influenced by any change in the mean climatology of the relevant field (eg z500), and thus not necessarily by the variability. For example, studies are now appearing (de Vries et al, CliDy under review) that explain the negative trends mostly in terms of the change of the mean field (climatology). Another point to	Accepted. - Sentence changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						be made, is that blocking exhibits strong interannual variability in all seasons. [Hylke de Vries, Netherlands]	
2-2505	2	67	33	67	33	"drought-conducive" -> could this be better written as "dry" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. - Changed.
2-2506	2	67	37	67	37	PNA could probably do with being written out in full here as it has not been explicitly defined in the text yet. [Kate Willett, United Kingdom]	Noted but sentence is omitted.
2-2507	2	67	38	67	38	Mokhov et al. (2012) find that since about 1990 there has been an increase in blocking frequency and days and a decrease in their intensity over the entire Northern Hemisphere. Refs: Mokhov, I.I., M.G. Akperov, M.A. Prokofyeva, A.V. Timazhev, A.R. Lupo, H. Le Treut, 2012: Blockings in the Northern Hemisphere and Euro-Atlantic region: Estimates of changes from reanalyses data and model simulations. Doklady, in press. Mokhov, I.I., A.R. Lupo, and M.A. Prokofyeva, 2012: Blockings Activity in the Northern Hemisphere: Tendencies of Change for Last Decades. Research Activity in Atmospheric and Oceanic Modeling World Meteorological Organization / Technical Document - No. 1064: 2.20 - 2.21 [Anthony Lupo, United States of America]	Accepted - One of the references is added.
2-2508	2	67	38	67	38	Change 2.7.9. to 2.7.8 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted but sentence is omitted.
2-2509	2	67	38	67	40	This sentence is not clear as there are two "betweens". [Ileana Bladé, Spain]	Accepted - Sentence changed.
2-2510	2	67	41	67	41	decrease in number but increase in intensity of blocking days ->decrease in the number of blocking days but an increase in intensity (of blocking) [Ileana Bladé, Spain]	Accepted - Sentence changed.
2-2511	2	67	41	67	41	What does "an increase in intensity of blocking days" mean? Are the days more intense? Is the pressure differential greater? Clarification required. [John McLean, Australia]	Accepted - Sentence changed.
2-2512	2	67	45	67	51	Shouldn't the main driver (BDC) be listed first ? [Dale Hurst, United States of America]	Rejected - we proceed from zonal to meridional circulation.
2-2513	2	67	45			reword: please include energy fluxes other than radiative. [Kevin Trenberth, United States of America]	Noted but sentence is omitted.
2-2514	2	67	47	67	48	The statement that a sudded strat. Warming always leads to a collapse of the polar vortex is not correct. There are minor and major warmings that disturb the polar vortex but that do not lead to a destruction (or collapse) of the vortex. This is only the case for the so-called final warming. [Rolf Müller, Germany]	Accepted - Sentence changed.
2-2515	2	67	51	67	51	add 'polar and' in front of 'subpolar' [Rolf Müller, Germany]	Accepted - Sentence changed.
2-2516	2	67	52	67	52	Chapter 10 doesn't make much mention of stratospheric circulation. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - Reference omitted.
2-2517	2	67	55	67	55	have been -> were [Ileana Bladé, Spain]	Editorial
2-2518	2	67	55	67	55	suggest commenting on cause and significance of polar vortex changes [Rolf Müller, Germany]	Rejected. Space is limied and focus is on observed changes not causes.
2-2519	2	68	2	68	2	unclear what is meant by a 'deeper' vortex. Stronger diabatic descent? Provide a reference for the statement. [Rolf Müller, Germany]	Accepted - sentence rephrased.
2-2520	2	68	4	68	4	"uncertainties in the data products are high" - "data products" is vague. Which "data products" ? [Dale Hurst, United States of America]	Accepted - sentence rephrased.
2-2521	2	68	13	68	13	Where was this decrease observed? [Ileana Bladé, Spain]	Accepted - sentence rephrased (globally).
2-2522	2	68	16	68	16	"over the Northern Hemisphere midlatitudes" [Dale Hurst, United States of America]	Accepted - sentence changed.
2-2523	2	68	16	68	17	"from measurements of age tracers CO2 and SF6" [Dale Hurst, United States of America]	Rejected - existing phrasing is sufficient
2-2524	2	68	18			Note: the one idea that is missing here is the possibility of trends in mid to low latitude mixing that may impact	Accepted - sentence changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						trends in species and age of air (which was a conclusion in the Ray et al. 2010 paper cited here) [Karen Rosenlof, United States of America]	
2-2525	2	68	26	2	26	I think it would have been good to define modes in one place only, instead much of this information has been duplicated in other chapters, and especially Chapter 14, where slightly different definitions and conventions have been used. That said, I believe that the "mode" approach is very useful for condensing information about the nature of climate variability [George Kiladis, United States of America]	Noted. Scopes of both Ch 2 and 14 require to use modes; it turned out impractical to have them defined in one chapter but to use them in both.
2-2526	2	68	29	68	30	I suggest: "e.g. by adding climate anomalies calculated from meteorological records at stations exhibiting the strongest correlation with the mode and subtracting anomalies at stations exhibiting anticorrelation with the mode" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial, revised accordingly
2-2527	2	68	33	68	33	After PCA add: "and cluster analysis" as this is frequent and influential too [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected: cluster analysis has not been used for defining any of climate patterns listed in this Box.
2-2528	2	68	33	68	33	I'm not supposed to edit for typos but "principle" should be "principal" and this in my experience may not get picked up! [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted: corrected.
2-2529	2	68	38	68	38	Table 2.12 is meant [Ileana Bladé, Spain]	Rejected: correct as written.
2-2530	2	69	16	69	16	PSA could probably do with being written out in full here as it has not been explicitly defined in the text yet. [Kate Willett, United Kingdom]	Rejected: defined in Box 2.5 Table 1
2-2531	2	69	18	69	18	The PDO is SST-based so this part of the caption of Box 2.5 Figure 2 is wrong. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted: corrected.
2-2532	2	69	25	69	25	by many -> by a variety of [Ileana Bladé, Spain]	Editorial: unclear if the suggestion improves the text.
2-2533	2	69	32			How do these trends for indices of climate variability account for phase shifts in the large-scale patterns that show multidecadal variability (e.g. PDO, AMO, IPO, NAM)? [David Sauchyn, Canada]	Taken into account: 1st paragraph has been revised to refer to this issue.
2-2534	2	69	36	69	36	are subject to -> exhibit [Ileana Bladé, Spain]	Editorial: unclear if the suggestion improves the text.
2-2535	2	69	44	69	55	The NAO change may mainly show a pattern shift, which cann't be identified using EOF/REOF teachique or indices representing the differences between two fixed grid points or stations in the high and low latitudes, respectively. For exampe, it has been shown that both in observations and global warming scenario simulations that NAO active centers shifted or will shift northward or northeastward, particularlly for the center in the north (Ulbrich and Christoph 1999; Hilmer and Jung 2000; Hu and Wu 2002; Dong et al. 2011). Hilmer, M. and Jung, T. 2000. Evidence for a recent change in the link between the North Atlantic Oscillation and Arctic sea ice export. Geophys. Res. Lett. 27, 989–992. Ulbrich, U. and Christoph, M. 1999. A shift of the NAO and increasing storm track activity over Europe due to anthropogenic greenhouse gas forcing. Climate Dyn. 15, 551–559. Hu, Z.-Z. and Z. Wu, 2004: The intensification and shift of the annual North Atlantic Oscillation in a global warming scenario simulation. Tellus, 56A (2), 112-124.473. DOI: 10.1038/nature10013. Dong, B.-W., R. T. Sutton, and T. Woollings, 2011: Changes of interannual NAO variability in response to greenhouse gases forcing. Clim. Dyn., 37, 1621-1641. [Zeng-Zhen Hu, United States of America]	Rejected; the discussion is based on traditional definition of NAO indices listed in Table 1. More subtle changes in the NAO pattern (and other climate patterns) could not be fit here.
2-2536	2	69	44	69	55	This paragraph is all over the place. It refers to the Pacific and Atlantic, then goes into details about the NAO and SAM. I suggest a paragraph break after sentence 1. [John McLean, Australia]	Editorial: the pargraph has been revised.
2-2537	2	69	47	69	47	Are potential links between sea ice changes and recent NAO/AO behaviour dealt with elsewhere? If so a cross-reference would be useful here. [Government of Australia]	Rejected: outside of scope of this section.
2-2538	2	69	49	69	50	Do you mean the CPC NAO index? That is based on Z500, and therefore shows a positive trend due to thev thermodynamical expansion of teh atmosphere (the positive pole is larger than the negative one). All indices in this section are wisely based on SLP, so this clause ("with the exception of ...present") can be deleted. [Geert Jan van Oldenborgh, Netherlands]	Rejected: PC-based NAO index (as defined in Box 2.5 Table 1) is based on SLP.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2539	2	69	50	69	50	NAM could probably do with being written out in full here as it has not been explicitly defined in the text yet. In fact there are other modes that are only written out explicitly in the table. Can these all be written out in the text too? [Kate Willett, United Kingdom]	Editorial. Generally, space limitations often make it necessary to define abbreviations in the appropriate tables rather than in the text.
2-2540	2	69	51	69	51	Table 2.12 presents trends for the AAO not the SAM. I thought these were equivalent terms but further down you quote trends in the AAO. [Ileana Bladé, Spain]	Editorial: clarified.
2-2541	2	69	52	59	52	"peaking in austral winter 2010" There is no signifiucant trend in austral winter SAM, so the implied causality between the trends in summer and autumn and the peak in winter 2010 is not valid. [Geert Jan van Oldenborgh, Netherlands]	Accepted: revised accordingly.
2-2542	2	69	53	69	53	A period is needed before Visbeck [Ileana Bladé, Spain]	Rejected: period is given at the end of this sentence.
2-2543	2	69	54	69	54	Suggest to introduce the AAO index. It seems to be the most commonly used index and it might thus be useful to provide some information about it here [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: reference to Box 2.5 Table 1, where all indices are defined, has been added.
2-2544	2	69	55	69	55	The PC-based AAO index can only be computed since 1950 so there is no 110-year trend. [Ileana Bladé, Spain]	Accepted: correction made.
2-2545	2	69	58	69	59	The trend over 32 years in a multi-decdaal index is not significant when the full autocorrelation is taken into account, which I guess has not been done here. [Geert Jan van Oldenborgh, Netherlands]	Rejected: significance has been computed taking into account AR(1)-type correlations, as with all other indices; details are given in Box 2.2
2-2546	2	69		69		Table 2.12 No SAM in this table? [Kate Willett, United Kingdom]	Taken into account: SAM and AAO connection clarified
2-2547	2	70	7	70	7	Not clear what "has changed rapidly in the last 20 years" means. If you are referring to a negative trend, this trend is only 5% significant in one of three datasets. At any rate the correct period is 30 years, not 20. [Ileana Bladé, Spain]	Editorial: sentence revised
2-2548	2	70	9	70	9	Query - are you sure that "1870s" shouldn't be "1970s"? That date would be consistent with the availability of SST data. [John McLean, Australia]	Rejected: correct as written
2-2549	2	70	9	70	9	"since about the 1970s" (not 1870s) better fits Bunge and Clarke's paper. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected: correct as written.
2-2550	2	70	9	70	13	Refer also to discussion in 14.4.1 [Eric Guilyardi, France]	Accepted: reference made.
2-2551	2	70	16	70	24	This needs a table to say what all the abbreviations mean (or say where they can be found - glossary ??) [John McLean, Australia]	Rejected: reference to Box 2.5 Table 1 is given in the beginning of the caption.
2-2552	2	70	16	70	24	Explain "(-1)*" [Christian-D. Schoenwiese, Germany]	Editorial: meant to be self-explanatory
2-2553	2	70	16	70	25	Table 2.12: the Darwin SOI might be better referred to as standardised Darwin MSLP here. [Government of Australia]	Rejected: explained in Box 2.5 Table 1
2-2554	2	71	1	71	1	Unintentional juxtaposition of the words "mean" and "mode" that makes it harder for the reader to realize that mode is not used in the statistical sense of the word. [Dian Seidel, United States of America]	Editorial, revised accordingly
2-2555	2	71	1	71	14	Also refer to the debate about the existence/robustness of CP/EP distinction in 14.4.4. [Eric Guilyardi, France]	Accepted: reference made.
2-2556	2	71	7	71	8	A recent study (Su and Jiang, 2012) using satellite observations have detailed the differences between the eastern Pacific El Nino and central Pacific El Nino. The Su and Jiang (2012) paper should be cited here. Reference: Su, H., and J.H. Jiang, "Tropical Clouds and Circulation Changes During the 2006-07 and 2009-10 El Niños," J. Climate, in press.A recent study (Su and Jiang, 2012) using satellite observations have detailed the differences between the eastern Pacific El Nino and central Pacific El Nino. The Su and Jiang (2012) paper should be cited here.	Rejected: the general discussion of CP vs EP El Nino is given in Ch.14. Here only indices established for these pehomena are discussed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Reference: Su, H., and J.H. Jiang, "Tropical Clouds and Circulation Changes During the 2006-07 and 2009-10 El Niños," J. Climate, in press. [Government of United States of America]	
2-2557	2	71	10			In addition to Takahashi et al. (2011), Ren and Jin (GRL 2011) showed similar results. [Andrew Wittenberg, United States of America]	Taken into account
2-2558	2	71	12	71	15	This conclusion is contradicted by the absence of a robust trend in column water vapor as reported and plotted from 1989 to 2010 in the 2012 NVAP-M study (citation above). [Forrest Mims, United States of America]	Rejected: meaning of the comment is unclear.
