IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 3

Batch A (8 June 2006)

Notes

The following table of expert and government review comments are for consideration by the chapter author teams. They require a formal response to each comment from the team, and those responses will be archived.

Responding to review comments and record keeping

The chapter writing teams must consider all review comments and record an agreed response in the following table. This may be done by discussing the more general and substantive comments among the whole author team and then allocating responsibilities for responding to specific comments to the relevant authors. Note that responses should be understandable by someone scrutinizing the archived comments file after the report has been finalized.

Responses should generally be brief but clear. The following, or similar, styles of responses are suggested:

- Where the authors agree with the comment and have made a corresponding change:
  Accepted - without comment (e.g., in case of minor modifications) or with brief comments (e.g., where partially accepted)
- Where the authors agree with the comment and changes are not necessary or changes are made in a different section:
  Taken into account - with brief explanation (e.g. "see section X.Y")
- Where the comment does not require a specific change, or the issue is already dealt with in the draft:
  No change necessary – with brief explanation where appropriate (e.g. "covered in next paragraph", "covered in section X.Y")
- Where the authors do not agree with a suggested change:
  Rejected – always with a brief explanation (e.g. "insufficient literature to support this", "outside scope of section", "outside purview and competence of WG1", etc)
- Where dealing with very similar comments or a common thread of comments from one reviewer and a response has been given to the corresponding earlier comment(s):
  See comment X-Y.
- Only where it is clear that the reviewer is not suggesting a specific revision to the chapter.
  Noted - with or without comments.

It is recommended that you do not use names of individual members of the author team in the final responses to comments. I.e., responses should represent the entire chapter team. Where a comment involves another chapter please liaise with the authors of that chapter as appropriate but retain the comment and response in the comment file that you were sent. I.e., do not transfer comments.

Please provide the Technical Support Unit with the completed version of this document as a single electronic file by August 4, 2006.

No.	Batch	Page:line From	Page:line To	Comment	Notes
3-1	A	0:0	0:0	The title of Chapter 3 is not accurate. This chapter includes section on consistency across obsevations. A more descriptive title would be e.g. "Observations: Surface and Atmospheric changes and consistency across all observations". [Govt. of Finland (Reviewer's comment ID #: 2009-49)]	
3-2	A	0:0	0:0	Consistency of all observations is an important topic. It is discussed in the chapter, but not included in the Excutive Summary. Please, add "bullet point" on consistency across observations. [Govt. of Finland (Reviewer's comment ID #: 2009-50)]	
3-3	A	0:0	0:0	This Chapter is completely distorted, sustained by suppression or denigration of publications which challenge its conclusions. It depends upon a failure to permit any publicatons or arguments which challenge the virginity of the amalgamated surface record and a refusal to admit that it is upwardly biased by its unrepresentative distribution of thermometer readings, greatly inflkuenced by proximity to cities for the land-based measurements, and distorted by greater ship size and energy output, and by a transition f from measurement in buckets drawn from the sea to to engine intake measurements, for sea surface measurements. Important publications which prove upwards bias caused by these influences are downplayed or suppressed altogether. [VINCENT GRAY (Reviewer's comment ID #: 88-297)]	
3-4	A	0:0	0:0	This chapter is very long. It should be shortened wherever there is an opportunity. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-6)]	
3-5	A	0:0		The use of acronyms in the text is inconsistent - in some cases they are defined, in some cases they are not. The use of acronyms should follow consistent rules - viz, only included when used subsequently in a chapter, and defined at their first use in each chapter; each chapter, as for References, should include an acronym list, since many readers, particularly online, will treat each chapter as a standalone document.) [Govt. of Australia (Reviewer's comment ID #: 2001-175)]	
3-6	A	0:0		Various forms are used for specifying ranges of years - the forms '1901 to 2000', '1901-2000' and '1976/1977' are all used in different places in the report (the last of these only for consecutive years as far as I could tell). Need consistency. [Govt. of Australia (Reviewer's comment ID #: 2001-176)]	
3-7	A	0:0		Chapter 3 overall shows a comprehensive assessment of recent climate observations and research, and presents many informative and pertinent figures to illustrate the text. Along with reporting on the observational record, the authors have paid particular attention to describing the underlying mechanisms which govern climate response, thus providing essential background material for Chapter 9. However, in addressing the relevant topics, a number of sections have gone beyond the subject matter required in an assessment of	

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				current and new knowledge related to global climate change and in parts has strayed into the domain of attribution (Chapter 9). Some of the text expounds at length on matters relating to basic climate understanding. To what extent does this IPCC Assessment need to fulfil such a didactic purpose - one that would normally be done more effectively and comprehensively in a well-written text book on, for example, climate and large-scale circulation?. [Govt. of Australia (Reviewer's comment ID #: 2001-177)]	
3-8	A	0:0		There are also specific examples given of localised changes in a single country or small region which are not obviously placed within the more general context of larger hemispheric or global domains and thus provide little insight into global climate change. If an example using a particular geographic region is given, it needs to have some relevance to the larger global picture. Without the larger context, such isolated examples are the spatial equivalent of assigning the occurrence of a single severe event to climate change. Further, the citing of too many local examples detracts from the global picture, and results in an unnecessary number of references. A balance may be difficult to achieve but is worth striving for. See also next 2 comments. [Govt. of Australia (Reviewer's comment ID #: 2001-178)]	
3-9	A	0:0		References should be limited to those adding substantial new information and be balanced geographically. There is a sense that some references have been added simply because they too addressed some particular topic - better to cite only those that that provided the key insight. These and following suggestions offer opportunities for paring the text, in particularly Sec 3.6. A more tightly written chapter will enable the reader to focus on those aspects that inform on recent breakthroughs and observational findings, and that contribute to a better understanding of climate change and its inextricable links with the natural variability of climate. [Govt. of Australia (Reviewer's comment ID #: 2001-179)]	
3-10	A	0:0		There are instances in the chapter where local exceptions to the global result are given; in some cases, multiple examples. This has the effect of 1) highlighting exceptions rather than the rule, 2) giving undue importance to forcing factors quite separate to that on the global scale. Such exceptions are typically cited in scientific papers with discussion on why they are exceptions, and hence references to the relevant papers should serve to cover these points. [Govt. of Australia (Reviewer's comment ID #: 2001-180)]	
3-11	A	0:0		Linear trends are frequently used in the text to describe recent changes in various climate variables. While it is recognised that climate change is often best visualised and comprehended by simple linear trends, they do have a tendency to oversimplify what is happening and, in some instances, distort the true picture - particularly with a highly	

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				variable parameter such as rainfall. Whenever a record has a one or more significant abrupt changes, a linear trend is a poor model. While this is recognised and even stated explicitly in the text, the limitations of this form of analysis should be made clear whenever it is used on a record for which it is marginally relevant or worse. [Govt. of Australia (Reviewer's comment ID #: 2001-181)]	
3-12	A	0:0		There are several mentions of the PDO/IPO and its phases throughout the chapter, and it receives considerable attention in Box 3.4. As the science still cannot explain what drives the IPO/PDO, and there have been several suggestions that the IPO/PDO is simply a statistical artefact of ENSO, it would seem preferable to qualify its significance until further evidence is compiled. At the very least, recognition of the PDO/IPO should be given to the fact that physical mechanisms driving this feature have yet to be found. [Govt. of Australia (Reviewer's comment ID #: 2001-182)]	
3-13	A	0:0		Terminology: The use of some terms such as 'likely', 'very likely' etc is sometimes not consistent with the precise definitions for these terms adopted by the IPCC. The text should always aim to use these precise terms whenever there is some uncertainty. Otherwise the text should indicate that the level of uncertainty is indeterminable. Introduction of undefined vague terms as 'probably' or 'considerable uncertainty' does nothing to help the reader. Some examples are cited in specific comments. [Govt. of Australia (Reviewer's comment ID #: 2001-183)]	
3-14	A	0:0		While the figures add greatly to the information provided in the chapter, attention needs to be given to consistency in scales and shading, especially where charts compare changes over different time periods. [Govt. of Australia (Reviewer's comment ID #: 2001-184)]	
3-15	A	0:0		In general I appreciate the work that obviously has been done by the authors to consider the comments of the reviewers. I am convinced that this has increase the value of chapter 3 which really is a fundamental and essential reference for the recent state of knowledge on past climate variability and change. There is only one shortage left - but I know that the authors are not to blame for it, but the decisions drawn at the respective November-2003-WG1-session in Vienna, where I had the impression that a clear description and discussion about the remaining uncertainties in AR-4 WG1 was not liked and shall be suppressed. As a consequence, I think that some parts of chapter three are not in balance with others in respect to the existing knowledge and data basis. A respective passage or chapter only devoted to remaining uncertainties and a clear definition of the resulting research needs for the future would have helped the reader to understand these unbalances. I give only one example in the next line and leave it to the author's team and IPCC in general to reflect this shortage in respect to AR-5 perhaps. [Reinhard Böhm (Reviewer's comment ID #: 23-1)]	

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3-16	A	0:0		I have compared the second draft of the Chapter 3 with the first draft and, I find the report improved, more accurate and presenting the findings more clearly. However, the no. of pages was not reduced. Personally, I consider the information provided is relevant and necessary for the clarity of the content. [Constanta Emilia Boroneant (Reviewer's comment ID #: 26-1)]	
3-17	A	0:0		The Chapter is generally well written and appropriately structured [Chris Folland (Reviewer's comment ID #: 71-1)]	
3-18	A	0:0		There are very few/or even no references to Russian, Chinese, Japanese or French journals. [Govt. of France (Reviewer's comment ID #: 2010-24)]	
3-19	A	0:0		The authors have achieved a great success to assess comprehensively and in a balance way the important advances and developments on the observations of surface and atmospheric climate changes since the TAR. This chapter well reflects the current state of scientific understanding of the related issues. Congratulation for the excellent work! [Qiang Fu (Reviewer's comment ID #: 78-4)]	
3-20	A	0:0		Spencer et al. (2006) questioned the retrievals of tropospheric temperature trends from MSU T2 and t4. The strong apparent sensitivity of the weights reported by Spencer et al. is caused by their use of different data sets in the regression (e.g., the regression between the satellite observed T2 and T4 and the LKS tropospheric temperatures), which has no bearing on the robustness of the Fu et al. retrieval algorithm (Johanson and Fu 2006, J. Climate, in press). [Qiang Fu (Reviewer's comment ID #: 78-12)]	
3-21	A	0:0		A crucial publication is McKitrick, R and P.J Michaels 2004 "A test of corrertions of extraneous signals in gridded surface temperatuure data" Climate Research Vol 26 pages 159-173. This paper shows that the surface record possesses a significant upwards bias from population size, coal usage, and the use of incomplete data. Another important publication, mentioned in the Chapter , was Peterson, TC, 2003, The author carried out a complex procedure called "homogeneity adjustment" to correct the temperature record of the contiguous United States, and ended with a record that showed very little net increase. The claimed absence of a difference between urban and rural sites is not strictly true as it was initially very large (0.31°C per decade), but this reduced to 0.04°C after other corrections were made. [VINCENT GRAY (Reviewer's comment ID #: 88-298)]	
3-22	A	0:0		The application of the technique of "homogeneity adjustment in China gives a "corrected" record with negligile temperature change since 1900 (Zhao, Z, Y Ding, Y Luo, and S Wang, 2005 Acta Meteorologica Sinica Vol 19 pages 389-400).It would seem likely that if a similar correction procedure were applied to the entire surface record most of the	

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				supposed "surface warming":would disapear [VINCENT GRAY (Reviewer's comment ID #: 88-299)]	
3-23	A	0:0		The 3.1 and 32. sections of current version lacks powerful, objective comments and summary to delive messages to the readers. Namely, these sections are a bunch of collected resutls/papers contributed by each author and reviewer, but in many places no concise comemnts which are understandable to public to give summary of results. [Menglin Jin (Reviewer's comment ID #: 118-1)]	
3-24	A	0:0		too many references for some author. I suggest each chapter doesn't refer the same scientist's paper more than 3 - in particular, some authors just publish one topic in various paper, which is not necessady to refer. [Menglin Jin (Reviewer's comment ID #: 118-3)]	
3-25	A	0:0		Executive summary is very well written [Menglin Jin (Reviewer's comment ID #: 118-4)]	
3-26	A	0:0		In comparison with the first draft this second-order draft has an evident improvement. For all that, I consider in some of paragraphs there are too many references and for reader is a little difficult to discern among them which are essential for the respectively topic. [ILEANA MARES (Reviewer's comment ID #: 161-1)]	
3-27	A	0:0		The title of this chapter is ambiguous. Does "surface" refer to the union of land surface and ocean surface? If so, does it refer to the state of the atmosphere at those surfaces? If so, then the "surface" part of the title is redundant. If not, then it must refer to the ocean and the land themselves, in which case the ocean part would overlap with the ocean chapter. Is chapter 3 rather meant to cover "Atmospheric and Land Climate Change" observations? In deciding how to address my questions, one should keep in mind that land is not a surface (two-dimensionsal, no volume or mass), but rather a mass. [P.C.D. Milly (Reviewer's comment ID #: 179-19)]	
3-28	A	0:0		As a general comment, sometimes I found it difficult to follow the text due to the high number of acronyms used in this chapter (see section 3.4.1.5. -Page 29- as an example) [Pedro Ribera (Reviewer's comment ID #: 213-7)]	
3-29	A	0:0		General comment: this chapter is often quite difficult to read. One of the reasons is that for many of the fields (radiation, clouds, precipitation) the observations are quite equivocal. That in itself would make it difficult, but the presentation does not help. Often paragraphs start off with a definitive statement about the direction of change of a parameter, and then, either in the next paragraph or sometimes even in the same one, conflicting evidence is provided. One has to wait until the summary to disentangle the diverse claims. It would be better if the opening sentence mentioned that there is conflicting evidence for changes, and then, modestly, provide examples of the different results. To a good extent this is what is done in chapter 8 with the model results, and it	

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				helps make that chapter much easier to read. A few examples of this are given in the following comments. 4 [David Rind (Reviewer's comment ID #: 214-18)]	
3-30	A	0:0		This entire chapter is marred by a theme of "increasing drought" that occurs throughout. This conclusion derives from the study by Dai et al. 2004 that used the PDSI. In some places the caveat has been added that drought increased "according to the PDSI". There is little doubt that the calculations (not observations) by Dai et al. 2004 show increasing drought but the problem is that this research used the Thornthwaite method to calculate potential ET. As was noted in the literature streching back to the 1950s, and noted in the text (see Box 3.1), the Thornthwaite approach calculates potential ET using only air temperature. The better approach is to use a Penman-style method (as noted in Box 3.1) or pan evaporation measurements as a measure of potential evaporation. The fact that pan evaporation is declining (as noted in the chapter), as is Penman based estimates (e.g. Chen et al. 2005, Climate Research, 28: 123-132) shows that on average, potential ET is declining. However, if the Thornthwaite approach is used, potential ET will increase because of increasing air temperature. The net effect is that Penman or pan based estimates of potential ET would give a general reduction in drought. The opposite of the conclusion in the draft. How different would the draft read if it said "a general world wide increase in drought" using estimates of potential ET based on the Thornthwaite approach that we know are wrong (e.g. Chen et al 2005) "but a world wide reduction in droughts" using standard measures of potential ET. [Michael Roderick (Reviewer's comment ID #: 218-1)]	
3-31	A	0:0		Having provided "expert review" comments on the zeroth and first order drafts, I find this second order draft to be a substantial improvement that is largely responsive to my earlier comments. The authors have made considerable and commendable efforts to be comprehensive, clear, and as concise as possible. [Dian Seidel (Reviewer's comment ID #: 231-5)]	
3-32	A	0:0		There is inconsistency in the detail with which placenames are identified - for example, 'Phoenix' at 3-19, line 39, but 'Atlanta, Georgia (United States)' at 3-20, line 6. I would suggest that placenames be used alone if they are used to refer to a place which most readers could be expected to have heard of, or with a country otherwise. [Blair Trewin (Reviewer's comment ID #: 266-2)]	
3-33	A	0:0		Throughout the chapter, results of linear trend analyses are presented that include estimates of statistical significance. In two specific sections of the chapter (page 3-9, lines18-22 and page 3-116, lines 53-56), the comment is made that the statistical significances of trends in variables estimated using Restricted Maximum Likelihood regression (REML) -- which is the method used within the report -- are likely to be	

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				overestimated; with citations given for Zheng and Basher, 1999 and Cohn and Lins, 2005. On page 3-116, lines 55-56, after acknowledging that this problem stems from the presence of long-term persistence in the underlying climatic processes, the report then states "Nevertheless, the results depend on the statistical model used, and more complex models are not as transparent and often lack physical realism." Indeed, the results do depend on the model used and, as pointed out by Cohn and Lins, 2005, simple models (like REML) do not capture the complexity of long-term persistence -- that's why results based on the use of simple models are in error. The comment that "more complex models are not as transparent and often lack physical realism" contradicts the central point of Cohn and Lins, 2005. If long-term persistence exists within climatic processes, and the 4AR draft says that it does (page 3-116, lines 53-54), then a more complex model, such as that used by Cohn and Lins (2005) MUST be used to estimate statistical significance. This is not a matter of subjective model choice but, rather, of selecting a model that can be demonstrated as capturing the inherent behavior of the process in question. REML, and all other simple linear models, do not capture the observed temporal behavior of land surface temperature, sea surface temperature, precipitation, and any other hydro-climatic variable. The 4AR draft is reporting statistical significances that are known to be gross overestimates. To address this problem, the authors have two choices. One is to recalculate the statistical significance estimates of all variables for which significance is currently reported using a procedure such as Cohn and Lins' (2006) Adjusted Likelihood Ratio Test that is specifically designed for use with data exhibiting long-term persistence. Alternatively, the report could retain all of the current information regarding trend magnitude (which Cohn and Lins document as being insensitive to the method used to estimate it), but remove all reference to statistical significance -- in text, tables and figures. Indeed, the latter option may be desirable because, as noted by Cohn and Lins, "it may be preferable to acknowledge that the concept of statistical significance is meaningless when discussing poorly understood systems." [Govt. of United States of America (Reviewer's comment ID #: 2023-132)]	
3-34	A	0:0		Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales. [Govt. of United States of America (Reviewer's comment ID #: 2023-133)]	
3-35	A	0:0		Use of "likely" and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of	

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				certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: "likely", "caused", "confidence", "attribution". [Govt. of United States of America (Reviewer's comment ID #: 2023-134)]	
3-36	A	0:0		Chapter 3 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. The discussion on "Mechanisms for longer scale variability" in Section 3.6 seems like a discussion of attribution or speculation, not adequately supported by references. It seems unsuited for the observations section of the assessment. It is more appropriate for Chapter 9 on "understanding and attribution". These discussions of attribution have extended the length of the observation chapters and lead to an uneven presentation. Strongly recommend removing these discussions, or if appropriate, move them to Chapter 9. Also strongly recommend a substantial shortening of the Chapter 3, 4 and 5 bundle in order to make them more even in presentation, as well as more focused, and improve the ease of reading. [Govt. of United States of America (Reviewer's comment ID #: 2023-135)]	
3-37	A	0:0		There are a variety of positions presented in Chapter 3 on some of the large-scale coherent patterns of the atmosphere, such as the AMO discussions. Recommend a thorough review of the use of these terms throughout Chapters 3, 4, and 5 to improve the consistency in the discussion. [Govt. of United States of America (Reviewer's comment ID #: 2023-136)]	
3-38	A	0:0		A preponderance of comments received on Chapter 3 was concerned with a general weakness regarding coverage of the water cycle. The authors should evaluate the treatment of hydrology and the water cycle to improve its presentation regarding atmospheric observations. [Govt. of United States of America (Reviewer's comment ID #: 2023-137)]	
3-39	A	0:0		This chapter is often quite difficult to read. One of the reasons is that for many of the fields (radiation, clouds, precipitation) the observations are quite equivocal. That in itself would make it difficult, but the presentation does not help. Often paragraphs start off with a definitive statement about the direction of change of a parameter, and then, either in the next paragraph or sometimes even in the same one, conflicting evidence is provided. One has to wait until the summary to disentangle the diverse claims. It would be better if the opening sentence mentioned that there is conflicting evidence for changes, and then, modestly, provide examples of the different results. To a good extent this is what is done in Chapter 8 with the model results, and it helps make that chapter much easier to read.	

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-138)]	
3-40	A	0:0		The fundamental organization of WG1 and Chapter 3 on observed changes fails to recognize that hydrologic changes are one of the most important geophysical response variables and indicators of climate change. There are chapters on sea-level rise and on snow, ice, and frozen ground but not for hydrologic changes. [Govt. of United States of America (Reviewer's comment ID #: 2023-139)]	
3-41	A	0:0		Timing of Streamflow – Western USA Aguado et al. 1992, J. Climate 5:1468-1483. [Govt. of United States of America (Reviewer's comment ID #: 2023-140)]	
3-42	A	0:0		Timing of Streamflow – NW USA Cayan et al., 2001, Bull. Amer. Met. Soc. 82:399–416. [Govt. of United States of America (Reviewer's comment ID #: 2023-141)]	
3-43	A	0:0		Timing of Streamflow - California Dettinger, & Cayan. 1995. J. Climate 8:606-623. [Govt. of United States of America (Reviewer's comment ID #: 2023-142)]	
3-44	A	0:0		Timing of Streamflow Dettinger & Diaz J. Hydrometeor. 2000, 1, 289-310. [Govt. of United States of America (Reviewer's comment ID #: 2023-143)]	
3-45	A	0:0		Timing of Streamflow - New England Hodgkins et al. 2003 J. Hydrol. 278:242-250. [Govt. of United States of America (Reviewer's comment ID #: 2023-144)]	
3-46	A	0:0		Timing of Streamflow – SW Canada Leith & Whitfield. 1998. Can. Water Resour. J. 23:219-230. [Govt. of United States of America (Reviewer's comment ID #: 2023-145)]	
3-47	A	0:0		Timing of Streamflow – Lena River, Siberia Yang et al. 2002, J. Geophys. Res., 107(D23), 4694, doi:10.1029/2002JD002542 [Govt. of United States of America (Reviewer's comment ID #: 2023-146)]	
3-48	A	0:0		Timing of Streamflow – West-Central Canada Burn 1994. J.Hydrol. 160:53–70. [Govt. of United States of America (Reviewer's comment ID #: 2023-147)]	
3-49	A	0:0		Timing of Streamflow – Fraser River Canada Morrison et al. (2002) J. Hydrol. 263: 230-244 [Govt. of United States of America (Reviewer's comment ID #: 2023-148)]	
3-50	A	0:0		Timing of Streamflow NW USA Stewart et al. 2004. Climatic Change 62:227-232 [Govt. of United States of America (Reviewer's comment ID #: 2023-149)]	
3-51	A	0:0		Timing of Streamflow – Western North America Stewart et al. 2005. J. Climate 18: 1136-1155 [Govt. of United States of America (Reviewer's comment ID #: 2023-150)]	
3-52	A	0:0		Timing of Streamflow – Hudson Bay Region Gagnon & Gough. 2002.Can. Water Resour. J. 27: 245–262. [Govt. of United States of America (Reviewer's comment ID #: 2023-151)]	
3-53	A	0:0		Timing of Streamflow – Eastern USA Czikowsky et al. 2004 J. Hydromet. 5:974-988	

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-152)]	
3-54	A	0:0		Timing of Streamflow – Mackenzie Basin Aziz and Burn (In Press) J. Hydrol. [Govt. of United States of America (Reviewer's comment ID #: 2023-153)]	
3-55	A	0:0		Timing of Streamflow – Liard Basin Burn et al. 2004 Hydrol. Sci. J. 49:69-83 [Govt. of United States of America (Reviewer's comment ID #: 2023-154)]	
3-56	A	0:0		Timing of Streamflow - Mackenzie Woo & Thorne 2003 Arctic 56:328-340 [Govt. of United States of America (Reviewer's comment ID #: 2023-155)]	
3-57	A	0:0		Timing of Streamflow – S. British Colombia, Canada Cunderlik, & Burn, 2004. J. Hydrologic Engrg. 9:246-256. [Govt. of United States of America (Reviewer's comment ID #: 2023-156)]	
3-58	A	0:0		Timing of Streamflow - Mackenzie Burn et al. 2004, Can. Water Resour. J. 29:283-298 [Govt. of United States of America (Reviewer's comment ID #: 2023-157)]	
3-59	A	0:0		Timing of Streamflow - Churchill-Nelson Westmacott & Burn, 1997 J. Hydrol. 202, 263-279. [Govt. of United States of America (Reviewer's comment ID #: 2023-158)]	
3-60	A	0:0		Timing of Streamflow – 42 Rivers Central Canada Dery et al. 2005 J. Climate 18: 1540-1557 [Govt. of United States of America (Reviewer's comment ID #: 2023-159)]	
3-61	A	0:0		Timing of Streamflow NW USA Regonda (2005) J. Clim. 18:372-384 [Govt. of United States of America (Reviewer's comment ID #: 2023-160)]	
3-62	A	0:0		Decreases in Streamflow Fu et al., InPress, Climatic Change. [Govt. of United States of America (Reviewer's comment ID #: 2023-161)]	
3-63	A	0:0		Decreases in Streamflow (Summer) Leith & Whitfield. 1998. Can. Water Resour. J. 23:219-230. [Govt. of United States of America (Reviewer's comment ID #: 2023-162)]	
3-64	A	0:0		Decreases in Streamflow (Summer) Prowse & Conly. 1998. Hydrol. Proc. 12:1589-1610. [Govt. of United States of America (Reviewer's comment ID #: 2023-163)]	
3-65	A	0:0		Decreases in Streamflow – parts of China Tao et al. 2003 Agricultural For. Met. 118:251-261 [Govt. of United States of America (Reviewer's comment ID #: 2023-164)]	
3-66	A	0:0		Decreases in Streamflow – Yellow River Jiongxin, X., 2005. Environ. Manage. 35:620 - 631 [Govt. of United States of America (Reviewer's comment ID #: 2023-165)]	
3-67	A	0:0		Decreases in Streamflow – 42 Rivers Central Canada Dery et al. 2005 J. Climate 18: 1540-1557 [Govt. of United States of America (Reviewer's comment ID #: 2023-166)]	