2-2559	2	71	13	71	14	None -> However, none. Also, finish the thought here: there is no therefore no evidence that he chartacter of ENSO events has changed over the last century or in recent decades. [Ileana Bladé, Spain]	Editorial: paragraph revised
2-2560	2	71	14	71	14	The significance indicated in the Table is 5% not 10%, so you may want to add "not shown". [Ileana Bladé, Spain]	Rejected: caption of Table 2.12 explains how inferences can be made at 10% significance level.
2-2561	2	71	16	71	18	The PSA trends in Table 2.12 are only since 1951, not for the whole 20th Century. One of the PSA1 trends for 1951-2010 in Table 2.12 has opposite sign from expecations from the other PSA1 index trends, though it is non-significant, so maybe the sign as well as the level of significance depends on the PSA definition. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Editorial; signs of not significant trends are generally not discussed.
2-2562	2	71	20	71	20	I can´t find index AONM in Table 2.12. [Ileana Bladé, Spain]	Rejected: 5th row from the bottom
2-2563	2	71	20	71	20	AONM isn't explicitly defined in Box 2.5 Table 1. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted: revised accordingly.
2-2564	2	71	22	71	22	Saying "the trend has been interpreted" without providing the interpretation is odd [Ileana Bladé, Spain]	Accepted: revised accordingly.
2-2565	2	71	27			Section 2.7.9 Synthesis (of changes in atmospheric circulation): It may be better to use the term 'Summary'; rather than 'Synthesis' in the heading, for consistency with the wording used for other summary sub-section titles earlier in the chapter. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - Title changed.
2-2566	2	71	35	71	37	The justification for your comment that it is likely that circulation features have moved poleward is weak. The situation is easily explained by the dominance of ENSO conditions on the El Nino side of absolutely neutral (ie. SOI = 0), the side that means increased Hadley Circulation and decreased Walker Circulation, and all the consequent El Nino-driven variations in weather patterns. Numerous references are available to support this, including Trenberth et al (2002) - "Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures" and Wang, C. (2002) - "Atmospheric Circulation Cells Associated with the El Nino–Southern Oscillation". [John McLean, Australia]	Rejected. - Chapter does not attribute observed changes to causes (ENSO or other).
2-2567	2	71	35	71	40	results in this section is very interesting. Is it possible to conduct further research and understand the nature of variablity of this zonal wind? [Government of United Republic of Tanzania]	Noted.
2-2568	2	71	42			Section 2.8 Consistency Across Observations: this is a very useful section, as it identifies the cross-linkages between the different types of observations covered in this chapter and the combined evidence they provide on changes in the climate system. [Government of United Kingdom of Great Britain & Northern Ireland]	Section has been eliminated
2-2569	2	71	49	71	52	Are these consistent with the recent satellite optical depth trends reported earlier? [Government of Australia]	Section has been eliminated
2-2570	2	71	54	71	55	There's no mention of the period over which this is being considered. Temperatures have risen since the end of the Little Ice Age, temperatures have been flat for the last 15 years. State what period you are referring to and why you have elected to use that period. [John McLean, Australia]	Section has been eliminated
2-2571	2	71	55	71	55	However, there is a discrepancy in the phase of the SST trend in eastern tropical pacific due to differences in some global SST datasets when longer period data from late 19 century are concerned (e.g. Bunge and Clarke, 2009; Deser et al., 2010; DiNezio et al., 2010]). [Government of India]	Section has been eliminated
2-2572	2	71				Section 2.8 - this section seems a little obscure and out on a limb. A change of title might help 'Interconnectedness of the climate system' or even 'Observations - overview/conclusion' would help. I also think it needs a good succinct final paragraph that wraps up this whole chapter possibly drawing on the fact	Section has been eliminated

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						that despite the observational uncertainty, of which there is a lot, across all of these different observing systems and different variables, the same large scale message is clear. [Kate Willett, United Kingdom]	
2-2573	2	72	1	71	2	These changes are also consistent with greater Hadley Circulation, spreading warm air away from the tropics, so this should be mentioned. [John McLean, Australia]	Section has been eliminated
2-2574	2	72	4	72	6	This is a very vague statement. If it means what I think it is supposed to mean, then say "Recent analyses show no significant change in trend for land-based precipitation from the trend from 1900 to 1970." (or whenever). [John McLean, Australia]	Section has been eliminated
2-2575	2	72	4	72	6	See comment on global land precipitation: namely there does still appear to be a positive trend though I realise it may be non-significant. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Section has been eliminated
2-2576	2	72	4	72	6	For precipitation, where long-term data are available there are statistically significant changes, with increases in parts of the mid- and higher latitudes, and decreases in lower latitudes. Are these observations also consistent with stream-flow analyses? [Thomas Stocker/ WGI TSU, Switzerland]	Section has been eliminated
2-2577	2	72	8	72	10	This sentence waffles. Do you consider the eastern USA and Argentina to be low latitudes? This is what you seem to be saying. Please reword. [John McLean, Australia]	Section has been eliminated
2-2578	2	72	10	72	10	Change "Argentina" to "southern South America" [Government of Brazil]	reject: Southern south America includes Chile where precipitation didnt became wetter,
2-2579	2	72	12	72	13	It should be explained what "in close association with" means. Surface specific humidity has changed over the ocean in a manner consistent with changes in sea-surface temperature, with little change in relative humidity. Over land this has not happened for the last decade. Surface temperature over land has risen faster than sea-surface temperature, and surface relative humidity has declined (section 2.5.5) over this period. A decade is rather a long time for this behaviour to be dismissed by the use of the word "generally" early in the sentence. Please also see what hs been written in section 12.4.5.1 and comment 323 below. [Adrian Simmons, United Kingdom]	Section has been eliminated
2-2580	2	72	12	72	13	There has not been an incraes in surface specific humidity in the ERA-interim dataset, which has a longer, more recent and spatially complete coverage than the HadCRUH dataset on which this statement is based. [Geert Jan van Oldenborgh, Netherlands]	Section has been eliminated
2-2581	2	72	14	72	14	"Widespread observed increase in the fraction of heavy precip events" - this seems a stronger statement than the conclusions of section 2.6.2 would support, i.e., that there have been increases in more regions than decreases, and that many regions present statistically non-significant or negative trends. [Thomas Stocker/ WGI TSU, Switzerland]	Section has been eliminated
2-2582	2	72	17	72	18	brought milder maritime air into Europe and much of high-latitude Asia from the North Atlantic -> brought milder North Atlantic maritime air into Europe and much of high-latitude Asia [Ileana Bladé, Spain]	Section has been eliminated
2-2583	2	72	17	72	20	This bullet point is poorly worded. What is "zonal flow"? Surely you mean westerly winds? Also state precisely what you mean by "recent decades". [John McLean, Australia]	Section has been eliminated
2-2584	2	72	17	72	20	This is not true. The trend in the NAO, the meridional pressure difference over the Atlantic Ocean has indeed reverted back. However, the trend in the pressure difference over Europe (Mediterranean to Scandinavia) is still very strong (eg Fig.14 in van haren et al, 2012, doi:10.1007/s00382-012-1401-5). This difference is more important for the weather over continental Europe, whereas the NAO is more important for the British Isles and the Iberian Penisnula (their Fig.16). [Geert Jan van Oldenborgh, Netherlands]	Section has been eliminated
2-2585	2	72	19	72	19	trends -> wind trends [Ileana Bladé, Spain]	Section has been eliminated
2-2586	2	73	1	111	2	In several cases throughout the reference list, "+" is placed instead of page numbers; the completeness of the paper info must be checked [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2587	2	73	1	111	3	There is an inconsistent presentation of page numbers and journal issues throughout the References [Dale Hurst, United States of America]	Editorial, revised accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2588	2	73	1	111		References: There are duplicate entries for Lawrimore (2011) and Vose (2012). They each have an a) and b) and they should be combined into one reference. [Government of United States of America]	Editorial, revised accordingly
2-2589	2	73	49	73	51	I have not found Allen and Sherwood, 2007 cited in the text of the chapter but it is in the list of references [Juan Antonio Añel Cabanelas, United Kingdom]	Editorial, revised accordingly
2-2590	2	73		110		I have found no evidence of missing references (text compared to reference list - well done). [Government of Australia]	Editorial, revised accordingly
2-2591	2	74	15	74	15	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2592	2	74	17	74	17	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2593	2	74	26	74	28	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2594	2	75	40	75	40	I do not think that the Bousquet paper is a single author paper. [Government of Australia]	Editorial, revised accordingly
2-2595	2	75	48	75	49	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2596	2	76	1	76	3	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2597	2	76	14	76	14	Name of the last author looks rather strange. [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2598	2	76	30	76	30	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2599	2	76	49	76	52	The same paper is listed twice [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2600	2	81	40	81	41	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2601	2	82	24	82	25	Author list is wrong; the journal name is printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2602	2	82	24	82	26	Fix last names in reference. [Birgit Hassler, United States of America]	Editorial, revised accordingly
2-2603	2	82	24	82	26	Please note that citation "G., L., Y., M.H., C., L.T. and al., e., 2011. Scaling and trends of hourly precipitation extremes in two different climate zones -Hong Kong and the Netherlands. HYDROLOGY AND EARTH SYSTEM SCIENCES, 15(9): 3033- 3041" is wrong and duplicated with the one "Lenderink, G., Mok, H.Y., Lee, T.C. and al., e., 2011. Scaling and trends of hourly precipitation extremes in two different cimate zones - Hong Kong and the Netherlands. HYDROLOGY AND EARTH SYSTEM SCIENCES, 15(9): 3033-3041" in the same reference list. [Sai Ming Lee, Hong Kong, China]	Editorial, revised accordingly
2-2604	2	83	54	83	62	It looks like the same paper is listed thrice; there is not enough info on the paper. [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2605	2	85	36	85	37	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2606	2	85	40	85	41	Incorrect reference. Change "Hurst, D." to "Hurst, D.F., Oltmans, S.J., Vömel, H., Rosenlof, K.H., Davis, S.M., Ray, E.A., Hall, E.G. and Jordan, A.F." [Dale Hurst, United States of America]	Editorial, revised accordingly
2-2607	2	85	42	85	43	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2608	2	86	6	86	7	Author name and title are printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2609	2	86	18	86	19	Author name and title are printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2610	2	86	38	86	43	The same paper is listed twice [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2611	2	86	38	86	43	Duplicated reference. [Adrian Simmons, United Kingdom]	Editorial, revised accordingly
2-2612	2	87	18	87	19	The Keeling et al reference is presumably Keeling et al., 1976b [Government of Australia]	Editorial, revised accordingly
2-2613	2	87	20	87	23	The Keeling et al. references 1976b and 1976 are the same reference, Presumably there should just be a Keeling et al., 1976a [Government of Australia]	Editorial, revised accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2614	2	87	28	87	28	Kennedy et al. (2011a) is no longer in press. Kennedy J.J., R.O. Smith, N.A. Rayner (2011a) Using AATSR data to assess the quality of in situ sea-surface temperature observations for climate studies. Remote Sensing of Environment, Volume 116, 15 January 2012, Pages 79–92 http://dx.doi.org/10.1016/j.rse.2010.11.021 [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial, revised accordingly
2-2615	2	88	36	88	37	The reference is to be corrected as "Moorthy, K. K., Babu, S. S., Manoj, M.R. and Satheesh, S.K., 2012. Buildup of Aerosols over the Asian Monsoon Regime. Geophys. Res. Lett. (Submitted) and positioned alphabetically. [K KRISHNA MOORTHY, INDIA]	Editorial, revised accordingly
2-2616	2	89	25	89	25	Name of the last author looks strange. [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2617	2	93	3	93	4	Consider replacing Mishchenko, M.I., Geogdzhayev, I.V., Rossow, W.B., Cairns, B., Carlson, B.E., Lacis, A.A., Liu, L. and Travis, L.D., 2007a. Long-Term Satellite Record Reveals Likely Recent Aerosol Trend. Science, 315(5818): 1543-1543. with Mishchenko, M.I. and Geogdzhayev, I. V., 2007a. Satellite remote sensing reveals regional tropospheric aerosol trends. Opt. Express 15: 7423-7438. [Government of United States of America]	Editorial, revised accordingly
2-2618	2	93	8			Suggest adding before "Mitas, C.M. and Clement, A., 2005." the following reference: Mishchenko, M.I., Liu, L., Geogdzhayev, I.V., Li, J., Carlson, B.E., Lacis, A.A., Cairns, B., and Travis, L.D., 2012. Aerosol retrievals from channel-1 and -2 AVHRR radiances: long-term trends updated and revisited. J. Quant. Spectrosc. Radiat. Transfer 113: 1974-1980. [Government of United States of America]	Editorial, revised accordingly
2-2619	2	99	27	99	28	Author name and title are printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2620	2	99	39	99	40	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2621	2	100	15	100	15	"Vomel" is incorrect: should be either "Vömel" or "Voemel" [Dale Hurst, United States of America]	Editorial, revised accordingly
2-2622	2	100	20	100	21	C. Schiller, J.-U. Grooß, P. Konopka, F. Plöger, F. H. Silva dos Santos, and N. Spelten [Rolf Müller, Germany]	Editorial, revised accordingly
2-2623	2	100	48	100	48	Author name and title are printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2624	2	100	57	100	62	The same paper is listed twice? [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2625	2	101	7	101	8	Journal name is printed in capitals [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2626	2	102	15	102	16	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2627	2	102	22	102	24	Replace the existing reference by this updated one: Stahl, K.; Hisdal, H.; Hannaford, J.; Tallaksen, L.M.; van Lanen, H.A.J.; Sauquet, E.; Demuth, S.; Fendekova, M.; Jódar, J. (2010) Streamflow trends in Europe: evidence from a dataset of near-natural catchments. Hydrology and Earth Systems Science, 14. 2367-2382. 10.5194/hess-14-2367-2010. [Chong-Yu Xu, Norway]	Editorial, revised accordingly
2-2628	2	102	25	102	25	Stahl, K., and Co-authors, 2010: 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 [European Union]	Editorial, revised accordingly
2-2629	2	102	25	102	26	Replace the existing reference by this corrected version: Stahl, K., Tallaksen, L.M., Hannaford, J. & van Lanen, H.A.J. (2012) Filling the white space on maps of European runoff trends: estimates from a multi-model ensemble. Hydrol. Earth Syst. Sci., 16, 2035-2047. doi:10.5194/hess-16-2035-2012. [Chong-Yu Xu, Norway]	Editorial, revised accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2630	2	102	51	102	52	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2631	2	103	30	103	30	Name of the last author looks rather strange. [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2632	2	103	58	103	58	The title is printed in capital letters [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2633	2	105	31	105	34	Vecchi and Knutson 2011a and 2011b refer to the same paper [Fabrice Chauvin, France]	Editorial, revised accordingly
2-2634	2	105	31	105	34	The same paper is listed twice [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2635	2	106	3	106	3	Replace "Submitted" with "2012", because this has been published. [Xiaolan Wang, Canada]	Editorial, revised accordingly
2-2636	2	106	5	106	5	Replace "Research" with "Research, 117, D18110, doi:10.1029/2012JD017859" to update this reference. [Xiaolan Wang, Canada]	Editorial, revised accordingly
2-2637	2	106	15	106	15	"Vomel" is incorrect: should be either "Vömel" or "Voemel" [Dale Hurst, United States of America]	Editorial, revised accordingly
2-2638	2	106	33	106	38	The same paper is listed twice? [Andrey Shmakin, Russian Federation]	Editorial, revised accordingly
2-2639	2	106	33	106	38	Two Vose et al references - one is 2012, one is submitted - both appear to be the same. This will need rectifying where these are referred to in the text too Section 2.4.3 and 2.5.1.1. [Kate Willett, United Kingdom]	Editorial, revised accordingly
2-2640	2	107	6	107	7	Its final version is: Wang, K.C., Dickinson, R.E., Wild, M. and Liang, S., 2012. Atmospheric impacts on climatic variability of surface incident solar radiation. Atmospheric Chemistry and Physics, 12(20): 9581-9592. [Kaicun Wang, China]	Editorial, revised accordingly
2-2641	2	107	30	107	31	Add the following reference between these lines: "Wang, X.L., Y. Feng, G. P. Compo, V. R. Swail, F. W. Zwiers, R. J. Allan, and P. D. Sardesmukh, 2012d: Trends and low frequency variability of extra-tropical cyclone activity in the ensemble of Twentieth Century Reanalysis. Clim. Dyn., published online: 26 July 2012. DOI: 10.1007/s00382-012-1450-9." [Xiaolan Wang, Canada]	Editorial, revised accordingly
2-2642	2	107	30	107	31	Add the following reference between these lines: "Wang, X.L., B. Trewin, Y. Feng, and D. Jones, 2012e: Historical changes in Australian temperature extremes as inferred from extreme value distribution analysis. Geophysical Research Letters, Submitted. " [Xiaolan Wang, Canada]	Editorial, revised accordingly
2-2643	2	107	30	107	31	Add the following reference between these lines: "Wang, X.L., Y. Feng, and L. Vincent, 2012f: Historical changes in Canadian temperature extremes as inferred from extreme value distribution analysis. Geophysical Research Letters, Submitted. " [Xiaolan Wang, Canada]	Editorial, revised accordingly
2-2644	2	109	1	109	3	remove duplicate reference to Worden et al., 2012. [Helen Worden, United States of America]	Editorial, revised accordingly
2-2645	2	109	7	109	7	link for GHRSST needs removing [Elizabeth Kent, United Kingdom]	Editorial, revised accordingly
2-2646	2	112	1	113	11	Appendix 2.A.I think the appendix is missing a description of halomethanes (CH2Cl2, CH3Cl, CHCl3, CH3Br, CH2Br2, CH3I) which should be included. [Florinda Artuso, Italy]	Editorial, revised accordingly
2-2647	2	112	1	139	1	These Appendices are very useful. They should be with the Final Report and not lost on some web site. This happened with AR4. Don't let this happen again. If you do let it happen you will have to go through and remove some refs as they only appear in these Appendices. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Editorial, revised accordingly
2-2648	2	112	5	112	5	All of these measurements were mad almost exclusively aover the sea, and unde restrictive circumstances. There are almost no measurements over land surfaces so the figures are far from being globally averaged. [Vincent Gray, New Zealand]	Editorial, revised accordingly
2-2649	2	112	43			Table 2.A.1 Why doesn't CO2 have a "yes" in the natural source column? Same question for SF6 (Deeds et al. (2008) and C2F6 (Worton et al.) [Government of United States of America]	Editorial, revised accordingly
2-2650	2	116		116		Table 2.A.2: Overview of O3 trends reported in the literature, using data sets with at least 10 years of measurements, wherein the expressions of Taiwan, China, Hong Kong and Beijing are incorrect. Please make changes at the first line of Table 2. A. 2, from “Taiwan, Surface” to “Taiwan, China, Surface” ; from “Hong	Editorial, revised accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Kong, surface" to "Hong Kong, China, surface" ; from "Beijing boundary layer" to "Beijing, China, boundary layer". Please make changes at the second line of Table 2. A.2, from "YangMing mountain site in north of the country" to "YangMing mountain site in north of the region" ; from "Composite of urban sites in the north of the country" to "Composite of urban sites in the north of the region" ; from "Composite of 4 sites in the south of the country" to "Composite of 4 sites in the south of the region. [Government of China]	
2-2651	2	119	3	119	3	Please consider to replace "ca." to "approximately" for consistency throughout the chapter. [Government of NORWAY]	Editorial, revised accordingly
2-2652	2	119	16			To be Inserted after PM10 levels.Acid precipitation trends show a significant increase in rain acidity in China since 2000 (Jie et al., 2010). Ref: Jie, T.; Xiao, X.; Bin, B.; Jin, A.; Feng, W.S. 2010. Trends of the precipitation acidity over China during 1992–2006. Chin. Sci. Bull. 2010, 55, 1800–1807. doi: 10.1007/s11434-009- 3618-1. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2653	2	119	21	21	28	Regarding trends of PM10 aerosols, the findings of the following paper should also be considered- Modelling 10-year trends of PM10 and related toxic heavy metal concentrations in four cities in India. Indrani Gupta, Abhaysinh Salunkhe and Rakesh Kumar. Journal of Hazardous Materials 179 (2010) 1084–1095. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2654	2	119	32		34	Since the regional networks such as ARFINET and AEROCAN have not been included in the analysis, sentence `Since AR4……..…..analysis' should be deleted. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2655	2	119	37			Section 2.3: Throughout this section, please check the Hand et al. 2011 a, b, c references. The citations are almost identical in the reference section p 2-83. The closest paper on-line is the following: Seasonal composition of remote and urban fine particulate matter in the United States J. L. Hand, B. A. Schichtel, M. Pitchford, W. C. Malm, and N. H. Frank J. Geophys. Res., 117, D5, doi:10.1029/2011JD017122, 2012 [Government of United States of America]	Editorial, revised accordingly
2-2656	2	119	41		51	The following report should also be considered to describe the trends of precipitation in USA Willey, J.; Kiber, R.; Avery, G.B., Jr. 2006. Changing chemical composition of precipitation in Wilmington, North Carolina, U.S.A.: Implications for the continental U.S.A. Environ. Sci. Technol. 40, 5675–5680. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2657	2	119	52			The scenario and trends of acidity in the United States, Europe, China and India have been reviewed by Kulshrestha (2012). This study also compares the nature of sulphate present in rain water in the United States, Europe and India. Ref: Kulshrestha U.C. Acid Rain. Encyclopedia of Environmental Management, Taylor and Francis (in press). [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2658	2	119				Section 2.A.2.3: This section lacks wet deposition and its trends in Indian region. I suggest that following the discussion about PM10 aerosols (after line 28), there is need to mention that- In India, a significant increase of SO4 and NO3 in rain water has been reported at Pune during 1984–2002 (Safai et al., 2004).But the Indian precipitation is not reported highly acidic due to the interference of CaCO3 dominated dust particles (Kulshrestha et al., 2003). Ref: Chemical characteristics of rain water at an urban site of south-central India. Kulshrestha, U.C.; Kulshrestha, M.J.; Sekar, R.; Sastry, G.S.R.; Vairamani, M. Atmos. Environ. 2003, 37, 3019–3026. Ref: Chemical composition of precipitation during1984–2002 at Pune, India	Editorial, revised accordingly

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						P.D. Safai*, P.S.P. Rao, G.A. Momin, K. Ali, D.M. Chate, P.S. Praveen. Atmospheric Environment 38 (2004) 1705–1714 [Umesh Kulshrestha, India]	
2-2659	2	120	22	120	26	In relation to Indian region, the following study needs to be included in this paragraph- Satheesh, S. K. and Ramanathan, V. (2000). Large differences in tropical aerosol forcing at the top of the atmosphere and Earth's surface. Nature Vol. 405, Issue 6782, pages 60-63. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2660	2	120	24		26	This sentence `At the northern station..... (Kaskaoutis et al., 2012)'is not relevant to the present para and hence needs deletion.This can be inserted on page 119 line 21. [Umesh Kulshrestha, India]	Editorial, revised accordingly
2-2661	2	120	25	120	25	Replace attribute by "which, the authors attribute to". In the next sentence, remove "which were attributed to". [Government of India]	Editorial, revised accordingly
2-2662	2	120	28	124	34	This one is very useful. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted
2-2663	2	120	36	124	35	Considering how tight the space restrictions are, why does the IPCC present 5 pages of elementary derivations of trend slope estimators that can be found in any introductory regression textbook? And why go through such lengthy explanations of methods that are 50 or more years behind the state of the art anyway? You are calling a great deal of attention to one of the weakest aspects of your work. All you need to say is, for example, that you use least squares trend estimates with AR1 errors, and then you can cite an intro regression text if somebody wants to see the derivation. Readers who don't know any stats won't read this section, and those who do know their stats won't think much of it. [Ross McKitrick, Canada]	Rejected. Space restrictions are very tight for the main text. Appendix provides an additional space allowance to supply the details that complement more general descriptions given in the main text. Here we provide explicit formulas to make it unambigous what exactly was calculated as linear trends and their uncerainties in this report.
2-2664	2	120	40	120	41	What is meant exactly by "for time series like the one used here"? For more regional series, which generally have more non-linear trends than global mean temperature, these conclusions about the estimates being similar will not always be valid. So I think you have been rather selective with the data example. It would be more transparent and honest to also include an example of a climatic time series where the methods give different results. In other words, be open about the limitations of the linear trend approach used in this chapter rather than advocating it based on a carefully chosen example. [David Stephenson, United Kingdom of Great Britain & Northern Ireland]	Taken into account: revised accordingly.
2-2665	2	120	40	120	43	All serious statistical analyses of temperature data indicate that an AR(1) model is insufficient to describe the complexity of the dependence. In many case long-term memory models or at least higher order ARMA models are needed, and in these cases the standard errors will turn out quite different from models assuming independence or AR(1). [Peter Guttorp, United States of America]	Taken into account. The sentence is rewritten.
2-2666	2	122	1	122	1	a.k.a.' should be given in full, but in any case is not appropriate here. The first-order auto-regressive error model is but one type of serially correlated error. There are obviously many other possibilities. [Government of Australia]	Accepted: revised accordingly.
2-2667	2	122	16	122	16	available (not avalable, typo). [Christian-D. Schoenwiese, Germany]	Editorial: corrected
2-2668	2	123	0	123	0	It is not clear from this discussion why the preferred methods were chosen - to be described [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account: revised accordingly.