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3-68	A	0:0		Decreases in Streamflow to Lake Chad (Charli/Logone River Systems) Coe, M.T., and J.A. Foley. 2001. J. Geophys. Res. 106:3349-3356. [Govt. of United States of America (Reviewer's comment ID #: 2023-167)]	
3-69	A	0:0		Decrease in Lake Level – Lake Chad Coe, M.T., and J.A. Foley. 2001. J. Geophys. Res. 106:3349-3356. [Govt. of United States of America (Reviewer's comment ID #: 2023-168)]	
3-70	A	0:0		Increases in Streamflow - USA Hubbard et al. 1997 Proc. IAHS Publ. No. 226 [Govt. of United States of America (Reviewer's comment ID #: 2023-169)]	
3-71	A	0:0		Increases in Streamflow - Arctic Lammers et al. 2001 J. Geophys. Res., 106(D4), 3321-3334 [Govt. of United States of America (Reviewer's comment ID #: 2023-170)]	
3-72	A	0:0		Increases in Streamflow - Global Labat et al. 2004 Adv. In Water Resour. 27: 631-642 [Govt. of United States of America (Reviewer's comment ID #: 2023-171)]	
3-73	A	0:0		Increases in Streamflow - USA Lins & Slack. 1999. Geophys. Res. Letters 26:227-230. [Govt. of United States of America (Reviewer's comment ID #: 2023-172)]	
3-74	A	0:0		Increases in Streamflow - USA McCabe & Wolock 2002. Geophys. Res. Lett. 2002 29(24), 2185, doi:10.1029/2002GL015999 [Govt. of United States of America (Reviewer's comment ID #: 2023-173)]	
3-75	A	0:0		Increases in Streamflow - Arctic Peterson et al., 2002. Science 298:2171-2173. [Govt. of United States of America (Reviewer's comment ID #: 2023-174)]	
3-76	A	0:0		Increases in Streamflow – Central USA Mauget 2004 Climatic Change 63:121-144. [Govt. of United States of America (Reviewer's comment ID #: 2023-175)]	
3-77	A	0:0		Increases in Streamflow - USA Groisman et al. 2001. Bull. Amer. Met. Soc. 82:219-246. [Govt. of United States of America (Reviewer's comment ID #: 2023-176)]	
3-78	A	0:0		Increases in Streamflow - Greenland Haq et al. (2002) XXII Nordic Hydrological Conference 2002, NHK/NHC [Govt. of United States of America (Reviewer's comment ID #: 2023-177)]	
3-79	A	0:0		Increases in Streamflow – Major Rivers USA Walter et al. 2004. J. Hydrometeorlogy 5:404-408 [Govt. of United States of America (Reviewer's comment ID #: 2023-178)]	
3-80	A	0:0		Increases in Streamflow - Baspa River Basin, Himalaya Region Kulkarni et al. (2003) Intl. Arch. Photogramm. Remote Sensing Spatial Infor. Sci. 34:1265-1269 [Govt. of United States of America (Reviewer's comment ID #: 2023-179)]	
3-81	A	0:0		Increases in Streamflow – Former USSR Georgievsky et al. 1996 Russian Meteorol. Hydrol. 11:66-74 [Govt. of United States of America (Reviewer's comment ID #: 2023-180)]	

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3-82	A	0:0		Increases in Streamflow - La Plata Basin, South America Berbery et al. (2002) J. Hydrometeorlogy 3:630-645 [Govt. of United States of America (Reviewer's comment ID #: 2023-181)]	
3-83	A	0:0		Increases in Streamflow – parts of China Tao et al. 2003 Agricultural For. Met. 118:251-261 [Govt. of United States of America (Reviewer's comment ID #: 2023-182)]	
3-84	A	0:0		Increases in Streamflow – Hudson Bay Gagnon & Gough. 2002.Can. Water Resour. J. 27: 245–262. [Govt. of United States of America (Reviewer's comment ID #: 2023-183)]	
3-85	A	0:0		Increases in Streamflow – Mackenzie R Aziz and Burn (In Press) J. Hydrol. [Govt. of United States of America (Reviewer's comment ID #: 2023-184)]	
3-86	A	0:0		Increases in Streamflow - Sweden Birsan et al. (2005) J. Hydrol. 314: 312–329 [Govt. of United States of America (Reviewer's comment ID #: 2023-185)]	
3-87	A	0:0		Increases in Streamflow – South America Garcia & Mechoso. 2006. Hydrol. Sci. J. 50:459-478. [Govt. of United States of America (Reviewer's comment ID #: 2023-186)]	
3-88	A	0:11	0:15	The surface cloud observations have a long history of documented biases (eg. Less Cirrus during new moon; no middle and high clouds when observer is obscured by haze and low cloud, etc. The "random-overlap" assumption of Norris is a poor one during the passage of various lower and upper tropospheric phenomena. To be fair it should be noted that the ISCCP of WCRP was reviewed and approved in the late 1970's with its principle objectives to detect the regional and interannual variability of clouds - not trends – in global or regional. Experiemental design is important and hundreds of journal papers have been published addressing the original objectives. Today we attempt to retrofit global trend analyses into the experiment and may be successful – given 3 to 5 years more research at the current LOE. Comments about "ISCCP spurious variability" are premature (line 43). Reconciliation among the cloud observations from satellites, from the surface, and from surrogate inferences of clouds (surface or satellite radiation measurements) will be reconciled and attention to this issue should be noted by IPCC. [Thomas Vonder Haar (Reviewer's comment ID #: 278-3)]	
3-89	A	2:12	2:12	I think paragraph 3.8.2 must be reformulated, because it is not clear and it is not compatible with the title of the main paragraph 3.8. [ILEANA MARES (Reviewer's comment ID #: 161-2)]	
3-90	A	3:0	6:	This chapter covers a very wide variety of observed parameter related to climate study. Overall it does a good job. However, a brief paragraph in the Introduction (p3-6, sec 3.1)	

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				should be added to note the "maturity" of the variety of parameter analyses. For example, Global Cloud Climatologies (eg. ISCCP) are very promising, but in a very preliminary state of analysis info far as "ternds" are concerned. Thi cloud question is still "open". GEWEX, WCRP are currently sponsoring detailed cloud assessments. These include a critical examination of the cloud-free "background" upon which down-viewing satellites depend; as well as a review of the representativeness of both old (manual) and new (manual/automatic) surface-based observations. The present analyses of global cloud amount, type, vertical profile, physical characteristics may be compared o the analyses of surface and atmospheric temperature data about 10 years ago. In turn, each of the variables discussed in section 3 have a greater or lesser maturity - and, if possible, this should be noted for the reader. [Thomas Vonder Haar (Reviewer's comment ID #: 278-2)]	
3-91	A	3:0		Figure 3.5.3. Reduce letter size of 'Adapted from' [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-12)]	
3-92	A	3:0		Figure 3.5.3. Reduce letter size of 'Adapted from' [Govt. of Spain (Reviewer's comment ID #: 2019-72)]	
3-93	A	3:1	5:50	The Executive Summary should serve to highlight the major findings of the chapter but it has not done this as well as it might. Several points contain unnecessary details that are could be left in the main text. [Govt. of Australia (Reviewer's comment ID #: 2001-185)]	
3-94	A	3:1		For policymakers I'm not sure of the value of identifying the different datasets and all the acronyms. Surely this information can be removed without any loss of value. For example rather than discussing CRU / NCDC / GISS records why not discuss "three estimates" and where two suggest that 2005 was hottest "2 of 3". A policymaker will have no interest in which dataset shows what and if they do it is in the main text. I think that trying to jargon-lite the Executive Summary will make it much more applicable to a policymaker audience. A scientist / interested person will read the text where such issues acn be spelt out. [Peter Thorne (Reviewer's comment ID #: 264-2)]	
3-95	A	3:1		Executive summary. There's a tendency throughout to quote facts without putting them into context. My specific comments give examples. Each para should say what we know, if it's consistent or not with what we expect under a warming climate, and what we don't know, if appropriate. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-9)]	
3-96	A	3:3	3:10	There are 6 different temperature estimates in this summary paragraph. Much of this detail should remain in the text.	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-186)]	
3-97	A	3:3	3:3	Insert after "temperatures", "measured by the unreliable surface technique" [VINCENT GRAY (Reviewer's comment ID #: 88-300)]	
3-98	A	3:3	3:3	Insert after "century". "A more accurate truly global record for the lower troposphere found no evident temperature change between 1979 and 1999, and radiosondes in the same region found no change between 1958 and 2004. There is evidence that a comprehensive adjustment to the surface record, such as has been carried out for the continental United States and for China, would remove most of the recent apparent warming.in the surface record. A cooling period since 1999 is currently evident.' [VINCENT GRAY (Reviewer's comment ID #: 88-301)]	
3-99	A	3:3	3:10	As written this is very confusing. The text following the finding mixes linear diagnostics with change diagnostics and the change diagnostic is a very short period minus a very long period. This may leave this finding open to attack. How about: "The evolution of globally averaged surface temperature over the 20th Century is complex. Therefore several different methods of extrapolating a change or trend can be argued to be applicable. Linear trend estimates yield 0.60 to 0.71 C/century whereas taking the difference between late 19th Century and early 21st Century temperatures yields a larger net change of 0.80C. Uncertainties are much smaller than these warming signals." [Peter Thorne (Reviewer's comment ID #: 264-1)]	
3-100	A	3:3		compare opening statement of this chapter "global mean temperatures ... have risen 0,8 +- 0,2 °C since the late 19th century" to SPM-6, line 38 ff, where the figure of 0,8 does not appear. Instead, a figure of 0,6 +- 0,2 °C is given as the trend over the 20th century. How do these two figures relate ? [Govt. of Germany (Reviewer's comment ID #: 2011-124)]	
3-101	A	3:3		"late 19th century" is vague. What is the initial year? [Richard Soulen (Reviewer's comment ID #: 248-35)]	
3-102	A	3:4	3:5	Delete "each of which has been independently adjusted for various homogeneity issues". This claim is untrue. The adjustment procedures can only be made where there are many weather stations with a long record; a condition which was originally thought to apply only to the continental United State, but has recently been applied to China. It cannot be applied to countries with very few stations, or with incomplete records [VINCENT GRAY (Reviewer's comment ID #: 88-302)]	
3-103	A	3:5	3:5	Delete "consistent". There are significant differences between the three records. [VINCENT GRAY (Reviewer's comment ID #: 88-303)]	
3-104	A	3:6	3:8	Delete from "The linear trends" on line 6 to "century" on line 8. You admit that the record is not linear, and it is not legitimate to try to draw a straight line through such an irregular graph/	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-304)]	
3-105	A	3:6		Remove the last comma [Richard Soulen (Reviewer's comment ID #: 248-36)]	
3-106	A	3:7		Spell out the names for CRU/UKMO etc. [Richard Soulen (Reviewer's comment ID #: 248-37)]	
3-107	A	3:7		Put a period after decade to the minus one and begin the next sentence This suggests . . ." [Richard Soulen (Reviewer's comment ID #: 248-38)]	
3-108	A	3:8	3:10	Why not estimate the linear warming over 1901-2005? (0.68C or 0.7C in round terms). In my experience, policymakers like to quote warming since the late nineteenth century e.g. as used in recent Hadley Centre COP brochures for policymakers. Non linear warming may be best estimated from a baseline of 1881-1900 using all the temperature data sets (or 1861-1900 using the Brohan data). The current level of global temperature might be best assessed as the low frequency value at 2005. This change would affect some later text. [Chris Folland (Reviewer's comment ID #: 71-2)]	
3-109	A	3:8	3:8	Delete "However" and capitalise "The trend" [VINCENT GRAY (Reviewer's comment ID #: 88-305)]	
3-110	A	3:8	3:10	Delete from "However" to end. This completely distorts the nature of the actual record. You should describe it honestly [VINCENT GRAY (Reviewer's comment ID #: 88-306)]	
3-111	A	3:8	3:10	Replace from "However" on line 8 to end on line 10 with the following "The surface temperature record falls into four distinct sections: a slight fall between 1868 and 1910, a rise of 0.4°C between 1910 and 1942, a fall of -.08°C between 1942 and 1978, and a rise of 0.42°C from 1978 to 2004. None of these sections could have been influenced by greenhouse gas incresases; the first two because the concentrations were low, the third one because increased greenhouse gases could not cause a fall in temperture, and the fourth because influence of greenhouse gas buildup could not possibly begin so late as 1978"" [VINCENT GRAY (Reviewer's comment ID #: 88-307)]	
3-112	A	3:8	3:8	No uncertainty is quoted for the 0.65 figure. In the SPM a figure of 0.65 +/- 0.2 is quoted and I think this would be appropriate here too. [Blair Trewin (Reviewer's comment ID #: 266-14)]	
3-113	A	3:8	3:10	It looks a little odd to compare a 70 year period with the last 5 years, and it doesn't illustrate the point about non-linearity very well either. It might be better to replace this sentence with one which describes a period without a warming trend from 1850 to 1900, then a period of warming, another period with no temperature rise, then warming from 1970. This would lead logically into the next para.	