2-2669	2	123	21	123	21	trend (not tend, typo). [Christian-D. Schoenwiese, Germany]	Editorial: corrected
2-2670	2	123	22	123	22	Explain what the notation '0.114–0.026+0.023' means, since this is non-standard [Government of Australia]	Accepted: revised accordingly.
2-2671	2	124	28	124	34	The description of the spline must explain two things to the person viewing figure in box 2.2: a) What smoothing is applied and how is it found; b) what is the meaning of the gray shaded envenlope. The way the confidence interval is found, and more importantly "what they mean" should be explained here. [Government of Iceland]	Accepted: revised accordingly.
2-2672	2	125	1	125	1	We would like to suggest adding a question related to the "hockey stick controversy". There has been published a tremendous number of articles related to this controversy, together with intense media and	Rejected. This is the domain of Chapter 5 and not Chapter 2 so is out of scope.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						blogosphere attention. For policy makers it is important to know the latest insights and the context of this controversy. [Government of Netherlands]	
2-2673	2	125	1	128	38	This one also but it can be improved by giving a little bit more information here and there. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Where suggestions have been given they are responded to individually below.
2-2674	2	125	6	125	6	These delays are reducing as more NMSs put data online quicker. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted and text added to this effect.
2-2675	2	125	6			It could be pointed out that the statement concerning to international data exchange delays refers to one type of observational data - the monthly CLIMAT messages used in analyses such as CRUTEM4. Delays do not apply to the plentiful surface synoptic data used by reanalyses that are extended in close to real time such as ERA-Interim and JRA-25. These two analyses have been shown to produce results in good agreement with CRUTEM3 and CRUTEM4, taking spatial sampling differences into account (Onogi et al., 2007; Simmons et al., 2010; Jones et al., 2012). [Adrian Simmons, United Kingdom]	Noted. The same also applies to daily summaries. A sentence has been added to make this clear.
2-2676	2	125	9	125	13	Is it worth stating that ICOADS is no longer funded? [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected. ICOADS is supported on reduced funding levels through NCDC. Further, this would arguably stray from a policy-neutral stance.
2-2677	2	125	25	125	44	This section is good, but with a little more work could be really good! [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Where suggestions have been given they are responded to individually below.
2-2678	2	125	25	125	44	Useful to state in the text or Table that the first two and maybe the third dataset use anomalies and don't use the absolute temperatures. Also add that all are working with monthly averages. Also that grids are produced for many datasets for land and for SST - again not using absolute values. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. We have added a sentence to make clear that these are monthly products.
2-2679	2	125	36	125	36	Replace 'stations' by 'some countries and some stations'. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2680	2	125	36	125	44	Some of these series may have already been adjusted. It would be useful to test the NCDC and BEST techniques using data for Canada which have already been adjusted. This way it could be found whether anything extra is added. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. We have added and adjusted to this passage of text. The suggestion of an investigation is new work and as such deemed outside the scope of an assessment per se.
2-2681	2	125	41	125	41	The new Rhode et al. (submitted) approach should be regarded with more caution than is given here, until such time as it gets properly assessed by the climate community. [Government of Australia]	Noted. The fact that it is new is clearly articulated here. Implicit in that is the rest of the concerns raised by the reviewer and it is not felt correct to explicitly sttae them. GHVCNv3 is also a new method for an old dataset so similar concerns rightly should pertain there.
2-2682	2	125	47	125	47	The CRUTEM4 number of stations has increased in the latest update - see the website. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. We queried this value with CA Colin Morice and have updated accordingly.
2-2683	2	125	47	125	47	Useful to say for CRUTEM4 that the 2 hemispheres are produced by weighted averages of grid boxes like GHCNv3. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2684	2	125	47	125	47	Need to emphasize that BEST doesn't produce a grid so useful for many analyses that the other datasets can achieve. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. This is already stated within the table.
2-2685	2	125				Changes in SST observing practices are given prominence on the following page - why not equal prominence to changes in land air temperature observing practices re the type of instrument enclosure. In Australia, the change to the Stevenson screen is known to be an important change. [Government of Australia]	Noted. The appendix is intended not to repeat content within the main text and the issue of changes in instrumentation is mentioned in the main text so not repeated here.
2-2686	2	125				Table 2.A.4: Lawrimore et al. 2011 do not mention infilling of their land surface air temperature products even for calculating global averages. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. This is in Smith et al., 2008 which is now added to the parenthetical comment to which this

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							pertains.
2-2687	2	126	3	126	3	Necessary to add SST before data as this is all about SSTs. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Rejected: there are words "Sea Surface Temperature" in the beginning of the title of subsection 2.A.4.3
2-2688	2	126	7	126	7	"floating" used here, does this mean a combination of both moored and drifting buoys? If so perhaps "surface" would be a better description. If it refers to just drifting buoys, use that terminology which is also used later in this subsection [Elizabeth Kent, United Kingdom]	Accepted
2-2689	2	126	14	126	20	Suggest making the period under discussion more explicit. Essentially what is being discussed is the post-WW2 SST biases not considered in e.g. HadSST2 and ERSST. I would remove reference to 1930 and the "modern era" (unless you define it) and define the period being discussed. [Elizabeth Kent, United Kingdom]	Taken into account in the revision
2-2690	2	126	14	126	20	There is no mention of the presence of biases in the modern buckets. These are smaller than for canvas buckets (which were probably in declining use until around 1970) but seemed to be detectable in nighttime data in the well-sampled North Atlantic (Kent and Kaplan 2006, JTech, 23, 487-500) in the period 1970-1997. [Elizabeth Kent, United Kingdom]	Rejected: covered by AR4
2-2691	2	126	14	126	20	The picture from the literature is of biases in ERI which can be large for individual ships. The biases can be of any sign but tend to be for ERI SSTs to be too warm (Kent et al. 2010, Kennedy et al, 2011 -pt2). The average warm bias in ERIs is greater than the expected cool bias relative to the surface measurements of the buckets. The Kent and Taylor and Kent and Kaplan (2006) studies suggested that the ERI biases are becoming smaller (and on average less warm) over time. This is complicated by the changing contribution of the different measurements methods over time. Given that the ship-satellite and ship-drifter comparisons are for the period after about 1990 when the ERIs seemed to have smaller biases, care needs to be taken in considering the time periods over which the comparison of relative biases between data types are relevant. [Elizabeth Kent, United Kingdom]	Taken into account: a potential time period differences between comparisons using ATSR data and in-situ-only studies.
2-2692	2	126	15	126	16	Kent et al. (2010) refer to many reasons why the ERI might be biased based on a review of the literature. Kent and Taylor (2006 - JTech 23, 464-475) showed that for the period 1970-1990 that ERI SSTs were warmer than bucket SSTs in the North Atlantic but that subsequently ERI and bucket SSTs were more similar. The comparisions of ship SSTs with drifting buoy SSTs are from the later period so may understimate the ERI bias for the earlier period, although the number of ERIs is likely to be smaller in this earlier period. [Elizabeth Kent, United Kingdom]	Taken into account
2-2693	2	126	17	126	17	This point with the 0.12-0.18K number would be useful in section 2.4.2. Also useful to refer to Fig 2.17 top. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted: done
2-2694	2	126	20	126	20	"tended to MEASURE colder". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial: not clear if this suggestion improves the text.
2-2695	2	126	22	126	23	Need some comment about the space and time scales over which SST and MAT anomalies are thought to be similar. [Elizabeth Kent, United Kingdom]	Taken into account in the revision
2-2696	2	126	25	126	26	MAT and SST are not independent. Tropical MAT is used to fit the adjustments applied for the pre-WW2 period in the Hadley Centre datasets (e.g. HadSST3, HadISST). The fit to MAT defines the proportions assumed for wooden and canvas buckets and largely defines the trend in the adjustment. The adjustments applied to ERSST are completely dependent on large-scale variability in MAT. This is touched on in the caption to Figure 2.16 but should be made clear here. [Elizabeth Kent, United Kingdom]	Taken into account in the revision
2-2697	2	126	26	126	28	HadNMAT2 should also be described here as it is used in e.g. Figure 2.16. HadNMAT2 starts in 1880 and avoids the adjustments applied in MOHMAT4.3N in the 19th Century. [Elizabeth Kent, United Kingdom]	Accepted
2-2698	2	126	28	126	28	N in NMAT is not explained. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have now spelt out Night
2-2699	2	126	31	126	31	Table A.2.5 contains also NMAT datasets not just SST [Elizabeth Kent, United Kingdom]	Accepted
2-2700	2	126	35	126	38	HadSST3 must go to 2011/2 as it is used in HadCRUT4. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. This is the case now but wasn't at the time of drafting.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2701	2	126	35			Include HadNMAT2 in Table 2.A.5 [Elizabeth Kent, United Kingdom]	Accepted
2-2702	2	126	38			Table 2.A.5 HadSST3 now runs to 'present' and not 2006 [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2700
2-2703	2	126				Replace MOHMAT4.3N by HadNMAT2 (Kent et al., 2012, accepted subject to revision) [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2704	2	127	14	127	14	Issue: Our group's relevant work could be cited to strengthen the discussion here. Suggested Changes: add two sentences after "...in the top 100 um." on line 14 of p. 2-127: "A physically-based model to account for the warming layer and cool skin effect has been developed and implemented in the ECMWF operational model (Zeng and Beljaars 2005). A multiyear hourly sea surface skin temperature data were also derived from the buoy bulk temperature and wind speed over the tropical Pacific (Zeng et al. 1999)." References for comments on Chapter 2: Wang, A., and X. Zeng, 2012: Development of global hourly 0.5-degree land surface air temperature datasets. J. Climate, accepted with revisions. Zeng, X., and A. Beljaars, 2005: A prognostic scheme of sea surface skin temperature for modeling and data assimilation. Geophys. Res. Lett., 32, L14605, 10.1029/2005GL023030. Zeng, X., and A. Wang, 2012: What is monthly mean land surface air temperature? Eos Trans., 93 (15), 156. Zeng, X., M. Zhao, R.E. Dickinson, and Y. He, 1999: A multi-year hourly sea surface skin temperature dataset derived from the TOGA TAO bulk temperature and wind speed over the Tropical Pacific. J. Geophys. Res. - Oceans, 104, 1525-1536. [Xubin Zeng, United States of America]	Rejected: this subsection reports on comparisons between different kinds of data with the purpose of constraining data uncertainty. It does not intend to present a comprehensive review of surface layer models and data sets per se.
2-2705	2	127	24	127	24	"Random errors on ATSR" should read "Uncertainties associated with random errors on ATSR" [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Editorial: replaced by "random error magnitudes"
2-2706	2	127	45	127	45	With the 90% error ranges, better to say these are 5-95%. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2707	2	128	12	128	16	Dates cited need to match the legend of Figure 2.A.2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted and so fixed
2-2708	2	128	20	128	20	Table numbering has gone awry in this section. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted and so fixed
2-2709	2	128	27	128	27	Change 2.4.1.2 to 2.4.1.3 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2710	2	128	36	128	38	For HadCRUT4 again say that combination is of weighted averages for the hemispheres then simple averaging of the two to the globe. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2711	2	128	36	128	38	Again state that the datasets produce grids as well as large-scale averages. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Accepted. Gridded added to the table caption.
2-2712	2	129	1	133	27	Many of the points raised for surface temperature also could be added to upper air temperatures as well - like grids and anomalies. [Phil Jones, United Kingdom of Great Britain & Northern Ireland]	Noted. Such text added where deemed appropriate.
2-2713	2	130	28	130	29	John et al. citation should be 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, D02305, doi:10.1029/2011JD016349. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2714	2	131	3			Section 2.A.4.7. should include reference to Wang et al (2012) and Thompson et al (2012) regarding SSU	Noted. This has been addressed in redrafting the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						data. [Dian Seidel, United States of America]	main text. Given that this section is discussing the technical issues and not summarizing the methods it is felt inappropriate to also add here.
2-2715	2	131	5	131	15	Please see earlier comments 90 and 91 concerning the work of Kobayashi et al.(2009) which is much more comprehensive than implied in the reference given in this supplemental material, and which should be referenced in the main body of the text, not buried away in this supplemental material. [Adrian Simmons, United Kingdom]	Accepted. This has been addressed in redrafting this section and the main text.
2-2716	2	132	37	132	45	Also include HadNMAT2 (Kent et al., 2012, accepted subject to revision) [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Given late availability of this product it could not be included in the SOD. It will be added.
2-2717	2	134	0	135	0	Table 2.3: It should incude the ARFINET observations of BC over south Asia: Currently BC measurements are available over 33 stations. Eight of them (data from 2000/2003 to 2011) show decreasing trend @ 2to 3% per annum, six of them show increasing trends (3% per year) while trends are insignificant at other stations. [K KRISHNA MOORTHY, INDIA]	Editorial, revised accordingly
2-2718	2	134				Suggest including satellite optical depth trends in this table. [Government of Australia]	Editorial, revised accordingly
2-2719	2	135		135		The references for Particle number concentration trends should be Asmi et al, Atmos. Chem. Phys. Discuss., 12, 20849-20899, doi:10.5194/acpd-12-20849-2012, 2012. Please also re-check the actual trend magnitudes, as there are minor disparities to the results presented on that paper [Ari Asmi, Finland]	Editorial, revised accordingly
2-2720	2	135		135		The citation given for trends in Particle Number Concentration is wrong. The correct citation is Asmi et al (2012), which is already included in the References on p. 2-74. [John Ogren, United States of America]	Editorial, revised accordingly
2-2721	2	136	2	137	2	3 choices of "CP/Modoki" possible definitions are listed out of the 10 that have been recently proposed. Given the debate that exists on the very existence of this "central Pacific El Nino" (14.4.4), I am not sure listing these is either helpful nor reflecting the current science. IPCC AR6 may be a better place to present this once the science is settled. [Eric Guilyardi, France]	Rejected. Box 2.5, par. 2 lists general principles according to which the indices were selected for Box 2.5 table 1.
2-2722	2	136				Box 2.5 Table 1, last column (“comments”), row related to the EMI: Replace “Defines..std” by "Defines “typical El Niño Modoki events” as those years for which the seasonal mean EMI value for both (JJAS and consecutive DJF seasons) is no less than 0.7σ, where σ is the seasonal EMI std." [Government of India]	Rejected: the suggestion is not consistent with the cited reference
2-2723	2	141	5		9	some indication of the number of sites making up the NOAA trend is needed--is it also only MLO and SPO as are the SIO results, or is it the full site of NOAA sites? [Stephen Montzka, United States of America]	Figures has changed
2-2724	2	142	1	191	2	Axis labels on figures are very inconsistent from one figure to the next. In some cases only the unit is shown (figures 2.5, 2.6 and 2.14). Capitalization is inconsistent, as an example figure 2.18 uses “Anomaly” whereas 2.19 uses “anomaly”. In some cases the anomaly baseline period is given in the axis label, in other cases it is not. [Government of Iceland]	Figures has changed
2-2725	2	142	1	191	4	Most of the graphs treat the time series as if iwere based on constants. In reality all them have large uncertainties, which, if added, would destroy the stitistical vakidity of many of the claimed trends [Vincent Gray, New Zealand]	Figures has changed
2-2726	2	142	5	142	5	Globally averaged CO2 dry air mole fraction: do you mean surface, marine air ? [Ingeborg Levin, Germany]	Figures has changed
2-2727	2	142	7			Strike "instantaneous"; otherwise caption is good. [Stephen E Schwartz, United States of America]	Figures has changed
2-2728	2	142	9	142	9	Please mention on what time scale the derivative is defined. [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2729	2	142				fig. 2.1 If including the locations (e.g. Mauna Loa and South Pole) for the SIO data-sets, probably should also include a description of the actual locations for the NOAA/ESRL/GMD data to be consistent. [Government of United States of America]	Figures has changed
2-2730	2	142				If red and blue in a) are both global avg, why so much oscillation in blue and not in red? And despite this the two data sets are in agreement in b). Something not explained here. [Stephen E Schwartz, United States of America]	Figures has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2731	2	143	1	143	1	Fig. 2.2 Red and green are not distinguishable for most colourblind people. [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2732	2	143	4	143	4	Globally averaged CH4 dry air mole fraction: do you mean surface, marine air ? [Ingeborg Levin, Germany]	Figures has changed
2-2733	2	143				Figure 2.2. Is it possible to enlarge the scale of graph b in order that the growth rate of AGAGE (red curve) is fully displayed? [Government of Chile]	Figures has changed
2-2734	2	144	4	144	4	Globally averaged N2O dry air mole fraction: do you mean surface, marine air ? [Ingeborg Levin, Germany]	Figures has changed
2-2735	2	144				Figure 2.4: label for green line in upper panel of the figure has different color than line. [Birgit Hassler, United States of America]	Figures has changed
2-2736	2	145	4	145	4	Are these marine air mole fractions ? [Ingeborg Levin, Germany]	Figures has changed
2-2737	2	145	4	145	9	Figure 2.4: the caption needs to describe how the global halogen-containing LLGHGs data are smoothed for presentation [Government of Australia]	Figures has changed
2-2738	2	146	1	146	1	There are several stray marks on the axes of Figure 2.5: "2.0" on y-axis of top panel;x-axes of both panels have the top half of a "Y" below the years [Dale Hurst, United States of America]	Figures has changed
2-2739	2	146	4	146	8	Figure 2.5: the caption needs a reference to the Boulder H2O data [Government of Australia]	Figures has changed
2-2740	2	146				fig. 2.5 Specify what year(s) the water vapor anomalies are relative to in top figure. In the bottom figure, the caption says normalized to 2000-2011, but visually, the 2000-2011 period seems to be mostly negative. [Government of United States of America]	Figures has changed
2-2741	2	147	4	147	4	top panel of Figure 2.6 claims "global" for 60°S-60°N instead of "middle latitudes and tropics" [Dale Hurst, United States of America]	Figures has changed
2-2742	2	148	1	148	1	Note that the "ppb" for Ozone is a volume mixing ratio and not a mole fraction, and thus needs to be defined separately to that of e.g.CH4 or N2O [Ingeborg Levin, Germany]	Figures has changed
2-2743	2	149		149		Please replace “Hong Kong” in Figure 2.8 with “Hong Kong, China”. [Government of China]	Figures has changed
2-2744	2	149				There should be no (a) on the figure, there is only one panel. [European Union]	Figures has changed
2-2745	2	150				figure 2.9: please clarify what is the meaning of black area? [Rahimzadeh Fatemeh, Iran]	Figures has changed
2-2746	2	151	6	151	6	There needs to be an explanation in the legend of what the colours mean in this figure - i.e. what does the blue colours mean and what do the reds mean etc. [Government of Australia]	Figures has changed
2-2747	2	151		151		The maps given on Figure 2.10 mark the Sino-Indian border with "McMahon Line" and no intermittent lines in the South China Sea. It is recommended to delete these maps or replace them with a border free one. [Government of China]	Figures has changed
2-2748	2	151				figure 2.10: please clarify what is the meaning of gray area? [Rahimzadeh Fatemeh, Iran]	Figures has changed
2-2749	2	152				fig. 2.11 Color code should be adjusted so that a value of -10 can be discriminated from +10 (both are a shade between purple and red). Also what is the difference between a black dot (non-significant trend) and a white dot (0% change)? [Government of United States of America]	Figures has changed
2-2750	2	153	1	153	3	Both figures are need further work. Axis labels are crap and have different sizes. The spline curve needs better explanation. How is the tension in the curve (the degree of smoothing) chosen? Furthermore the average reader will not understand the jargon “90% confidence interval on the smooth curve”. 90% of what? For many time periods more than half the points lie outside the envenlope. [Government of Iceland]	Figures has changed
2-2751	2	153				I suppose, it is very important box. But for to be better , I recommend you show in the figure trend with and without considering the first order autocorrolation! Meanwhile I suggest you to notice at the diggle approch that it was mentioned in the AR4 [Rahimzadeh Fatemeh, Iran]	Figures has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2752	2	153				Box 2.5, fig. 1 Suggest adding a y=0 line in each figure [Government of United States of America]	Figures has changed
2-2753	2	153				Box 2 Fig. 1 See the comment on ch 2 p 21 line 6-21 that using 50 vs 60 years for the two lines is misleading. Use 55 years for each straight line fit. Secondly recommend superimposing at least red noise corrected or preferably ARIMA (1,1) corrected, trend of temperature calculated by a moving least squares trend line using the length of the Schwab solar cycle to remove its effects. e.g. with an 11 year, or preferably 10.75 year length for calculating the moving trend. Girma shows a very clear 60 year cycle superimposed on a gradually increasing sine wave. Please state the equivalent analysis length of the spline curve shown. The trend has been zero since 2000 and negative since 2001 and 2002 as shown by statistician Lucia Liljegren at the Blackboard 3 Oct 2012 http://rankexploits.com/musings/2012/arima11-mc-corrected-gistemp-trends-inconsistent-with-0-2cdecade/ [David L. Hagen, United States of America]	Figures has changed
2-2754	2	154	1	154	2	You claim that the energy is "balanced" and the parameters are all constants. This implies it is in equilibrium, but it is not, because your "Climate System" is not isolated from imput or output of energy, so there is no reason why the energy should be "balanced" [Vincent Gray, New Zealand]	Noted - The climate system to date is indeed not in complete equilibrium. The current imbalance of about 0.6 Wm-2 (imbalance at the Top of Atmosphere, which is equal to the imbalance at the surface due to the low heat capacity, and shown as residual on Figure 2.12), induces a warming of the climate system.
2-2755	2	154	1	154	2	This model is fundamentally incorrect. A preliminary critique can be found at http://nzclimatescience.net/images/PDFs/the%20greenhousexxx.pdf . Your "Climate System" is only one part of a complete climate system..The complete climate is a heat engine. The input energy is the radiation from the sun, which is aborbed by each part of the earth, only by day. Some of the absorbed energy is converted ito chemical energy, which carries out reactions to maintain all the living organisms with a decrease of entropy. This importan part of the sun's energy is not included in your model. What remains is dispersed by conduction, convection, and latent heat exchange. Air and ocean movements play an an important part in these processes. Their scientific study is inhibited by the "chaos" associated with problems of fluid flow. Your model implausibly claims to have eliminated :chaos: altogether as well as almost elimnating all of the essential mechanisms for energy transfer. Finally this energy is radiated to space from every surface of the earth and every level of the atmosphere; which represents the exhaust of the system. The energy received from the sun undergoes an increase in entropy in the entire process. Your model largely ignores all of these procedures and replaces them with aninternal radiation exchange. It is possible these exchanges may make a contribution to the entire energy flow, but they have not been detected and they would probably be covered up by the chaos of fluid flow. Despite massive efforts you have failed to show their effects are either detectable or of practical importance.They do not currently figure in practical weather forecasts and probably never will. [Vincent Gray, New Zealand]	Rejected - this is not a model, but a schematic diagram of the spatially and temporally averaged energy balance of the Earth, thus representing global annual mean conditions. Therefore, neither diurnal nor seasonal variations are shown in the diagram, as it is an annual mean representation, and no circulation is depicted, as this diagram does not include a horizontal spatial dimension. This does not mean that these effects are not important and influence climate at a specific location, season and daytime on the globe. In fact the 3-dimensional global climate models include all these aspects, they calculate with a time step of only a few minutes the entire diurnal cycle (thus day and nighttime conditions), and include the fluid dynamics of the ocean and atmosphere to their full extent. Theses models used in meteorology and climate science, to which the reviewer refers to, are thus highly complex systems and deal with all the issues raised by the reviewer. They are of immense merit in practical weather forecasting. The diagram 2.12 on the other hand shows only the first order balances on a global mean basis which determine the overall energy exchanges between Earth, Sun and space, within the atmosphere and at the surface. These are, however crucial as well, as subtle changes in the balances at the top of atmosphere cause a warming (if more goes into the system than out) or a cooling (if more goes out than in) of the system. Changes in the radiation balance at the Earth's surface can induce changes in the latent heat flux and thus changes in the intensity of the global water cycle.
2-2756	2	154	1	154	2	This version of the model admits that there is a "range" of possible figures for the various parameters. This alone implies that there could never be a :balance" as the difference between input and output is now variable. In this situation any calculated effects of increases in gennhouse gases is on top of a moving target, which	Rejected - the ranges given in Figure 2.12 are uncertainty ranges in the current estimates of the magnitudes of the various fluxes. They are not meant

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						may be positive or negative. In actuallity many of the parameters are averages from skewed distributions and some are based on supposed knowledge of the average temperature of the earth wjich is itselt afected by large inaccuracies. So this model breaks down even if all the other implausible assumptions are accepted. The entire report is futile. [Vincent Gray, New Zealand]	to be variable and do allow for the closure of the budget. Even though the estimation of the absolute magnitudes of the fluxes are afflicted with some uncertainty, this does not mean that changes in these fluxes (e.g. due to increasing levels of greenhouse gases) can be ignored.
2-2757	2	154				Figure 2.13 - this is an earlier version of the Figure in which CERES and OHC datasets were offset by 6 months. A new version of this figure appears in Loeb et al. (2012) where the datasets have consistent averaging periods and unfortunately the agreement isn't quite so good! [Richard Allan, United Kingdom]	Accepted, we have address this point by revising Fig. 2.13.
2-2758	2	154				Fig. 2.12: This is a key figure but the picture in the background disturbs more than it helps. I propose to keep this schematic as in the original Kiehl/Trenberth paper. [Reto Knutti, Switzerland]	Noted, the Figure obtained also many positive comments on its graphic design (e.g., Jim Hansen GISS Nov 22, 2012 per email: "congratulations for making the best version of that diagram -- finally, one that is pretty and good for an audience! Jim". Of course this is also always a matter of taste and habituation.