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				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-10)]	
3-114	A	3:9	3:10	I question the utility of taking a difference over 5 years. This is probably not a meaningful number and too many numbers have already been given. [Dennis Hartmann (Reviewer's comment ID #: 100-10)]	
3-115	A	3:12	3:13	Delete this sentence. It is repe+H46titious [VINCENT GRAY (Reviewer's comment ID #: 88-308)]	
3-116	A	3:12	3:12	Do you think it might improve clarity if the point about surface temperatures rising by 0.16 to 0.18 deg C per decade since 1979 specified that this is for surface temps over both land and ocean? I missed this distinction when I read the point in Section 3.2.2.1 (line page 8, lines 24-25) that cites an increase of 0.27 deg C per decade. Perhaps it's plenty clear now, as you state it. I'm just thinking that adding this qualifier to the executive summary statement might help non-experts appreciate what you're explaining. [Melinda Marquis (Reviewer's comment ID #: 162-42)]	
3-117	A	3:15	3:18	Delete from beginning to "years" on line 18. This claim is not confirmed by other independent global temperature records such as the NASA satellites and radiosondes, for the lower troposphere, and several surface proxy records. [VINCENT GRAY (Reviewer's comment ID #: 88-309)]	
3-118	A	3:15	:17	Reword: 2005 is one of the two warmest years in the instrumental record dating back to 1850, the other being 1998. 1998 ranked first in the CRU/UKMO estimate; 2005 ranked first in the NCDC and GISS estimates. [Richard Soulen (Reviewer's comment ID #: 248-39)]	
3-119	A	3:16	3:16	Perhaps 'warmer' is better than 'ahead'. (Otherwise it sounds like a competition!) [Ian Simmonds (Reviewer's comment ID #: 241-1)]	
3-120	A	3:18		If the change immediately above is adopted delete "in the series since 1850" [Richard Soulen (Reviewer's comment ID #: 248-40)]	
3-121	A	3:19	3:19	Delete from "but" to the end..It is too early to comment on the current slightly warm period [VINCENT GRAY (Reviewer's comment ID #: 88-310)]	
3-122	A	3:21	3:25	Land warming is now sufficiently different that similar figures to those suggested in the previous comment could usefully be given here. [Chris Folland (Reviewer's comment ID #: 71-3)]	
3-123	A	3:21	3:21	Add at beginning "According to the unreliable surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-311)]	
3-124	A	3:21	3:21	Insert after ".oceans" "but this is not confirmed by the other, more reliable records. The satellite record does, however, show greater variability over land than over the sea." [VINCENT GRAY (Reviewer's comment ID #: 88-312)]	

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3-125	A	3:21	3:24	Delete from "Warming" in line 21 to "with" on line 24. This discussion oversimplifies the complexities oif the surface record which cannot be simply cut up into "decades" [VINCENT GRAY (Reviewer's comment ID #: 88-313)]	
3-126	A	3:24	3:24	Capital letter for "The", [VINCENT GRAY (Reviewer's comment ID #: 88-314)]	
3-127	A	3:24	3:24	Insert after "warming" , "over land took place" [VINCENT GRAY (Reviewer's comment ID #: 88-315)]	
3-128	A	3:24	3:24	Why does the executive summary refer to a 0.25 deg C per decade warming since 1979 for land only, whereas Section 3.2.2.1 (page 8, lines 24-25) that cites an increase of 0.27 deg C per decade (which I understand is also for land only)? The Technical Summary (page 19, line 31). [Melinda Marquis (Reviewer's comment ID #: 162-43)]	
3-129	A	3:27	3:36	The various numbers used here are confusing - the 76% and 72% refer to the % of area showing trends of a given sign, but the 71% (at line 32) refers to data coverage. Suggest replacing 'over the 71% of the land surface where data are available' with 'over those land areas where data are available'. The 71% data availability figure is too much detail for an executive summary. [Govt. of Australia (Reviewer's comment ID #: 2001-187)]	
3-130	A	3:27	3:27	Insert after "climate" "by local urban influences" [VINCENT GRAY (Reviewer's comment ID #: 88-316)]	
3-131	A	3:29	:33	The statement that the highest (lowest) 10% of warm (cold) nights has changed is wrong. The percentages are relative numbers and the lowest (highest) 10% are always the lowest (highest) 10%, what has changed are the temperatures of the 10% warmest and coldest nights. Statement in text needs clarification. [Govt. of United States of America (Reviewer's comment ID #: 2023-187)]	
3-132	A	3:30	3:30	"76% of land regions" should be "74% of land regions" from Alexander et al., 2006 [Lisa Alexander (Reviewer's comment ID #: 1-1)]	
3-133	A	3:31	3:31	"72% of same regions" should be "73% of same regions" from Alexander et al., 2006 [Lisa Alexander (Reviewer's comment ID #: 1-2)]	
3-134	A	3:31	3:33	it is difficult to understand the significance of this sentence about diurnal temperature range. It should be quoting evidence which supports the bold type in line 27, but the part about "zero change from 1979-2004" seems inconsistent with this. Does the cessation of DTR reduction mean DTR evidence is NOT consistent with warming of the climate? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-11)]	
3-135	A	3:33	3:33	I was confused at first by the statement in the executive summary that although DTR decreased from 1950 to 2004, DTR ... "had virtually no change from 1979 - 2004." I	

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				presume that the DTR+H55 didn't change for this period because both the nighttime and the daytime maxima increased approximately the same amount. If so, I suppose it may be helpful to the non-expert to state this explicitly, e.g, either on page 3 or page 61 (line 32 discussed DTR). [Melinda Marquis (Reviewer's comment ID #: 162-45)]	
3-136	A	3:36	3:36	Add at end "All this is consistent with an influence of increasing population, building development and energy output in the urban areas where most weather stations are situated" [VINCENT GRAY (Reviewer's comment ID #: 88-317)]	
3-137	A	3:38	3:42	Give specific values over a defined recent period for interhemispheric differences in warming in the Atlantic, and for Indian ocean warming. [Chris Folland (Reviewer's comment ID #: 71-4)]	
3-138	A	3:38	3:38	Insert after "oceans" "but there is serious doubt on the reliability ofd these readings which are not considered worthy of such attention by US investigators, and are undoubtedly subject to many instrumental and other biases" [VINCENT GRAY (Reviewer's comment ID #: 88-318)]	
3-139	A	3:38	3:38	Replace."are" by "seem to be" [VINCENT GRAY (Reviewer's comment ID #: 88-319)]	
3-140	A	3:40	55:43	"Based on a summer monsoon index derived from MSLP gradients between land and ocean in the East Asian region, Guo et al. (2003) found a systematic reduction in the East Asian summer monsoon during 1951–2000, with a stronger monsoon dominant in the first half of the period and a weaker monsoon prevailing in the second half (Figure 3.7.2)." should be reorganized. In fact, early in 2001, Wang (2001) reported the significantly weakened Asian summer monsoon circulation during 1979-1998 relative to 1949-1976 based on the MSLP and low-tropospheric wind reanalysis data from the NCEP/NCAR. Additionally, Jiang et al. (2005) recently confirmed the above weakening during 1951-2000 based on the NRA data and further suggested that it is likely a natural interdecadal change by systematically examining the six historical integrations derived from the CCSR, CGCM2, CSIRO_Mk2, ECHAM4/OPYC3, HadCM3, and NCAR-PCM. Consequently, the suggested revision is "Based on the NRA, Wang (2001) revealed a weakened Asian summer monsoon circulation after 1976-1977 climate shift. Using a summer monsoon index derived from MSLP gradients between land and ocean in the East Asian region, Guo et al. (2003) further confirmed a systematic reduction in the East Asian summer monsoon during 1951–2000, with a stronger monsoon dominant in the first half of the period and a weaker monsoon prevailing in the second half (Figure 3.7.2). Qualitatively, the weakening of East Asian summer monsoon during the period is not present in the six AOGCMs' historical integrations (Jiang and Wang, 2005), a natural	

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				interdecadal change may be implied". References:Wang, H.J., 2001: The weakening of the Asian monsoon circulation after the end of 1970's. Adv. Atmos. Sci., 18, 376-386; Jiang, D. and H.J. Wang, 2005: Natural interdecadal weakening of East Asian summer monsoon in the late 20th century. Chinese Science Bulletin, 50, 1923-1929. [Govt. of China (Reviewer's comment ID #: 2006-35)]	
3-141	A	3:41	3:41	Replace ."lead to important" by "suggest" [VINCENT GRAY (Reviewer's comment ID #: 88-320)]	
3-142	A	3:42	69:43	"The decreasing trend appears linked to the reduced cyclone frequency and increasing winter (DJF) temperatures (Qian et al., 2002)." should be slightly added according to the recently related literatures. The suggested revision is "The decreasing trend appears linked to the reduced cyclone frequency, increasing winter temperatures, intensified westerlies near 50oN, weakened East Asian major trough and the Siberian High as well as the Aleutian Low during boreal winter (Qian et al., 2002; Kang and Wang, 2005). It is also revealed that the Antarctic Oscillation (AAO) is statistically-significantly related to spring dust activities in North China (Fan and Wang, 2004), although causal effect remains unclear at present.". References: Fan, K. and H.J. Wang, 2004: Antarctic oscillation and the dust weather frequency in North China. Geophys. Res. Lett., 31, L10201, doi:10.1029/2004GL019465. Kang, D.J. and H.J. Wang, 2005: Analysis on the decadal scale variation of the dust storm in North China. Science in China (Ser. D), 48, 2260-2266. [Govt. of China (Reviewer's comment ID #: 2006-36)]	
3-143	A	3:44	3:50	The TAR concluded that the urban heat island effect could have affected global average surface temperature by as much as 0.12 C. AR4 owes the reader an explanation of why the TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. [Lenny Bernstein (Reviewer's comment ID #: 20-51)]	
3-144	A	3:44	3:44	Delete "but local" How absurd!. ALL temperature effects are "local" but this does not prevent you from deriving an average [VINCENT GRAY (Reviewer's comment ID #: 88-321)]	
3-145	A	3:44	3:44	Delete "not". You have suppressed the evidence that they DO affect the record. See for example, my paper , Gray, V R, 2000, "The Cause of Global Warming", Energy and Environment, Volume 11, pages 613-629, and McKitrick, R and P J Michaels 2004 "A test of corrections for extraneoous signals in gridded surface temperature data. "Climate Research" Vol 26 pages 159-173 [VINCENT GRAY (Reviewer's comment ID #: 88-322)]	
3-146	A	3:44	3:50	The stance on the urban heat island/global temperature contamination discussion is not	

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				clear. Here is stated that "urban heat island effects are real but local, and have not biased the large-scale trends." This is in contrast to a statement in Chapter 1, Page 7, Line 24-25, which states, "one recurring homogeneity concern is potential heat island contamination in global temperatures." [Govt. of Japan (Reviewer's comment ID #: 2014-37)]	
3-147	A	3:44	3:50	This finding represents a major departure from the TAR, which concluded that the urban heat island effect could have contributed as much as 0.12 C to global average temperature. While AR4 can and should depart from the TAR's conclusions when new information warrents doing so, it should clearly state when it is doing so and provide the reasons for the departure. [Jeff Kueter (Reviewer's comment ID #: 137-48)]	
3-148	A	3:44	:50	The TAR concluded that the urban heat island effect could have affected global average surface temperature by as much as 0.12 C. AR4 owes the reader an explanation of why the TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. [Govt. of United States of America (Reviewer's comment ID #: 2023-188)]	
3-149	A	3:46	3:46	Replace "negligible" by "important" [VINCENT GRAY (Reviewer's comment ID #: 88-323)]	
3-150	A	3:46	3:46	Delete "because" and capitalise "The" [VINCENT GRAY (Reviewer's comment ID #: 88-324)]	
3-151	A	3:46	3:46	Change "are negligible" by "seem to be negligible" [Govt. of Spain (Reviewer's comment ID #: 2019-20)]	
3-152	A	3:47	3:47	Delete "but local".This is irrelevent [VINCENT GRAY (Reviewer's comment ID #: 88-325)]	
3-153	A	3:47	3:47	Insert after "are", "inadequately" [VINCENT GRAY (Reviewer's comment ID #: 88-326)]	
3-154	A	3:47	3:47	Delete "In any case they are not present" Other inadequacies are. Capitalise "In" [VINCENT GRAY (Reviewer's comment ID #: 88-327)]	
3-155	A	3:48	3:48	Insert after "record" Biases rresult from change in measurement method (see Christy et al 2001) and increases in size and energy usage of ships [VINCENT GRAY (Reviewer's comment ID #: 88-328)]	
3-156	A	3:52	3:53	Replace from "temperatures" in line 52 to "2005" in line 53 with "showed no temeperature change between 1979 and 1999, for the satellite series, and no change between 1958 and 2002 for the radiosonde series.The sattellite record sshowed a large peak in 1999 from the El Niño event of that year, and a warm period since 2002" [VINCENT GRAY (Reviewer's comment ID #: 88-329)]	

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3-157	A	3:52	4:8	This finding is too certain and does not fairly reflect the text section which it is summarising. Headline should be: "Lower-tropospheric temperature records all indicate warming, but are highly uncertain". Then the text needs to be significantly streamlined and to be made less certain about whether the troposphere is indeed warming relative to the surface. Suggest "Robust measurement of temperature above the surface is very technologically challenging. Historically this has been acheived by radiosondes (weather balloons) since 1958 and satellites since 1979. Both techniques have undoubted problems. Several groups have attempted to create estimates of recent climate changes from these data. None of these efforts is perfect and problems certainly remain in all estimates. However, all estimates agree that the lower troposphere has been warming. They disagree over whether this warming is greater than that reported for the better observed surface. Disagreements between available estimates are largest within the tropics where sampling is poorest." This would be a fairer reflection of state-of-the-science and leave the ES less open to accusations of spin. [Peter Thorne (Reviewer’s comment ID #: 264-3)]	
3-158	A	3:52	4:8	This paragraph is long and complicated - too much so for an executive summary. In page 4 line 5 it seems to compare a trend from 1979 (to present?) with a decadal warming rate. Why introduce the ERA-40 reanalysis for surface warming here, when it wasn't mentioned in the first para of this Exec summary? The abstract of the US CCSP report on this subject is much more straightforward - I commend its style. [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-12)]	
3-159	A	3:53	3:53	Delete "markedly" [VINCENT GRAY (Reviewer’s comment ID #: 88-330)]	
3-160	A	3:53	3:54	Delete from "and increasing" online 53 to "tropics" on line 54. This statement is unfair. It is done to draw attention away from the much greater unreliability of the surface record [VINCENT GRAY (Reviewer’s comment ID #: 88-331)]	
3-161	A	3:54	3:54	Be more specific: say that it is likely (or very likely) that a number of radiosonde records have a cooling bias, especially in the tropics. [Chris Folland (Reviewer’s comment ID #: 71-5)]	
3-162	A	3:54	4:8	Delete all the rest of this paragraph. It is a transparent attempt to conceal the very real differences between the surface record and the two lower tropospere records.These differences cannot be reduced to "trends", Great use is made of the very large 1999 El Niño event on the MSU record, and it is used to derive a spurious "trend" since 1979 which falls to zero if this event is omitted. The short warm period since 2002 cannot be considered part of a "trend". Excessive attentionhas been paid to inaccuracies in the MSU and radiosonde records while the much greater inaccuarcies in the surface record have been covered up	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-332)]	
3-163	A	4:5	4:5	no units are given for the first warming range. [Blair Trewin (Reviewer's comment ID #: 266-63)]	
3-164	A	4:5		This is apparently an error. The surface temperature of ERA-40 is clearly less positive than HadCRU3v so the ERA-40 Troposphere/Surface relationship is quite strange. In Fig. 3.4.3 one can't even get a clear relationship of trends because the ERA-40 surface trend is so small, especially in the tropics. [John Christy (Reviewer's comment ID #: 41-1)]	
3-165	A	4:6	4:8	This is speculation and wishful thinking. The lion's share of evidence points to a slightly cooler (or perhaps same) trend in the troposphere as the surface since 1979. [John Christy (Reviewer's comment ID #: 41-2)]	
3-166	A	4:6	4:7	It is an accurate statement that "it is likely that there is increased warming with altitude from the surface throughout the troposphere in the tropics". [Qiang Fu (Reviewer's comment ID #: 78-5)]	
3-167	A	4:7	4:8	I am not sure about "likely". The evidence is still mixed about whether the observed warming trend is more or less in the tropics in the troposhere relative to the surface. A reason for being cautious is that it s unclear over the 1979-1999 period how much more warming one would expect in the troposphere if the models used in the CCSP report had all correctly calculated the relative influences on surface and tropospheric trends of the two major volcanic eruptions. The key change in the CCSP report from previous reports is that it is very likely/virtually certain that there has been warming in the global and tropical troposphere since 1979. [Chris Folland (Reviewer's comment ID #: 71-6)]	
3-168	A	4:14	4:14	Replace "lijely" with "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-333)]	
3-169	A	4:14	4:14	Insert 'records from' before 'radiosondes' [Ian Simmonds (Reviewer's comment ID #: 241-2)]	
3-170	A	4:15		Stratospheric warmings occur after more than just volcanic events. [David Rind (Reviewer's comment ID #: 214-19)]	
3-171	A	4:18	3:18	This paragraph is too generalised - and does not apply to large land areas in the Southern Hemisphere. [Govt. of Australia (Reviewer's comment ID #: 2001-188)]	
3-172	A	4:18	4:23	This summary statement contains no comment on areas where significant drying has taken place and therefore appears unbalanced (refer also comment on 3-18)) [Govt. of Australia (Reviewer's comment ID #: 2001-189)]	
3-173	A	4:18	4:23	The title is not corresponding to the content. It has to be replaced	

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				[JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-2)]	
3-174	A	4:18	4:23	The title is not corresponding to the content. It has to be replaced [Govt. of Spain (Reviewer's comment ID #: 2019-62)]	
3-175	A	4:21	4:21	"up" should be "upwards" - over what period? [Chris Folland (Reviewer's comment ID #: 71-7)]	
3-176	A	4:25	4:26	Delete from "Substantial" in line 25 to "that" in line 26. Capitalise "There". There is no evidence that increased precipitation has resulted from the recent short "warm" period [VINCENT GRAY (Reviewer's comment ID #: 88-334)]	
3-177	A	4:26	4:26	prefer 'considered' to 'deemed'. [Blair Trewin (Reviewer's comment ID #: 266-15)]	
3-178	A	4:26	4:26	delete the word "deemed", it's unnecessary [Govt. of United Kingdom (Reviewer's comment ID #: 2022-13)]	
3-179	A	4:27	4:28	...within many land regions…,' Put some examples [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-3)]	
3-180	A	4:27	4:28	...within many land regions…,' Put some examples [Govt. of Spain (Reviewer's comment ID #: 2019-63)]	
3-181	A	4:32	4:39	I think this type of comment opens the IPCC to accusations of biased thinking by ignoring the mega-droughts in the western U.S. in the past 2 millennia, for example, which could not have been related to human influences. [John Christy (Reviewer's comment ID #: 41-3)]	
3-182	A	4:32	4:39	"Droughts have become widespread" is very vague -- need more precision [Isaac Held (Reviewer's comment ID #: 105-16)]	
3-183	A	4:35	4:37	Delete from "In Australia" on line 35 to "droughts" on line 37. There is no evidence for this "inferenec" [VINCENT GRAY (Reviewer's comment ID #: 88-335)]	
3-184	A	4:37	4:39	Statement that "more generally, decreased precipitation and increased temperatures that enhance evapotranspiration and drying are important factors that have contributed to more regions being in drought, as measured by the Palmer Drought Severity Index, (PDSI)" places too much weight on the PDSI. This should be reviewed as the average policy reader will not realise that the measure is incorrect and intrinsically uses temperature as a measure of net radiation. [Govt. of Australia (Reviewer's comment ID #: 2001-190)]	
3-185	A	4:37	4:37	Delete "More generally" and capitalise "Decreased" [VINCENT GRAY (Reviewer's comment ID #: 88-336)]	
3-186	A	4:37	4:39	This asserts that decreasing rainfall and increasing evapotranspiration has increased droughts. If rainfall decreased but evapootraspiration increased, then there would be less	

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				runoff and/or soil moisture - in conflict with most of the studies cited throughout the chapter. [Michael Roderick (Reviewer's comment ID #: 218-2)]	
3-187	A	4:37	4:39	The assertion of less rainfall and more evapotranspiration is in conflict with text on lines 53-56 (p. 4) which asserts more rainfall and more evaportranspiration but less potential evapotranspiration. [Michael Roderick (Reviewer's comment ID #: 218-3)]	
3-188	A	4:37	4:39	The assertion that warming enhances evapootranspiration is wrong and in conflict with the text on lines 53-56 (p. 4). The text on lines 53-56 is excellent and more or less a correct summation of the situation. [Michael Roderick (Reviewer's comment ID #: 218-4)]	
3-189	A	4:39	4:39	"being in" is better expressed as "experiencing" [Chris Folland (Reviewer's comment ID #: 71-8)]	
3-190	A	4:41	4:42	Delete from "Surface specific humidity" on line 41 to "ocean". H13This statement is based on wet and dry bulb measurements in locations which are not distributed over the earth's surface in random manner. The "higher temperatures" are also mainly the result of local instrument influence from urban environments and larger ships [VINCENT GRAY (Reviewer's comment ID #: 88-337)]	
3-191	A	4:48	4:56	This summary is confusing. My understanding of the topic is that the apparent decrease in solar radiation was caused by a portion of the radiation being used for evapotranspiration. If my understanding is correct, a statement clearly linking "dimming" to evapotranspiration should be inserted. If my understanding is incorrect, you have evidence that the summary does not provide the information it should. [Lenny Bernstein (Reviewer's comment ID #: 20-52)]	
3-192	A	4:48	4:56	The link between solar intensity and evapotranspiration is not clear, and without this link, the conclusion does not make sense. [Jeff Kueter (Reviewer's comment ID #: 137-49)]	
3-193	A	4:48	4:56	there should be a reference to evaporation trends in the heading of this section. [Blair Trewin (Reviewer's comment ID #: 266-16)]	
3-194	A	4:48	:56	The link between solar intensity and evapotranspiration is not clear, and without this link, the conclusion does not make sense. [Govt. of United States of America (Reviewer's comment ID #: 2023-189)]	
3-195	A	4:53	4:56	This text is consistent with the conventional approaches, i.e. in water-limitd environments, actual evapotranspiration depends mostly on rainfall, while in energy-limited environments, actual evaportranspiration depends on energy supply (i.e. potential evaportranspiration). The assertion here that actual evapotranspiration increased and	

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				potential evapotrabspiration decreased relates to water-limited envionments and is indicative of reduced droughts, i.e. supply is more capable of meeting the demand. Also see the Introduction in Roderick & Farquhar 2004 (cited in this chapter). The reduction in drought is the opposite of the main theme of the chapter, see comment #1. [Michael Roderick (Reviewer's comment ID #: 218-5)]	
3-196	A	4:55	4:56	Notion of a trade-off is hard to understand for a policy reader. Review drafting (and also in TS) [Govt. of Australia (Reviewer's comment ID #: 2001-191)]	
3-197	A	5:4		suggest removing "possibly relate in part to the El Nino Southern Oscillation" since the grounds for this are not established and the spatial signature of the decadal changes appears statistically distinct from ENSO (e.g. Allan and Slingo 2002, GRL, 29(7), 1141, DOI 10.1029/2001GL014620) [Richard Allan (Reviewer's comment ID #: 3-24)]	
3-198	A	5:5	5:5	El Niño-Southern Oscillation is often hyphenated. [Blair Trewin (Reviewer's comment ID #: 266-64)]	
3-199	A	5:9	3:35	From the context it seems that the authors are suggesting that changes in large-scale atmospheric circlation have been assoicated with global warming, but I do not believe this case has been made. [Dennis Hartmann (Reviewer's comment ID #: 100-11)]	
3-200	A	5:12	5:12	(also abundant locations elsewhere) There is a great deal of inconsistency and imprecision in the terminology used for ENSO in this chapter. Prefer 'El Nino events' to 'El Ninos' as the latter is incorrect Spanish. [Blair Trewin (Reviewer's comment ID #: 266-17)]	
3-201	A	5:12		What is the level of understanding of the 1976/77 "regime shift"; can models reproduce this? Perhaps more of a question for Chapter 8… [Richard Allan (Reviewer's comment ID #: 3-25)]	
3-202	A	5:18	:19	The AMO is not universally accepted as a true atmospheric circulation pattern. Kerry Emmanuel has given a seminar in which he considers the AMO an artifact of data analysis. He should be contacted to determine if his idea has been published in a refereed journal. [Govt. of United States of America (Reviewer's comment ID #: 2023-190)]	
3-203	A	5:19	5:19	The work of Hope et al (2006) showing shifts in synoptic patterns in the Indian Ocean occurring since the mid 1970's should also be noted. The last sentence could have added: "... while changes in the atmospheric circulation patterns over the southern Indian Ocean have been observed to occur since 1975." (Hope, P., Drosdowsky, W., and Nicholls,N. (2006) Shifts in the synoptic systems influencing southwest Western Australia.Climate Dynamics, 7, 751-764).	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-192)]	
3-204	A	5:21	5:35	The discussion of NAO, NAM and SAM is rather confusing. I suggest removing this discussion here, for example, remove from "In September 2002…NAM and NAO are closely related." and remove "…as part of the NAO and NAM changes…" [Richard Allan (Reviewer's comment ID #: 3-26)]	
3-205	A	5:21	5:35	This paragraph overstates the case for westerly wind increases. The NAO/NAM has turned down quite strongly in the last decade; 2005-6, when added, will accentuate this change. [Chris Folland (Reviewer's comment ID #: 71-9)]	
3-206	A	5:26	5:27	It is confusing to have the singular SH sudden warming characterized as due to a exceptionally weak polar vortex immendiately following the statement that the polar vortices are strengthening [Isaac Held (Reviewer's comment ID #: 105-17)]	
3-207	A	5:35	5:35	increased extratropical storminess? I am not sure that the chapter supports such a strong statement. I am frankly also concerned that models are ambiguous on this issue (consistent with the fact that chapters 9 and 10 do not have a simple clear message on this topic.) [Isaac Held (Reviewer's comment ID #: 105-18)]	
3-208	A	5:37	5:37	Insert after "cyclines" , "are thought by some authorities" [VINCENT GRAY (Reviewer's comment ID #: 88-338)]	
3-209	A	5:37	5:49	"Tropical cyclones have increased in intensity and duration since the 1970s." This statement needs a qualifier for intensity. For example, in the Technical Summary, this is termed "more likely than not". Duration I believe is at least as uncertain as intensity in terms of trend, and it does not even appear in the "Extremes Table" for the Technical Summary. Later in the paragraph, "Globally, estimates of the potential destructiveness of hurricanes..." Emanuel's analysis was not global. He presented results for the NW Pacific and Atlantic, and (on his web site) includes the NE Pacific. Sriver and Huber have something coming out in GRL on global PDI, but I assume that's not fair game for this report. Also, I'm not sure they address the question of "longer lifetimes", as Emanuel did (for his subset of basins in the Nature study). [Thomas Knutson (Reviewer's comment ID #: 132-12)]	
3-210	A	5:37	5:37	The bold statement should continue…..it should put these recent increases into a historical perspective. Are they unprecedented? Or nothing exceptional, compared with past variability? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-14)]	
3-211	A	5:37	5:49	This para is a collection of facts not really suited to an exec summary. It should be shortened and contain a reference to what we'd expect under global warming, if we know	