2-2759	2	154				The 0.6 at bottom left of figure should denote "planetary heating rate", not simply "residual". Graphically, it might be shown over ocean rather than land. [Stephen E Schwartz, United States of America]	Taken into account, we changed "residual" to "imbalance"
2-2760	2	155	1	155	2	Figure 2.13 label on y-axis says: “Net TOA flux or ocean heating rate”. The use of OR is wrong. Figure shows both ocean heating rate AND Net TOA flux. [Government of Iceland]	Accepted, we've changed "or" to "and"
2-2761	2	155				I question the uncertainties associated with the CERES product here and indeed whether the CERES EBAF product can be used to say anything about net TOA flux and upper ocean heating rates. From the CERES EBAF web page (http://eosweb.larc.nasa.gov/PRODOCS/ceres/level3b_ebaf_table.html): EBAF-TOA Monthly and climatological averages of TOA clear-sky (spatially complete) fluxes, all-sky fluxes, and cloud radiative effect (CRE), where the TOA net flux is constrained to the ocean heat storage. EBAF-Surface Monthly and climatological averages of computed surface clear-sky and all-sky fluxes (up/down) consistent with the CERES EBAF-TOA fluxes. As these data are constrained to ocean heat storage, how can they be given as measures thereof? From Loeb et al 09 The 5-yr global mean CERES net flux from the standard CERES product is 6.5 W m-2, much larger than the best estimate of 0.85 W m-2 based on observed ocean heat content data and model simulations. [Stephen E Schwartz, United States of America]	Noted - The constraint is for global mean net radiation averaged from 2005-2010 (i.e., one number only). There is no constraint on the temporal variations in CERES net TOA radiation, which is what the "missing energy" discussion is all about. The temporal variability is determined by the CERES instrument only.
2-2762	2	156	1	156	7	If the trend is 5-year moving averages, the first and last 2 years should be absent. Why not a smoothing spline with uncertainties here, or a IRW trend with uncertainties? These curves will be somewhat more smooth. [Hans Visser, The Netherlands]	Noted - at the margins only one-sided averaging windows are used.
2-2763	2	157				Figure 2.15: the CRUTEM and GISS lines have very similar colours. GHCN and Berkeley are hard to distinguish too. Solution might be (1) try different colours (2) make the lines thicker on the graph and a lot thicker in the legend. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Colours will be optimized
2-2764	2	157				As the data are so close, might be useful to show differences also as a separate plot; might be possible thereby to discern systematic differences. [Stephen E Schwartz, United States of America]	Rejected. Plots in the FOD included difference series and were universally panned by reviewers and this simpler presentation style was enacted.
2-2765	2	158	1	158	10	I would like to reiterate something I said in the previous review round. The Figure showing land temperature changes has a vertical scale from -1.2 to +1.0 C. The oceanic data over the same interval only spans -0.9 to	Rejected. The range in each individual figure is selected to make this figure most readable. Numerical

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						+0.4 C. By stretching the scale it makes the two look like they have the same trend, whereas the Sea Surface Temperature trend is much smaller than that for the land temperatures. This should be illustrated clearly for the reader by keeping the vertical scale consistent, rather than making the two look artificially alike. [Ross McKitrick, Canada]	values of trends on the land and surface ocean are cited in corresponding tables. Furthermore, Figure 2.22 allows one to compare visually the magnitude of trends on the land and ocean.
2-2766	2	158	8	158	8	The phrase "informed, in particular, by night marine air temperature" does not make clear that the SST adjustments have been fitted to NMAT. [Elizabeth Kent, United Kingdom]	Editorial
2-2767	2	158				Figure 2.16: the orange and red lines are hard to tell apart, the blue and black are also hard to distinguish between. Either (1) try different colours or (2) make the lines thicker on the graph and a lot thicker in the legend. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account in the revision
2-2768	2	159				Figure 2.17: should read "Averages are computed over all times and locations where both ERI (and hull) and bucket measurements, but not necessarily buoy measurements, were available. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted:revised accordingly
2-2769	2	159				Figure 2.17: Kennedy et al. (2011c) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted: revised accordingly
2-2770	2	160	10			Merchant et al. paper is currently in press and available online: Merchant, C. J., O. Embury, N. A. Rayner, D. I. Berry, G. Corlett, K. Lean, K. L. Veal, E. C. Kent, D. Llewellyn-Jones, J. J. Remedios, and R. Saunders A twenty-year independent record of sea surface temperature for climate from Along Track Scanning Radiometers J. Geophys. Res., doi:10.1029/2012JC008400 [Christopher Merchant, United Kingdom]	Accepted. This will be updated.
2-2771	2	160				Figure 2.18: the Merchant et al. paper is now in press. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2770
2-2772	2	161				fig. 2.19 Title should read: "Global Sea Surface Temperature and Night Marine Air Temperature Timeseries" [Government of United States of America]	Accepted. Title has been modified accordingly.
2-2773	2	161				Figure 2.19: A think line indicating 0 °C could be helpful to understand the evolution of the shown anomalies. [Birgit Hassler, United States of America]	Accepted: figures were revised
2-2774	2	161				Figure 2.19: I would suggest making the lines thicker or choosing colours that are easier to distinguish (or both). The orange and red are similar, as are the black and blue. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted: figures were revised
2-2775	2	162	2	162	7	It would be useful to have a statement on what this graph shows - i.e. last three decades were warmer than preceding decades since 1850 [Government of Australia]	Rejected. Change instead made to the text that cites the figure. Double stating is overkill
2-2776	2	162				Figure 2.20: Several suggestions here. (1) don't use red and blue to denote temperatures above or below the essentially arbitrary average (2) de-emphasise the zero anomaly line by making it the same weight as all the other (3) simplify the legend (suggestions (1) and (2) will help with that). [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted
2-2777	2	162				Fig 2.20 I am surprised to see this absurd figure still in the SOD. No such figure appears in the cited paper Morice et al, or in any other published paper I am aware of , or in previous IPCC reports. Such a figure would be widely and rightly ridiculed as an attempt disguise the recent slow-down of warming. [Paul Matthews, United Kingdom]	Rejected. This figure has been used in several fora and the numbers are derived from the data presented in Morice et al. An equivalent plot appears in Arndt et al., 2010 (http://www.ncdc.noaa.gov/bams-state-of-the-climate/2009.php) so to state that it has no peer reviewed basis is also incorrect.
2-2778	2	162				Why plotted this way and not temp vs time as conventional? [Stephen E Schwartz, United States of America]	Noted. It is plotted this way for presentational purposes.
2-2779	2	163	4	163	5	Fig. 2.21: Clarity would be much increased by plotting HadCRUT4 in a contrasting colour to GISS. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2763
2-2780	2	163				Figure 2.21: orange and red are hard to tell apart. Thicker lines or more contrasting colours needed. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Taken into account. See response to 2-2763

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2781	2	164	1	164	1	Fig 2.22. GISS HAS TWO DATASETS< WITH @%)KM AND !@))KM DECORRELATION SCALE< i assume the latter was used. Is this correct? [Geert Jan van Oldenborgh, Netherlands]	Noted. Comment has been rendered indecipherable by excel. We use the operational GISS product version served from their website.
2-2782	2	164	2	164	10	HadCRUT and Giss are well-known. What is “Mlost”? [Hans Visser, The Netherlands]	Noted. MLOST is NCDC's product as is clearly stated in the text, tables and appendix.
2-2783	2	166	1	166	8	Some lines are very hard to see even in the high-resolution version. [Melissa Free, United States of America]	Taken into account. See response to 2-2763
2-2784	2	166				Legend for Fig 2.24 should give dataset versions. [Dian Seidel, United States of America]	Accepted
2-2785	2	168	1	169	6	It would be easier for the reader if the same colors were used for the same datasets in all these upper-air figures. [Melissa Free, United States of America]	Accepted
2-2786	2	168	1	169	6	The pressure level labels don't line up well with the symbols in the graph, making it hard to tell which level is which. [Melissa Free, United States of America]	Noted. Efforts have been made to increase clarity.
2-2787	2	170	1	170	1	For consistency with FAQ 2.1, Figure 2, should Figure 1 refer only to sea ice area rather than "Sea Ice Volume & Area"? [Francis Zwiers, Canada]	Accepted
2-2788	2	170	4			It is not clear what "repeated analyses" refer to here. Also, the time period for these trends is missing. [Dian Seidel, United States of America]	Noted. We have edited the caption to take account of these concerns. The trends are variable specific as is obvious from cross-referencing to the figure cited in the caption.
2-2789	2	170				Fig. FAQ 2.1: I don't understand the caption. Why repeated analysis, and why is independent highlighted? Givnen that this is an FAQ, couldn't this simply say: trends in different components and regions of the Earth expected in a warming world? [Reto Knutti, Switzerland]	Noted. We have simplified this caption in response to this and other comments.
2-2790	2	171	1	171	8	There is an error in the scale for the top left panel. The lower limit should be -1.0, not -0.1 [Ross McKitrick, Canada]	Accepted
2-2791	2	171	1	171	8	The figure is supposed to contain global indicators. The one in the 4th row, second column shows declining polar sea ice, but only includes Arctic sea ice extent. It should also include Antarctic sea ice data, in which case I expect the overall trend will be flat [Ross McKitrick, Canada]	Rejected. The rationale behind this choice and a mention of the Antarctic trend is given in the accompanying text.
2-2792	2	171	1	171	8	The Figure is created to emphasize the similarity of evidence warming across the different data sets. But some of that similarity is obtained by the fact that the scales are different. For instance, the sea surface and marine air temperatures go from -0.6C to +0.4C, which is only half the range of the land surface temperatures, which go from -1.0C to +1.0C. Since they are measuring the same thing, in principle, they should be shown on the same vertical axes. [Ross McKitrick, Canada]	Noted. We considered in response to FOD comments using different axis spacing and decided to retain the style used in the FOD.
2-2793	2	171	1	171	10	I don't see the Antarctic Sea Ice time series. This is an omission that can be readily criticized in other venues as a bias in the presentation of information. [John Christy, United States of America]	Taken into account. See response to 2-2791
2-2794	2	171	6	171	6	Please specify the "common period of recorrd". [Christian-D. Schoenwiese, Germany]	Rejected. The common period of record is panel dependent. This detail is given in the accompanying appendix material
2-2795	2	171				FAQ 2.1 Figure 2 - the caption under the figure should be replaced with the caption given in the main text. Not all the lines are "independent". [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. Caption consistency will be ensured in the final draft.
2-2796	2	171				FAQ 2.1 Figure 2 - this figure is used in the TS but not in the SPM. The SPM version has the advantage that it draws on individual series used in the chapters. However the SPM version is less comprehensive than the Chapter 2 FAQ/TS version. It would be best to combine the better aspects of each. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. We had a cross-chapter meeting on this issue to clarify which prioducts should contribute to each panel in the final draft.