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				- if we don't, say so! And is it consistent with the underlying chater? Eg p65 line 21 refers to some controversy which doesn't seem to be reflected here. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-15)]	
3-212	A	5:37	:49	Summary here does not quite match table on 3-74 vis-à-vis numbers [Govt. of United States of America (Reviewer's comment ID #: 2023-191)]	
3-213	A	5:39	5:40	The statement that internal variability tends to increase tropical storms in some regions and decrease them in others is not the case for the Atlantic -- since essentially all storms are in the NH, AMO-like variability cooling the SH while warming the NH would increase the total number of storms [Isaac Held (Reviewer's comment ID #: 105-19)]	
3-214	A	5:42	5:42	The use of "substantial" seems somewhat qualitative. Perhaps this word could be omitted. [John Caesar (Reviewer's comment ID #: 36-2)]	
3-215	A	5:48	5:49	The "first recorded" wording may not be correct, since there are non reliable statistics of these events. I would prefer the use of "unusual" since there is not guarantee that other events may have ocurred in the past. [Jose Marengo (Reviewer's comment ID #: 159-1)]	
3-216	A	6:1	7:27	The Introduction could be shortened, to a paragraph on where the TAR ended and what major developments have occurred since then, followed by a 'road map' to each of the different sections. [Govt. of Australia (Reviewer's comment ID #: 2001-193)]	
3-217	A	6:3		"…assesses the observed CHANGES IN surface and atmospheric climate, PLACING new observations…": suggested modification for improved readability. [Richard Allan (Reviewer's comment ID #: 3-27)]	
3-218	A	6:4	6:4	Need to spell out TAR - as for a reader who only reads chapter 3, it takes qhite a long time to figure out what is TAR. [Menglin Jin (Reviewer's comment ID #: 118-5)]	
3-219	A	6:12	6:12	Insert after "1976-200" . "Unfortunately, apart from the last one, these divisions do not coincide with the four najor temperature sequences, which were 1858 to 1910, when there was a slight fall, 1910 to 1942 when ther was a rise of 4°C, 1942 to 1978 when there was a fall of 0.8°C, and from 1978 to 2005 when there was arise of 4.2°C. None of these sequences could have been influenced by increase in greenhouse gases; the first two because greenhouse gas concentrations were low, the second, because the rise in greenhouse gases was accompanied by a fall in temperature, and the fiourtnh because greenhouse concentrations could not have begun to oprate so late as 1978". [VINCENT GRAY (Reviewer's comment ID #: 88-339)]	
3-220	A	6:12	6:12	replace "are of" with "had" to be consistent with rest of sentence	

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				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-16)]	
3-221	A	6:14	6:14	Is the 1976 "climate shift" really "widely acknowledged"? An informal poll of a few observationally oriented researchers whom I respect suggests that a lot of poeple think of this as resulting from a random superposition of 2 or 3 things, and not as a fundamentla shift. I find the emphasis so early in the chapter on this "shift" as awkward and uneccesary. The text seems to imply that a "jump" of some sort is what we should expect when the anthropogenic signal emerges from the noise, which seems implausible to me. [Isaac Held (Reviewer's comment ID #: 105-20)]	
3-222	A	6:15	6:15	Replace "has been" by "could not be". [VINCENT GRAY (Reviewer's comment ID #: 88-340)]	
3-223	A	6:15	6:15	As written it implies 100% attribution, which is misleading, since the idea that all cliamte change is attributable to GHG forcing is an extreme position held by few if any experts. Insert "partially" after the word "been" and before "attributed". This suggestion was made in the FOD review and ignored. It is hereby repeated, for the same reason: the present wording is deliberately misleading. [Ross McKitrick (Reviewer's comment ID #: 174-12)]	
3-224	A	6:16	6:16	Insert after "atmosphere" "because there was no evidence of such an influence during the previous period" [VINCENT GRAY (Reviewer's comment ID #: 88-341)]	
3-225	A	6:16	6:16	Replace "see" by "in contrast to" [VINCENT GRAY (Reviewer's comment ID #: 88-342)]	
3-226	A	6:23	6:24	References - Kalnay et al.(1996) and Uppala et al.(2005), respectively - should be given for the NRA and ERA-40 at this point, not just buried away in an Appendix. [Adrian Simmons (Reviewer's comment ID #: 242-38)]	
3-227	A	6:24		Change "after 1979" to "from 1979 onwards" or "after 1978". The key changes to the observing system came in close to the end of 1978, in preparation for the FGGE year of 1979. See also comment #22. [Adrian Simmons (Reviewer's comment ID #: 242-39)]	
3-228	A	6:25		A reference to Simmons et al. (2004) could, in fairness, be included here. We discussed sharp changes at the end of 1978 also, and I think it important to include a refence that indicates that the analysis producers as well as users are well aware of the issue. And we were certainly the first to appreciate it, if not to publish it, as we were expecting it and saw it happen as production proceeded. [Adrian Simmons (Reviewer's comment ID #: 242-40)]	
3-229	A	6:26		Change "post-1979" to "post-1978". [Adrian Simmons (Reviewer's comment ID #: 242-41)]	

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3-230	A	6:29	6:34	"atmospheric waves" is too vague to be understood -- better would be "stationary meanders of the jet stream on scales of thousands of kms". What "anomalies" are being referred to? Interannual? (Long term changes in the phase of these waves is evidentally insufficient to change the sign of the temperature response in models anywhere, but this sentence makes it sound like we should expect climate change to involve temperature changes of different signs in different regions). It would help if this paragraph were rewritten starting with a discussion of what it means for trends in circulation to look like natural modes of variability. [Isaac Held (Reviewer's comment ID #: 105-21)]	
3-231	A	6:34	6:36	If this is retained in the Introduction a reference is needed. [Govt. of Australia (Reviewer's comment ID #: 2001-194)]	
3-232	A	6:41	6:41	after "changes in" insert "phenomena such as" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-17)]	
3-233	A	6:44	6:44	I do not understand the difference between a "variable" and a "phenomenon". If we can't quantify a change in a "phenomenon" in terms of a "variable" how can we talk about it objectively. [Isaac Held (Reviewer's comment ID #: 105-22)]	
3-234	A	6:48	6:48	I think the use of the phrase 'but they are merely' takes away from the importance of extremes as stated earlier in the sentence. I would suggest a change to "they can be considered as an expression of the variability" [John Caesar (Reviewer's comment ID #: 36-1)]	
3-235	A	6:50	6:50	Replace "are most" by "cannot be" [VINCENT GRAY (Reviewer's comment ID #: 88-343)]	
3-236	A	6:50	6:51	Delete from "and" in line 50 to "cooling" in line 53. There really is no evidence relating "forcing" to "extreme events" [VINCENT GRAY (Reviewer's comment ID #: 88-344)]	
3-237	A	6:52	6:52	What regions are being referred to that cool in response to "global warming induced changes in the planetary wave pattern"? I am not aware an any such midlatitude regions in model projections -- so I am inclined, unless convinced otherwise, to assume that any observed cooling trends are due to internal variaiblity or aerosols. The South Pole and its relationship to the poleward displacement of the circulation in SH might be the closest thing of this type, but it is not related to changes in the phase of a planetary wave. [Isaac Held (Reviewer's comment ID #: 105-23)]	
3-238	A	6:53	7:7	The example chosen is too complicated. One could simply give "typical values" instead of Table 3.1. [Govt. of Finland (Reviewer's comment ID #: 2009-51)]	

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3-239	A	6:54	6:54	Replace "are" with "could be" [VINCENT GRAY (Reviewer's comment ID #: 88-345)]	
3-240	A	6:57	7:2	'A normal distribution is a reasonable approximation…'. This is not true for Australia (and probably not, in general, for areas near a coastline with a strong land/sea temperature contrast). References can be provided if required. The statement in the text is not really necessary for the argument. The sentence could be replaced by 'Whilst temperature is not normally distributed everywhere, the standard deviation is still a reasonable indicator of variability on daily to annual timescales.' [Govt. of Australia (Reviewer's comment ID #: 2001-195)]	
3-241	A	7:0		Table 3.1: Why not also show this measure of variability for an area intermediate in size between the whole US and a point, perhaps using one of the Giorgi regions -- the reason being that for temperature, at least, point variability of monthly means is not that different from the variability on the 1,000 km scale of the planetary waves -- the US is big enough to contain both signs of a typical temperature monthly anomaly, but a Giorgi region is not. [Isaac Held (Reviewer's comment ID #: 105-24)]	
3-242	A	7:9	7:14	It is difficult to follow what is presented in table 3.1 (different time and space scales not very clearly identified in the text) [Pedro Ribera (Reviewer's comment ID #: 213-8)]	
3-243	A	7:12	7:13	It should be better to explain the meaning of "difference"; more likely the "number of differencies" wrt the mean at each time scale in the legend of table 3.1. It could be confused with the "intensity of the difference". [Tiziano Colombo (Reviewer's comment ID #: 46-11)]	
3-244	A	7:12		The "Range of Temperature" 13.1 (December) to 15.1 (September) could be misinterpreted as a range from 13.1 to 15.1; I suggest just using one month. [Richard Allan (Reviewer's comment ID #: 3-28)]	
3-245	A	7:17	7:29	If these definitions are consistent with those used in Chapter 9, may be there is not need to repeat them here and just refer to Chapter 9. It may be confusing. [Jose Marengo (Reviewer's comment ID #: 159-2)]	
3-246	A	7:20	7:20	really' should be 'very' [Ian Simmonds (Reviewer's comment ID #: 241-3)]	
3-247	A	7:23	7:27	The descriptions of 'likely' etc could be moved to a footnote at the front of the chapter. [Govt. of Australia (Reviewer's comment ID #: 2001-196)]	
3-248	A	7:23	7:27	Delete from "Where this is not possble" on line 23 to the end on line 27. This is an attempt to place spurious figures on comple, subjective, investigator-biased guesswork and can only mislead the reader [VINCENT GRAY (Reviewer's comment ID #: 88-346)]	

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3-249	A	7:23	:27	Suggest providing the information in a table rather than as text. Provide or refer to table in one place in the document, perhaps the Technical Summary table. [Govt. of United States of America (Reviewer's comment ID #: 2023-192)]	
3-250	A	7:24	7:27	Do you want to refer to the Box on Uncertainty (definitions of "likely," etc.) in the TS (TS, page 3, line 41 to page 4, line 42)? [Melinda Marquis (Reviewer's comment ID #: 162-41)]	
3-251	A	7:24		These lines could just reference the relevant Box in Chapter 1. [Richard Allan (Reviewer's comment ID #: 3-29)]	
3-252	A	7:36	7:36	After "revisions" add "to the land surface record" [Chris Folland (Reviewer's comment ID #: 71-10)]	
3-253	A	7:40	7:41	Delete from "as" on line 40 to "urbanizatioin" on line 41. This claim is false. No details of this procedure have been published and McKitrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 have shown that the whole set, even after supposedly "corrected" for urbanization effects, is significantly influenced by a whole range of socioeconomic factors such as increases in population, coal usage and prosperity. [VINCENT GRAY (Reviewer's comment ID #: 88-347)]	
3-254	A	7:49	8:1	Although the difference between skin and bulk temperature is explained later in the section, this is the first time the two concepts have been given. The difference should be given here. [Govt. of United States of America (Reviewer's comment ID #: 2023-193)]	
3-255	A	8:0		Section 3.2.2.1: the details of differences in temperature variations is not policy relevent and could be reduced by noting that differences relate to the infilling technique and homogenisation in the first paragraph.This would make this paragraph more concise. [Richard Allan (Reviewer's comment ID #: 3-31)]	
3-256	A	8:2	8:2	The order of references need to be started from the paper publiserd earlier, namely, here it hsould be "Peterson et al., 2000; Jin and Dickinson 2002; Kwok and Comiso 2002b) [Menglin Jin (Reviewer's comment ID #: 118-6)]	
3-257	A	8:16	8:19	Confusing sentence. Suggest: "The performance of ERA-40 is degraded prior to (i) the availability of satellite data in the mid-1970s and (ii) the adequate collection of sub-daily surface data from 1967 (see Appendix 3.B.5)." [Richard Allan (Reviewer's comment ID #: 3-30)]	
3-258	A	8:23	8:51	Two different acronyms are used here: GHCN en NCDC. Do they refer to the same data set? If so, use one acronym only. [Fons Baede (Reviewer's comment ID #: 9-26)]	
3-259	A	8:23	8:51	Comment on why the NOAA and Brohan et al records diverge recently after a long period	