2-2797	2	171				FAQ2.1 Figure 2: Appendix 2.A lists only 6 ocean heat content datasets but 8 sea level datasets. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted. With changes in the input data this has been revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2798	2	171				This is an enormously powerful figure. I am pleased that the data sets will be identified and the data made available. This should be a standard for the entire report. [Stephen E Schwartz, United States of America]	Noted
2-2799	2	172	1	172	2	Explain abbreviations in Figure legend [European Union]	Figures has changed
2-2800	2	172	2	172	8	I'm supprised to see the recontructed precipitation from Smith et al. is included in the land precipitation comparison, while the comprehensive data sets from Willmott and Mastruura (2009) and Chen et al. (2002, and updates) are not included. The CRU and GHCN (used similar gauges for the last 15 yrs) contain much fewer (<1500) raingauges than the GPCC, Willmott and NOAA CPC data sets (see Fig. S13, Sheffield et al. 2012, Nature), and thus are less reliable for the global and regional averages than the other data sets. One needs to examine the gauge coverages in comparing and commenting on the data different P sets [Aiguo Dai, United States of America]	Figures has changed
2-2801	2	172	2	172	8	Why these 'filtered trend'? Why not stochastic trends with uncertainties (cf. Visser and Petersen, CP 2012). [Hans Visser, The Netherlands]	Figures has changed
2-2802	2	172				Fig. 2.28 - it should be made clear that all are for land/islands apart from (I think) Smith et al. and GPCP which are ocean and land. [Richard Allan, United Kingdom]	Figures has changed
2-2803	2	172				fig. 2.28 Meaning of caption is ambiguous. Is it "over land areas for four bands and over global land?" Or is it "over land areas for four bands and over the globe (ocean included)?" [Government of United States of America]	Figures has changed
2-2804	2	172				Figure 2.29: Does this figure really provide an objective trend estimate? The number of stations is changing with time and also changing spatially. This will lead to different uncertainties and thus different weights of individual datapoints. I guess this is not considered in the analysis so far. What would be the expected impact? [Alexander Loew, Germany]	Figures has changed
2-2805	2	173	0			Figure 2.29: To ensure readability of the figure, there is a need to enlarge or improve resolution of maps so that "+" symbols are clearly visible [Government of Canada]	Figures has changed
2-2806	2	173	1	173	2	Figure 2.29, the shaded grids are not clear, and it seems there is data missing in many grids, I would suggest that some regional data and analysis would be added to the dataset with a more detailed description if possible, such as for China, South America and Africa. [Qingxiang Li, China]	Figures has changed
2-2807	2	173	4	173	4	Please give version numbers for all datasets used. Is the CRU dataset CRU TS 3.10.01? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2808	2	173	4	173	4	What is the criterion for leaving areas white? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2809	2	173		173		Figure 2.29 should be updated prior to publication - e.g. to end in 2011 rather than 2009 [Government of Australia]	Figures has changed
2-2810	2	173				Figure 2.29. The quality of this figure is not good [Government of Chile]	Figures has changed
2-2811	2	173				Fig 2.29 The poor quality of the figure does not make possible to distinguish any trend around the Mediterranean Sea, NE Canada and Oceania. [Government of France]	Figures has changed
2-2812	2	173				fig. 2.29 Caption states that most of the aerosol trends are significant. "most" is a very vague word. Should use the "+" symbols to indicate statistical significance at 90% level consistent with other regional trends figures in Chapter 2. If the "+" symbols were present, they were illegible - even in the high res version of the file. [Government of United States of America]	Figures has changed
2-2813	2	173				Figure 2.29: '+' in Figure are very hard to distinguish. [Birgit Hassler, United States of America]	Figures has changed
2-2814	2	173				Figure 2.29: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Figures has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2815	2	173				Fig. 2.29 I assume the units should be %/decade, not mm/decade, otherwise I cannot get the trends in Congo and Austrlia on the same scale. If not, please show relative trends in order to increase compatibility with the projection chapters. [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2816	2	173				Fig. 2.29 I cannot reproduce the CRU figure, did you use CRU TS 3.10.01? My trend plots for this dataset are available as trend_cru_ts31001_1901-2009.png and trend_cru_ts31001_1979-2009.png [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2817	2	173				Fig. 2.29 I cannot reproduce the GPCC figure, did you use v6? My trend plots for GPCC v6 are available as trend_gpcc_v6_1901-2009.png and trend_gpcc_v6_1979-2009.png [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2818	2	173				Fig.2.29 The gridd GHCN dataset is as far as I know only available as anomalies, how have the relative changes been computed? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2819	2	174	0			Figure 2.30: If available, it would be useful to provide a global map (e.g., from Simmons et al. 2010, Low-frequency variations in surface atmospheric humidity, temperature, and precipitation: Inferences from reanalyses and monthly gridded observational data sets, JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 115, D01110) showing spatial variation in trends of relative humidity as shown in the upper panel (a) for specific humidity trends. [Government of Canada]	Figures has changed
2-2820	2	174	3	174	9	A trend through the data would be very informative here! Especially for the middle panel. [Hans Visser, The Netherlands]	Figures has changed
2-2821	2	174	5	174	5	In the Figure above this line indications a) for upper panel and b) for lower panel are missing. [Christian-D. Schoenwiese, Germany]	Figures has changed
2-2822	2	174	6	174	6	Delete one "a". [Christian-D. Schoenwiese, Germany]	Figures has changed
2-2823	2	174				It is misleading to label b and c as "global" results, when they are just for land. [Dian Seidel, United States of America]	Figures has changed
2-2824	2	174				Figure 2.30: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Figures has changed
2-2825	2	174				Fig. 2.30 How can the ERA-interim re-anlaysis, which starts in 1979, be drawn in the years before this? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2826	2	174				Fig. 2.30 b,c pleaes add the word "land" to the title of the panels [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2827	2	174				Fig. 2.30 How has a land average been defined for HadCRUH when panel a shows a totral lack of observatiopns over Africa and large holes over South America and thepolar regions? Is Antarctica incliuded? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2828	2	174				Fig. 2.30a. Please mention the criterion for leaving grid boxes white. I get many fewer grid boxes with data when I demand at least 70% data like in other plots in this chapter. [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2829	2	174				Fig. 2.30a. Why did you not show the trend in the more complete, longer and more recent ERA-interim dataset, which is treated on equal footing in the plots below? I downloaded from data-portal.ecmwf.int the specifiic humidity at model level 60, which should correspond closely to surface specific humidity. The trend figure is attached as trend_erai_huss.png (can also easily be made at climexp.knmi.nl). It shows large drying trends over the Pacific (connected with the cooling there), over South America and the eastern Sahel. [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2830	2	174				Fig. 2.30b I cannot reproduce the ERA-analysis (interim?) global mean of specfiic humidity time series. I downloaded from data-portal.ecmwf.int the specifiic humidity at model level 60, which should correspond closely to surface specific humidity. Taking the average over all land points of this field gives similar interannual variability but no trend (see attached figure tsierai_huss_0-360E_-90-90N_n_5lanyr0.png, also obtained directly from climexp.knmi.nl). [Geert Jan van Oldenborgh, Netherlands]	Figures has changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2831	2	174				Given the lack of trend in the ERA-interim dataset, the dependence of the trend on begin and end year, the confidence in the observed increase in specific humidity should be downscaled to medium confidence at most (in spite of all the theoretical arguments that point to a trivial connection with temperature rise; the observational records are just too short.) [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2832	2	174				Fig.2.30a I cannot reproduce a few details in Fig. 2.30a using the latest version of HadCRUH from the www.metoffice.com/hadobs web site, such as the trends in Australia. Has the dataset been updated since this figure was prepared? Does it have a versiopn number to check this? [Geert Jan van Oldenborgh, Netherlands]	Figures has changed
2-2833	2	175				It is misleading to label top as "global" results, when they are just for ocean. [Dian Seidel, United States of America]	Figures has changed
2-2834	2	176	2	176	11	Lower panel: which trend model is used. Uncertainties? [Hans Visser, The Netherlands]	Figures has changed
2-2835	2	176				Box 2.4, Figure 1: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted.
2-2836	2	176				Box 2.4 Fig.1 As the decorrelation scale of high temperature events is comparable to the grid box scale the similarity is to be expected. It would be much more informative to show a variable in which this is not the case, such as daily precipitation, where the maximum of grib averaged precipitation may behave very different from the grid box average of maximum precipitation due to the maxima at different stations being uncorrelated and hence often on different days. [Geert Jan van Oldenborgh, Netherlands]	Noted. While we take the reviewers point, it is not possible at this time to compare the datasets for precipitation maxima as the HadGHCND dataset only contains temperature measures. However in the case of annual maxima of temperature the decorrelation length scales are not that large and we feel that this measure is sufficient to make the point.
2-2837	2	178				Fig. 2.33: It would be nice if the bottom panel could be redone with the same projection/style as the first two. [Reto Knutti, Switzerland]	TAKEN INTO ACCOUNT:
2-2838	2	178				Figure 2.33: Figure was checked for inconsistencies with own professional experience and competency. No relevant disagreements were detected. [Dirk Thielen, Venezuela]	Noted.
2-2839	2	178				Fig. 2.33a This figure would be much more clear if shown as a percentage vhange, so that the extratropics als would have some colours beyond thecrosses. [Geert Jan van Oldenborgh, Netherlands]	TAKEN INTO ACCOUNT:
2-2840	2	178				Fig. 2.33b: the only grid boxes visible with this colour scale indicate random events in very dry areas, whereas the significant boxes are all grey. Can teh figure be made more informative by chooing a relative measure of a non-linear colour scale? [Geert Jan van Oldenborgh, Netherlands]	TAKEN INTO ACCOUNT:
2-2841	2	179	2	179	10	Strange analysis of data: first 5-year running mean, and then an OLS straight lines. No uncertainties shown. Especially for the middle panel: a straight line is not logical. A flexible trend such as the Integrated Random Walk would do much better. [Hans Visser, The Netherlands]	TAKEN INTO ACCOUNT:
2-2842	2	179	4	179	8	Figure 2.34 : Please indicate the statistical significance of the linear trends depicted in the diagram. Moreover, please provide data source and reference for the Figure 2.34(c), landfalling typhoons in China. Please also check the trend shown in Figure 2.34(c). It seems to be different from the assessment provided by China Meteorological Administration in other publications that there is no significant trend in the landfalling typhoons in China. 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. - Lee, T. C., T. R. Knutson, H. Kamahori, and, M. Ying, 2012a: Impacts of Climate Change on Tropical Cyclones in the Western North Pacific Basin. Part I : Past Observations. Tropical Cyclone Res. Rev. 1, 213-230. http://tcrr.typhoon.gov.cn/EN/abstract/abstract30.shtml [Sai Ming Lee, Hong Kong, China]	Noted. Reference for data source is CMA (2011). China Climate Change Bulletin which is now added, Data shown is typhoon frequency (typhoons measured as intensity reaching 17.2 m/s). References by reviewer relate to data that analyses all tropical cyclones landfallng in China rather than just typhoons. Therefore data shown are correct. Moreover due to data issues prior to 1961 we have revised the figure to only show data after this date and up to 2011.
2-2843	2	179	8	179	9	"… trends of at least 5% significance." This is a sloppy formulation. Please use correct statistical langauge. [Ralf Weisse, Germany]	TAKEN INTO ACCOUNT:
2-2844	2	179				Figure 2.34: Are all 3 linear trends statistically significant? Please state in the caption. [Thomas Stocker/ WGI	TAKEN INTO ACCOUNT:

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						TSU, Switzerland]	
2-2845	2	181	1	181	1	I think it will be difficult for lay readers to discern evidence of changes in spread and shape from this particular presentation of the distributions of TN and TX. [Francis Zwiers, Canada]	TAKEN INTO ACCOUNT:
2-2846	2	181	1	181	8	This Figure's title is misleading. It claims the listed events are "happening everywhere" and the list includes Tropical cyclone frequency and intensity, Heavy precipitation, Cold nights, Heatwaves, Warm days/nights and droughts. But then a more careful study of the graph shows that 4 of the 7 items are stated with medium or low confidence, 2 of the 7 items are going down rather than up, and one is going up and down at the same time. In other words, its detailed content runs more or less opposite to the impression created by the headline. The heading should be changed to something neutral like "Geographic spread of selected changes in weather and climate indicators". [Ross McKitrick, Canada]	ACCEPTED: This figure has been significantly revised.
2-2847	2	181	4	181	6	I would be useful to include some additional details in the caption. FOr example, what region is depicted in FAQ 2.2, Fig 1? Mixing observations from climatologically different regions could distort whatever information there may be about changes in shape and spread, as would comparing distributions that are calculated from differing numbers of stations. I assume that units are degrees C. [Francis Zwiers, Canada]	Noted. This figure has been significantly revised.
2-2848	2	181				FAQ 2.2 Figure 1: Please provide units and labels for the horizontal and vertical axes of the graphs [David Wratt, New Zealand]	Noted. This figure has been significantly revised.
2-2849	2	182	0			FAQ 2.2, Figure 2: This figure is misleading as the global map, which dominates the figure, does not actually depict any of the changes or confidence in changes. The legend, which is superimposed on the map, actually relates to the arrows depicting change globally or on a regional level. The map in this instance does not add value to this figure or understanding changes. Consider a new layout to better connect the legend with the arrows. [Government of Canada]	ACCEPTED: This figure has been significantly revised.
2-2850	2	182	0			Figure FAQ2.2 Fig 2: The title of this image declares that heavy precipitation events are happening everywhere except the noted region. This conclusion seems stronger (and clearer) than statements in the SPM about changes in heavy precipitation (SPM page 4 lines 6-8 and Table SPM.1) where it says "it is likely that the number of heavy precip events has increased in more regions than it has decreased since the 1950s". While not inconsistent statements, here the impression is that heavy precip has increased most places, while in the SPM, the impression is of less uniform increases around the globe. [Government of Canada]	ACCEPTED: This figure has been significantly revised.
2-2851	2	182	0			Figure FAQ2.2 Fig 2: It is easy to miss that events described as exceptions in this figure (bottom icons) can be because of a change in confidence, not direction (sign) of change. This is confusing and also seems at odds with the title of the graphic which identifies icons listed above the map as events that are happening. Recommend only listing as exceptions regions where the sign of change is opposite. Heavy precip increases in central and eastern NA and heatwave increases in Europe are happening and are observed with stronger confidence; therefore, there placement as exceptions to these events happening is very misleading. They should be above the map. [Government of Canada]	ACCEPTED: This figure has been significantly revised.
2-2852	2	182	1	182	1	FAQ 2.2 Figure 2: I understand the purpose of this figure is as an easy-to-read qualitative description of changes in climate extremes, but I find a couple of aspects of the figure confusing. Firstly, I think it is unclear from the figure whether the tropical cyclones in East Coast Australia are decreasing in frequency or intensity. Secondly, I think the different colours used to indicate varying likelihoods are perhaps too similar to each other. In my opinion a table would provide a clearer explanation of what the figure is trying to show. [Andrew King, Australia]	ACCEPTED: This figure has been significantly revised.
2-2853	2	182	1	182	1	I find the titles on this plot a bit confusing. Without reading the details in the caption, you might jump to the conclusion, for example, that heatwaves are intensifying everywhere expept in Europe. A way to deal with this might be to replace the lower title with "Regional assessments that differ from the "global" assessments", although I recognize that's rather wordy. Also, the upper title seems misleading, or at least, has the potential to lead the reader to stronger conclusions than assessed. As indicated, there is low confidence that TC's have intensified - but that is a nuance that will be ignored by many when they use this graphic. Perhaps a better title would be "Global consensus of regional assessments" (I assume that's what this is). [Francis Zwiers, Canada]	ACCEPTED: This figure has been significantly revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2854	2	182	1	182	1	I do not think it is appropriate or helpful to combine confidence assessments and likelihood assessments into a single scale. The latter is quantified, the former is not. The latter can be used, in some circumstances, even when confidence is assessed to be medium, etc. [Francis Zwiers, Canada]	ACCEPTED: This figure has been significantly revised.