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				of good agreement. [Chris Folland (Reviewer's comment ID #: 71-11)]	
3-260	A	8:24	8:51	Following comment 1, for example, in this paragraph, too many jargon for technique details of data sets. It is very hard to understand, and is not interesting at all for one to read. I suggest to move such techniques into footnotes. And just highlight the key results. [Menglin Jin (Reviewer's comment ID #: 118-2)]	
3-261	A	8:31	8:31	This appears to be an inappropriate use of the term 'very likely in the context of its formal definition, authors should review. [Govt. of Australia (Reviewer's comment ID #: 2001-197)]	
3-262	A	8:33	9:31	This section needs to be shortened - with tighter text focussing on a more global perspective. Too much of the text deals with the USA. [Govt. of Australia (Reviewer's comment ID #: 2001-198)]	
3-263	A	8:40	3:40	Insert before "homogeneity" "the very limited". "homogeneity adjustment" cannot be thoroughly applied unless there are large numbers of stations. Full "hoimogeneity adjustment" has only been carried out so far in the continental United Staes and in China. . [VINCENT GRAY (Reviewer's comment ID #: 88-348)]	
3-264	A	8:41	8:41	Lmsert after "urbanixation" "but full details of this procedure have not been published, and they have been shown to be incomplete by McKitrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 [VINCENT GRAY (Reviewer's comment ID #: 88-349)]	
3-265	A	8:42	8:45	This is a specific country example which does not significantly add to the discussion. The sentence could be deleted. [Govt. of Australia (Reviewer's comment ID #: 2001-199)]	
3-266	A	8:43	8:43	Insert before "homogeneity" "thorough" [VINCENT GRAY (Reviewer's comment ID #: 88-350)]	
3-267	A	8:51	8:51	Add at end. "A .Chinese subset pf the global surface record by ZHOU, Zongci, Yihui DINGi,Yong LUO, and Shaowu WANG. 2005 "Recent Studies on attributions of climate change in China", Acta Metorologica Sinica Vol 19, pages 389-400 shows that in common with the US workers, the presence of many stations, with the possibility oif accurate corrections, gives a final surface temperature record from 1900 which shows very little overall temperature change. This suggests that if a similar thorough adjustment could be made globally that there would also be little overall warming" [VINCENT GRAY (Reviewer's comment ID #: 88-351)]	
3-268	A	8:56	8:56	Also state what the actual ERA-40 trends are.	

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				[Chris Folland (Reviewer's comment ID #: 71-12)]	
3-269	A	9:1		ERA-40 is not entirely independent of the data that went into the Jones and Moberg analysis, as correctly stated. But by the same token it is not entirely independent of the data that went into the GHCN, GISS and Lugina et al. analyses, in that some of the SYNOP data analysed by ERA-40 are subject to similar station-dependent biases as some of the CLIMAT data used in the other analyses. The trend quoted for ERA-40 (0.03K/decade less than Jones and Moberg) has an additional dependence on the Jones and Moberg trend in that, for the purpose of comparison, the ERA-40 trends were computed using only grid boxes for which a data value was calculated by Jones and Moberg. The ERA-40 trend is somewhat larger when computed using complete data coverage, as noted by Simmons et al.(2004). [Adrian Simmons (Reviewer's comment ID #: 242-42)]	
3-270	A	9:6	9:15	Putting this paragraph at this point breaks the flow. It would naively make more sense to place this paragraph after Table 3.2 but before Figure 3.2.2. The paragraph is not referenced by the paragraph that follows it. [Peter Thorne (Reviewer's comment ID #: 264-4)]	
3-271	A	9:26	9:30	The REML method can use more complex AR models. Have you tested if the AR1 model fits best? Referring back to line 21, will an ARx model allow adequately for long term persistence anyway? This could e.g. arise from the signal of the AMO in global and hemispheric temperatures. I dont actually think this matters a great deal but you do make a point of this on line 21,so consistency is needed. The REML method can also allow for the uncertainties in all the annual values. The trend is almost unaffected by doing this, but trend uncertainties are widened. Did you use this form of REML (I think the TAR did). If not, why not?. [Chris Folland (Reviewer's comment ID #: 71-13)]	
3-272	A	9:26		Table 3.2 Table 3.2 provides warming rates for land and ocean separately, and for NH and SH separately. Global land and global ocean is also given, but not global land+ocean, which are the figures quoted in the Executive Summary (3-3, lines 3-10) and SPM (SPM-6, lines 38-41). [A. Brett Mullan (Reviewer's comment ID #: 182-7)]	
3-273	A	9:31		Table 3.2: this would be more informative as a bar chart/figure. [Richard Allan (Reviewer's comment ID #: 3-32)]	
3-274	A	10:0		Section 3.2.2.3, is obviously important but the amount of details may detract from the description of the main issues. [Richard Allan (Reviewer's comment ID #: 3-34)]	

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3-275	A	10:5	11:10	In the next few rows I repeat comments supplied in review of the FOD, none of which have been dealt with. [Ross McKitrick (Reviewer's comment ID #: 174-14)]	
3-276	A	10:11	10:11	Add aftter "sea". But on average, their effects are substantial, as has been demonstrated by McKitrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 have shown that the whole set, even after supposedly "corrected" for urbanization effects, is significantly influenced by a whole range of socioeconomic factors such as increases in population, coal usage and prosperity [VINCENT GRAY (Reviewer's comment ID #: 88-352)]	
3-277	A	10:13	10:28	The TAR concluded that the urban heat island effect could have affected global average surface tempertaure by as much as 0.12 C. AR4 owes the reader an explanation of why the TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. [Lenny Bernstein (Reviewer's comment ID #: 20-53)]	
3-278	A	10:13	10:28	This finding represents a major departure from the TAR, which concluded that the urban heat island effect could have contributed as much as 0.12 C to global average temperature during the 20th century. While AR4 can and should depart from the TAR's conclusions when new information warrents doing so, it should clearly state when it is doing so and provide the reasons for the departure. [Jeff Kueter (Reviewer's comment ID #: 137-50)]	
3-279	A	10:13	:28	The TAR concluded that the urban heat island effect could have affected global average surface temperature by as much as 0.12°C. AR4 owes the reader an explanation of why the TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. [Govt. of United States of America (Reviewer's comment ID #: 2023-194)]	
3-280	A	10:14	10:15	States, "the key issue from a climate change standpoint is whether urban-affected temperature records have significantly biased large-scale temporal trends." giving the impression that the effect of urban-affected temperature is debatable. However, in Chapter 3, Page 3, Line 44-50, the stance is more definitive ("urban heat island effects are real but local, and have not biased the large scale trends"). [Govt. of Japan (Reviewer's comment ID #: 2014-38)]	
3-281	A	10:15	10:15	Insert after "trends" " McKitrick, R and P J Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 have shown that these effects are substantial. Even on supposedly "corrected" sets, giving corrected sequences which showed little overall warming" [VINCENT GRAY (Reviewer's comment ID #: 88-353)]	

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3-282	A	10:15	10:15	Replace "Studies" by "Earlier studies" [VINCENT GRAY (Reviewer's comment ID #: 88-354)]	
3-283	A	10:15	10:19	The sentence initially began "The few studies that have looked at hemispheric and global scales conclude that any urban-related effect is an order of magnitude smaller..." The first two words have been deleted, but the sentence remains untrue, for reasons argued in this and the next 5 rows. The claim of the effect being an "order of magnitude" smaller is traced to Jones (1990), which shows no such thing. First, the paper is 15 years old, and refers to regional data sets that are not used in the AR4. Second, Jones 1990 only examines the US, the western USSR, eastern Australia and eastern China, so it is not a global or hemispheric sample. Third, it proves the opposite of the assertion being made, since the evidence presented in the paper all points to differential urban-rural trends that dominate the regions. In the USSR data they say: "Over the 1930-1987 period, a cooling of ~0.2 C in RUSSR [rural series] is observed. This cooling is about 0.1 C smaller in JUSSR [combined rural-urban], but there are no statistically significant differences between the two series." (p.171). For eastern China they say: "The warming in UCHI [urban series] is 0.39C, considerably higher than that in RCHI [rural series]. For this region, UCHI is the only series for which warming is staistically significnat." (pp. 171-172). For eastern Australia they find similar warming in the rural and urban series, though they define "rural" as up to 33,368 persons (and they don't exaplin why they chose just the eastern part of Australia) so this is the least important region in their analysis. For the US they report earlier findings of a significant (0.15C) urban warming bias. Yet in both the abstract and the conclusion of their paper, they assert that their results provide little or no evidence of urbanization bias, a statement directly contradicted by their own evidence. They suggest that urbanization represents at most 0.05 C of the observed 0.5 C warming over the entire century, with no quantitative basis whatsoever. The 0.05 figure is not calculated anywhere in the paper, it is an off-the-cuff guess about the maximum that might be observed in key areas of the world they did not examine, i.e. Europe and the tropics (p. 172). Despite finding an urban warming bias everywhere but eastern Australia they assert that "In none of the three regions studied here is there any indication of significant urban influence" and "The United States result therefore does seem somewhat atypical compared with other industrialized regions of the world" (p. 172). This latter statement is particularly misleading since their ad hoc sample of eastern China, eastern Australia and the western USSR hardly constitute the "industrialized regions of the world" outside the US. Jones et al.'s own conclusion stands at odds with the findings in their paper. To quote their "spun" conclusion while ignoring the fact that the paper's own evidence contradicts the AR4 claim, is deceptive to IPCC readers. If you want to refer to Jones (1990) then quote it accurately: it provides evidence that urban influences on temperature data do show up in several regions including the US, China and the Russia,	

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				and it provides no evidence that these influences are small in the global average. [Ross McKitrick (Reviewer's comment ID #: 174-15)]	
3-284	A	10:15	10:19	The claim that studies looking at global and hemispheric scales have found only minuscule effects is also false on the grounds presented in McKitrick and Michaels (2004) and deLaat and Maurellis (2004), both of which look at global scales, both of which found non-miniscule effects but neither of which were cited here. McKitrick and Michaels used a global sample, comparing effects observed in a global sample of 218 weather stations with the effects in the corresponding grid cells. The authors established that there are statistically significant nonclimatic biases at the global level in the post-1979 trends from station data and that the same pattern carries over to the gridded data. They also showed that the biases likely add up to a net warming bias, accounting for one-third to one-half of the observed average trend. deLaat and Maurellis examined all land areas and divided them into regions above/below a threshold in CO2 emissions. Regions with higher local CO2 emissions are not predicted to have higher local warming, according to GCMs. But the data show they do experience significantly higher local warming, suggesting the CO2 level is acting as a proxy for local industrial activity. deLaat and Maurellis have extended this result and established it on a wider range of data sets, including land+ocean areas, in the March 2006 Int J Clim. That paper is probably too late for IPCC usage, but their 2004 paper is certainly qualified for use. [Ross McKitrick (Reviewer's comment ID #: 174-16)]	
3-285	A	10:15	10:19	Additional Note on McKitrick and Michaels (2004): This paper was the subject of much controversy, mostly unpublished. The paper's results are robust and its conclusions are highly pertinent to AR4 deliberations. A published comment by Benestad (Climate Research 2004 27:171-173) argued against some of the conclusions on the grounds that the SH data and a subset of explanatory variables failed to predict the NH dependent variables; the reply by McKitrick and Michaels pointed out that this was an ill-posed test, and the cross-validation exercize in the paper itself (in which the North and South American data were withheld and skillfull predicted) is more appropriate; also Benestad acknowleged attempting a number of respecifications and found they yielded "similar, although not identical, model coefficients, t-values, and R2 scores to those reported by McKitrick & Michaels, indicating that the analysis captures similar relationships." An unpublished commentary on the internet identified a minor coding error in which latitude data was used in degrees while a cosine calculation assumed they were in radians. This error was corrected and new results promptly published (CR Vol 27(3)) showing only minor effects on the coefficients and standard errors and the upholding of the original conclusions of the paper. Additional, unpublished internet commentary suggested that the standard errors were mis-estimated because of clustering effects in the data. This primarily reflected the failure of the commentator to understand the estimator used in the	