2-2855	2	182	2	182	2	Great to have a picture like this as a figure. The 'except in these regions' is however a little confusing. It could be clearer by showing what the actual trend is in that 'except for these regions' region, rather than what is not happening there. [Government of Australia]	ACCEPTED: This figure has been significantly revised.
2-2856	2	182				FAQ 2.2 Figure 2.2: is it necessary to show the map for whole the world. There is any sign or any other thing in it? [Rahimzadeh Fatemeh, Iran]	ACCEPTED: This figure has been significantly revised.
2-2857	2	182				FAQ2.2, Fig. 2 I don't understand the panels under "except in these regions". How do increases in heavy precip (central and eastern north america) and increases in heatwaves (Europe) not agree with the panels shown under "these events are happening everywhere"? [Government of United States of America]	ACCEPTED: This figure has been significantly revised.
2-2858	2	182				FAQ 2.2. Figure 2: Showing "Heat waves" but not "cold waves" appears to be biased. This should be amended to account for the corresponding cold waves in Europe and the severe 2009 Dzud in Mongolia. Etc. [David L. Hagen, United States of America]	Noted. This figure has been significantly revised.
2-2859	2	182				I find Fig. FAQ 2.2 Figure 2 completely confusing. First, what is the point of showing the map of the world? Second, the trend only "goes both up and down" for droughts, so why not be clear about that in the legend. Third, the notion that "These events are happening everywhere" makes no sense, as tropical cyclones, for example, don't happen much outside the tropics. This needs lots of work for inclusion in an FAQ. [Dian Seidel, United States of America]	Noted. This figure has been significantly revised.
2-2860	2	182				FAQ 2.2 Figure 2. The hurricane arraows are not compatible wkith the text, which says that there is no trend in the frequency of triopical cyclones and low confidence in a trend in the intensity. Given teh high decadal variability this should be changed to up-and-down arrows. [Geert Jan van Oldenborgh, Netherlands]	Noted. This figure has been significantly revised.
2-2861	2	182				FAQ 2.2 Figure 2. "These events are happening everywhere" I think there are regions without tropical cyclones. Heatwaves, heavy precipitation. [Geert Jan van Oldenborgh, Netherlands]	Noted. This figure has been significantly revised.
2-2862	2	182				FAQ 2.2 Figure 2. A lot more exceptions are givien in the text than shown in the figure, although the figure claims to be compreehesive "These changes are happening everywhere except in these regions: ...". This cannot be defended. [Geert Jan van Oldenborgh, Netherlands]	Noted. This figure has been significantly revised.
2-2863	2	182				FAQ 2.2, Figure 2: Some of the simplified interpretations in this Figure seem inconsistent with the chapter assessment, or are not found anywhere in the underlying chapter assessment. For example, 'medium confidence' in decreasing tropical cyclones in Australia can not be found anywhere in the chapter text. Also, a likely increase in heavy precipitation 'everywhere' seems unsupported or a considerably stronger statement than can be supported by the chapter text (increases in more regions than decreases). 'Low confidence' in droughts is never actually stated in the text either, you simply state that unlike the AR4, you do not have high confidence. SREX identified some areas with a medium confidence of changes in droughts, but these are not included here - is this because the assessment for these regions is no longer supported? [Thomas Stocker/ WGI TSU, Switzerland]	Noted. This figure has been significantly revised.
2-2864	2	182				FAQ2.2 Figure 2: I do not think the map of the world adds anything to this figure, and in fact it is initially confusing because the reader initially tries to relate the "likelihood legend" to the colours in this map, when the map colours are actually (I think) altitude indications. I suggest removing the map from the figure and using the resulting space to enlarge the little iconic picures of changes. [David Wratt, New Zealand]	Noted. This figure has been significantly revised.
2-2865	2	182				FAQ2.2 Figure 2: I found the "Except in these regions" heading in this figure misleading: When I first read it I thought you meant the events referred to (e.g. heatwaves) were NOT happening in the indicated regions. However mostly it is a difference in the likelihood of the changes which is being refered to, rather than a difference in the direction. I suggest you chnage the wording to something like "But there are some differences in likelihood in these regions" to go above the icons for North America and Europe, and "except there are differences in sign" above the Australia icon. [David Wratt, New Zealand]	Noted. This figure has been significantly revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2866	2	183	4	183	5	Fig 2.36: Need to justify the choice of half year periods. October in the extratropical NH is more like winter and April is more like summer from an atmospheric circulation perspective. If this change was made, does it increase the coherence of the trends? [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected - Nov-Apr was chosen because it is very widely used in climatology (e.g. Thomspon and Wallace, 1998)
2-2867	2	184				Fig 2.37 The chosen projection does not make possible to see the results over Europe [Government of France]	Rejected. A standard projection used for global maps in the AR5 is used.
2-2868	2	188				Figure 2, FAQ 2.2. Is there something missing on the map? I do not understand why is there, it looks like empty [Government of Chile]	Misplaced question: refers to FAQ2.2, not Box 2.5
2-2869	2	190	4	190	5	Fig. 2A2: Caption dates are inconsistent with those on the figure. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Accepted and so changed
2-2870	2	190	5			Fig 2.A.2 - It appears that the dates in the legend do not match the dates in the Figure. [Government of United States of America]	Accepted and so changed
2-2871	2	191	1	191	2	Figure 2 in FAQ 2.1 is well done. However, a choice seems to have been made to back all slopes to 45° or so, by tuning the axis range (and unit spacing) accordingly. As an example the figures seems to imply that warming rates over land are the same as over the ocean. Did the authors consider to use the same range and spacing for all temperature anomaly data, but to change the vertical extent of the figures to reflect the different warming rates? [Government of Iceland]	Noted. The time axis interval is identical on all panels. The y-axis spacing was considered in detail in response to FOD review comments and the FOD presentation style retained.
2-2872	2	191	4	191	6	Fig 2A6: Comments as Fig 2.36 - might be better to use Oct- Mar and Apr-Sept. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Rejected - Nov-Apr was chosen because it is very widely used in climatology (e.g. Thomspon and Wallace, 1998)
2-2873	2	210		211		References to be added: Ashok, K., S. K. Behera, S. A. Rao, H. Weng, and T. Yamagata (2007a), El Niño Modoki and its possible teleconnection, J. Geophys. Res., 112, doi:10.1029/2006JC003798 .(Note: The above reference has been already listed. Only the year needs to be changed to 2007a). [Government of India]	Rejected - see comment 2-2472
2-2874	2	210		211		Ashok, K., H. Nakamura, and T. Yamagata, 2007b: Impacts of ENSO and IOD events on the Southern Hemisphere storm track activity during austral winter, J. Climate, 20, 3147-3163. [Government of India]	Rejected - see comment 2-2483
2-2875	2	210		211		Ashok K., C.-Y. Tam, W.-J. Lee, 2009: ENSO Modoki impact on the Southern Hemisphere storm track activity during extended austral winter, Geophys. Res. Lett., 36, L12705, doi:10.1029/2009GL038847. [Government of India]	Rejected - see comment 2-2483
2-2876	2	210		211		Ashok, K., T. P. Sabin,P. Swapna, and R. G.Murtugudde (2012), Is a global warming signatureemerging in the tropical Pacific?, Geophys. Res. Lett., 39, L02701, doi:10.1029/2011GL050232. [Government of India]	Rejected - see comment 2-2472
2-2877	2	210		211		Bunge, L. and A. J. Clarke (2009), Verified Estimation of the El Niño Index Niño-3.4 since 1877, J. Clim., 22, 3979-3992. [Government of India]	Noted. - Is already in the reference list
2-2878	2	210		211		Dani, K.K., P. Ernest Raj, P. C. S. Devara, G. Pandithurai, S. M. Sonbawne,R. S. Maheskumar, S. K. Saha and Y. Jaya Rao (2012), Long-term trends and variability in measured multi-spectral aerosol optical depth over a tropical urban station in India, Int. J. Climatol., 32, 153-160, DOI: 10.1002/joc.2250. [Government of India]	References has been considered
2-2879	2	210		211		Deo, A.A., D.W. Ganer, G. Nair, 2011" Tropical cyclone activity in global warming Scenario, Natural Hazards, 59, November 2011, DOI 10.1007/s11069-011-9794-8, 771-786. [Government of India]	References has been considered
2-2880	2	210		211		Deser, C., A. S. Phillips, and M. A. Alexander (2010) Twentieth Century Tropical Sea Surface Temperature Trends Revisited. Geophys. Res. Lett., 37, L10701, doi:10.1029/2010GL043321. [Government of India]	Noted. - Is already in the reference list
2-2881	2	210		211		DiNezio, P., A. C. Clement, G. A. Vecchi, B. J. Soden, B. Kirtman and S. K. Lee (2009), Climate Response of the Equatorial Pacific to Global Warming, J. Clim. 22, 4873-4892. [Government of India]	References has been considered
2-2882	2	210		211		Dessler, A. E., and S. M. Davis (2010), Trends in tropospheric humidity from reanalysis systems, J. Geophys. Res.,115, D19127, doi:10.1029/2010JD014192. [Government of India]	References has been considered

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2-2883	2	210		211		Gurung, D. R., A. V. Kulkarni, A. Giriraj, K. S. Aung, B. Shrestha, and J. Srinivasan, 2011, Changes in seasonal snow cover in Hindu Kush-Himalayan region, The Cryosphere Discussions, 5, 755–777. [Government of India]	References has been considered
2-2884	2	210		211		Goswami B.N., Venugopal V., Sengupta D., Madhusoodanan M.S., Xavier Prince K., 2006: Increasing trend of Extreme Rain Events over India in a Warming Environment, Science, 314, 5804, 1 December, 1442-1445. [Government of India]	References has been considered
2-2885	2	210		211		Ghude, S. D., S. Fadnavis, G. Beig, S. D. Polade, and R. J. van der A (2008), Detection of surface emission hot spots,trends, and seasonal cycle from satellite-retrieved NO2 over India, J. Geophys. Res., 113, D20305, doi:10.1029/2007JD009615. [Government of India]	References has been considered
2-2886	2	210		211		Ghude S.D., Van Der A R.J., Beig G., Fadnavis S., Polade S.D, Satellite derived trends in NO2 over the major global hotspot regions during the past decade and their inter-comparison (2009), Environmental Pollution 157 1873–1878, doi:10.1016/j.envpol.2009.01.013 [Government of India]	References has been considered
2-2887	2	210		211		Hamza,V., Preethi B., Revadekar JV. 2012. Diurnal and Spatial Variation of Indian Summer Monsoon Rainfall by Tropical Rainfall Measuring Mission Rain Rate, Journal of Hydrology, dx.doi.org/10.1016/j.hydrol.2012.09.056. [Government of India]	References has been considered
2-2888	2	210		211		Kothawale D.R., Revadekar J.V., Rupa Kumar K, 2010. Recent trends in pre-monsoon daily temperature extremes over India. Journal of Earth System Science, 119, 51-65. [Government of India]	References has been considered
2-2889	2	210		211		Krishnan R, Sabin T, Ayantika DC, Kitoh A, Sugi M, Murakami H, Turner A, Slingo J, Rajendran K, (2012) Will the South Asian monsoon overturning circulation stabilize any further?, Clim Dyn, Doi:10.1007/s00382-012-1317-0]. [Government of India]	References has been considered
2-2890	2	210		211		Nakamura, H., and A. Shimpo, 2004: Seasonal variations in the Southern Hemisphere storm tracks and jet streams as revealed in a reanalysis dataset. J. Climate, 17, 1828–1844. [Government of India]	Rejected - see comment 2-2483
2-2891	2	210		211		Rao, B. R. S., D. V. B. Rao, and V. B. Rao (2004), Decreasing trend in the strength of Tropical Easterly Jet during the Asian summer monsoon season and the number of tropical cyclonic systems over Bay of Bengal, Geophys. Res. Lett., 31, L14103, doi:10.1029/2004GL01981. [Government of India]	References has been considered
2-2892	2	210		211		Revadekar J.V., Hameed S., Collins D., Manton M., Sheikh M., Borgaonkar H. P., Kothawale D. R., Adnan M., Ahmed A. U., Ashraf J., Baidya S., Islam N., Jayasinghearachchi D., Manzoor N., Premalal K. H., 2012. Impact of altitude and latitude on changes in temperature extremes over South Asia during 1971-2000. International Journal of Climatology, DOI: 10.1002/joc.3418, 1-11. [Government of India]	References has been considered
2-2893	2	210		211		Weng, H., K. Ashok, S. K. Behera, S. A. Rao, and T. Yamagata (2007), Impacts of recent El Niño Modoki on dry/wet conditions in the Pacific rim during boreal summer, Clim. Dyn., 29, 113–129, doi:10.1007/s00382-007-0234-0. [Government of India]	Rejected - see comment 2-2472