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				original paper, but additional code presenting replication of the results applying an exact clustering adjustment was made available at the paper's SI (http://www.uoguelph.ca/~rmckitri/research/gdptemp.html). [Ross McKitrick (Reviewer's comment ID #: 174-17)]	
3-286	A	10:15	10:19	McKitrick and Michaels (2004) treats the IPCC claim of the absence of a global nonclimatic bias in the surface record as a hypothesis to be tested, exactly in accord with the methodological goals prescribed in Chapter 1 of the AR4. You do not have the option of simply ignoring results you don't like. The hypothesis was tested and rejected, and the study has not been refuted. Nor can you ignore the deLaat and Maurellis findings, which again contradict the claim in this paragraph. This section raises the question of global data quality. The question has been treated in the literature and some key evidence has been published that does not go in favour of earlier positions taken by the IPCC. It is a disservice to readers to suppress this information. [Ross McKitrick (Reviewer's comment ID #: 174-18)]	
3-287	A	10:15	10:28	Parker (2005) is the only other support for the strong claims in this paragraph. Even though Parker is a Lead Author does not mean his work should be the only material cited. Parker's paper is very short, relies on a visual comparison of trends and has not been in print long enough to be subject to much critical discussion. The paper relies on the assumption that wind is a significant mitigating factor for the UHI. Yet Morris, Simmonds and Plummer (2000 http://ams.allenpress.com/amsonline/?request=get-abstract&issn=1520-0450&volume=040&issue=02&page=0169) showed that wind does not have a straightforward mitigating effect: it varies seasonally, the effects diminish at the 1/4 exponential rate and cloud cover is more influential. These findings have apparently been corroborated in several urban studies (see survey in McKendry, Progress in Physical Geography, Volume 27, Number 4, 1 December 2003, pp. 597-606). Also Pielke and Matsui (2005) have argued that the Parker experiment is ill-posed, since there is no strong prior for assuming that the trend lines ought to be parallel under the null hypothesis of no urbanization bias. Overall these studies indicate that Parker's result cannot, on its own, overturn any and all claims that nonclimatic biases have been removed from the global temperature data base. By all means cite it, but don't mislead readers by suggesting that it is the only study out there, and is so all-encompassing and infallible that counter-arguments should not even be mentioned. Even considering the UHI issue on its own, it may simply be equally effective in both windy and calm subsamples; alternatively the UHI may be making equivalent two trend lines that would have otherwise differed. You cannot make sweeping, permanent claims based on one study which happens to be authored by a chapter LA. The whole point of the IPCC is to survey all the science, not just the bits you like. [Ross McKitrick (Reviewer's comment ID #: 174-19)]	

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3-288	A	10:15	10:28	The paragraph should be reworded as follows: "Many local studies have demonstrated that the microclimate within and around cities is, on average, warmer than if the city were not there, and that as cities grow or become more densely arranged the potential urban heat bias also grows. However, the key issue from a climate change standpoint is whether nonclimatic influences on temperature records have significantly biased large-scale temporal trends. An early study that looked at western Russia, eastern China and eastern Australia found some evidence of small urban-related warming biases but conjectured the global effect was small (Jones et al., 1990). Parker (2006) noted that warming trends in night minimum temperatures over 1950–2000 were not enhanced on calm nights, which would be the time most likely to be affected by urban warming. McKitrick and Michaels (2004) compared the spatial pattern of trends in station and gridded data (i.e. before and after treatment for nonclimatic biases) and concluded the gridded data exhibit a significant correlation to socioeconomic covariates, in a manner suggestive of incomplete treatment for inhomogeneities. DeLaat and Maurellis (2004) compared surface and tropospheric trends in regions partitioned by a varying threshold level of CO2 emission levels, which they took to be a proxy for density of local industrial activity. Regions with high CO2 emissions exhibited significantly stronger local warming, a pattern not predicted in climate models, suggesting the effects were nonclimatic. Overall, it is likely that the global land-based warming trend is biased upwards to some extent by nonclimatic influences arising from anthropogenic modifications of the Earth's surface, including, but not limited to, urban heat island effects." [Ross McKitrick (Reviewer's comment ID #: 174-20)]	
3-289	A	10:15	10:28	Sources cited above: McKitrick, R and P. J. Michaels (2004). "A Test of Corrections for Extraneous Signals in Gridded Surface Temperature Data" Climate Research 26(2) pp. 159-173. "Erratum," Climate Research 27(3) 265—268; de Laat, A. T. J. and A. N. Maurellis. (2004) "Industrial CO2 emissions as a proxy for anthropogenic influence on lower tropospheric temperature trends." Geophysical Research Letters, VOL. 31, L05204, doi:10.1029/2003GL019024, 2004. [Ross McKitrick (Reviewer's comment ID #: 174-21)]	
3-290	A	10:16	10:16	Insert begore "conclude" "seemed to" [VINCENT GRAY (Reviewer's comment ID #: 88-355)]	
3-291	A	10:17	10:19	I believe that this is an attribution statements, that is more suitable to Chapter 9 than in Chapter 3 [Jose Marengo (Reviewer's comment ID #: 159-3)]	
3-292	A	10:21	10:21	Insert after "warming" "but this is only one of the many factors identified in "homogeneity adjustment". [VINCENT GRAY (Reviewer's comment ID #: 88-356)]	

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3-293	A	10:21	10:22	Delete from "This" on line 21 to .(Parker 2006) on line 22 McKitrick, R and P J Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 have shown that this statement is untrue. [VINCENT GRAY (Reviewer's comment ID #: 88-357)]	
3-294	A	10:22	10:22	Remove "very" before unlikely. [Govt. of Spain (Reviewer's comment ID #: 2019-21)]	
3-295	A	10:23	10:23	Replace "trends" with "mean annual anomalies were 0.31°C less than urban anomalies, but after correction for elevation, time of observation bias and instrumentation, rural series were" [VINCENT GRAY (Reviewer's comment ID #: 88-358)]	
3-296	A	10:24	10:24	Replace "The same is true of" with "similar considerations apply" [VINCENT GRAY (Reviewer's comment ID #: 88-359)]	
3-297	A	10:25	10:26	Delete from "One possible reason" in line 25 to (Peterson 2003) in line 26". This is nonsense. Weather stations are situated in a large variety of locations, most of which are influenced by increasing urban surroundings. The comment may apply exclusively to the USA [VINCENT GRAY (Reviewer's comment ID #: 88-360)]	
3-298	A	10:26	10:27	Delete from "In summary" on line 26 to "locations" on line 27". This can not be generalised from such a small sample. [VINCENT GRAY (Reviewer's comment ID #: 88-361)]	
3-299	A	10:27	10:27	Insert after "ewffect is" "not necessarily" [VINCENT GRAY (Reviewer's comment ID #: 88-362)]	
3-300	A	10:27	10:28	Replace "all global studies" on line 27 to "negligible" on line 28 with"as McKitrick and Michaels study has shown that urban effects are a major" [VINCENT GRAY (Reviewer's comment ID #: 88-363)]	
3-301	A	10:30	10:41	This large amount of text seems to be devoted to indicating that the Kalnay and Cai (2003) conclusions are incorrect. Would it be better to instead just mention that reanalyses may be suitable for estimating trends since 1979 but that most of the changes relate to changes in the type of data assimilated? [Richard Allan (Reviewer's comment ID #: 3-33)]	
3-302	A	10:30	11:10	I was disappointed not to see a reference to Christy et al. 2006 (J. Climate) regarding land-use changes and the very detailed and exhaustive analysis that was done with the surface temperature data in Central California showing significantly different trends between the heavily changed Valley and the unchanged Sierras. In particular there is a highly significant change in relative DTR between Valley and Sierra regional composites which is consistent with widespread irrigation in the Valley.	

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				[John Christy (Reviewer's comment ID #: 41-4)]	
3-303	A	10:32	10:33	Delete from "This conclusion" in line 32 to ""observations" in line 33. This satement is untrue [VINCENT GRAY (Reviewer's comment ID #: 88-364)]	
3-304	A	10:33	10:41	Delete from "But the reanalyses" on line 33 to "Appendix 3.B.5" on line 41. This is just a list of porrly justified excuses for which very little evidence is given. [VINCENT GRAY (Reviewer's comment ID #: 88-365)]	
3-305	A	10:36		I believe there are several studies which support Kalnay and Cai, yet this portion seems to go out of its way to discredit Kalnay and Cai. Note that Christy et al. 2006 (J. Climate) has information supporting Kalnay and Cai at least for Central California. [John Christy (Reviewer's comment ID #: 41-5)]	
3-306	A	11:1	11:1	Delete "Nevertheless" [VINCENT GRAY (Reviewer's comment ID #: 88-366)]	
3-307	A	11:14	12:55	Also applies to Tables 3.2 and 3.3. State here or elsewhere how you have combined the hemispheres for the marine and combined data sets. [Chris Folland (Reviewer's comment ID #: 71-14)]	
3-308	A	11:14		There is considerably more description given to techniques for correcting SST data than to other observations. For example, there is no discussion of the possible effect of the different observing methods (e.g., CTD, bottles, XBTs, etc.) used to generate the World Ocean Data summaries. For parallel discussions, either the SST technique descriptions should be reduced or ideally, the subsurface databases and potential biases between measurement systems should be increased. [Govt. of United States of America (Reviewer's comment ID #: 2023-195)]	
3-309	A	11:15	11:16	here it says "the temperature in the uppermost few meters of the ocean". Just hope the author double check if this should be "centermeter". I don't know SST very well, but since it called "surface temperature", "a few meters" seems not to be "surface". I may be wrong, but I feel uncomfortable and please double check. [Menglin Jin (Reviewer's comment ID #: 118-7)]	
3-310	A	11:28	11:29	Delete from "not large enough" on line 28 to 3.B.3) on line 29..and replace with "is only one of many sources of upwards bias for sea-surface observations" [VINCENT GRAY (Reviewer's comment ID #: 88-367)]	
3-311	A	11:34	11:38	Delete from "Confirmation" in line 34 to (Folland 2005) in line 38. This agreement is fortuitoud as there are other upward biases in sea-surface measurements from increases in size and temperature of ships which are not allowed for. [VINCENT GRAY (Reviewer's comment ID #: 88-368)]	
3-312	A	12:30	12:30	Add at end "Despite these improvements there are unresolved upwards biases resulting	

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				from increases in size and ebergy consumption of ships" [VINCENT GRAY (Reviewer's comment ID #: 88-369)]	
3-313	A	12:47	12:47	"one signature of the THC (e.g., Zhang and Delworth, 2005). Ref: Zhang, R., and T. L. Delworth, 2005: Simulated tropical response to a substantial weakening of the Atlantic thermohaline circulation. Journal of Climate, 18(12), 1853-1860. [Thomas Knutson (Reviewer's comment ID #: 132-13)]	
3-314	A	13:4	13:28	It is indicated that 1998 and 2005 were very close in term of warmest year. It could be underlined that 1998 was a strong Niño year for which high global temperature could be expected. However this was not the case for 2005 [Govt. of France (Reviewer's comment ID #: 2010-25)]	
3-315	A	13:6	13:8	Isn't this sentence the wrong way around? (surely it was the continental warmth being extrapolated over the Arctic Ocean, not vice versa as implied?). [Blair Trewin (Reviewer's comment ID #: 266-18)]	
3-316	A	13:25	13:28	In Turner,J., King,J.C., Lachlan-Cope,T.A. & Jones,P.D. (2002) Recent temperature trends in the Antarctic. Nature, 418, 291-292 we make a strong case that the analysis of Doran, which you quote, was based on a flawed analysis of the limit amount of station data being extrapolated across huge distances of the Antarctic. I don't feel that the data suggests a statistically significant cooling of the whole Antarctic mainland since 1966. As a second point I would suggest that the high quality in-situ data from the Antarctic Peninsula shows that the warming across the region is more than 'very likely'. I think this change is better proven than the Doran result which you seem to fully accept. [John Turner (Reviewer's comment ID #: 272-1)]	
3-317	A	13:25	:26	But there is no statistically significant cooling in annual average surface air temperatures anywhere in the Antarctic when looking at the entire station time series - see comment 7. Doran et al. used quite short series. [Steve Harangozo (Reviewer's comment ID #: 98-15)]	
3-318	A	13:28	13:28	All of these studies are contaminated with socioeconomic factors and other upard biases, such as have been identified by McKitrick, R and P J Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173 [VINCENT GRAY (Reviewer's comment ID #: 88-370)]	
3-319	A	13:31	13:45	I think it is very problematic to be dividing the record up at 1945. First, the raw data during the war years are really quite suspect, and large adjustments (e.g., a degree or two for nighttime marine air temperature) have, as I understand it, often been made; there have also been rather significant changes in spatial coverage of the data--it is really a bit surprising that the error bounds on the data are not larger during the war years, and having	

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				confidence that things are right to a tenth of a degree or two seems quite problematic to me. Second, I would think that the calculation of these trends should be based on the time-averaged curves, not one year results--and 1945 was a really unusual year--that right after the war, things turned around seems to me likely more than coincidence (indeed, it seems emblematic of a problem with the data). I also believe that in looking at long-term climate change, one should be able to get the same sense of the changes by blocking out any short section of the record--interestingly, blotting out the years covering WWII, when data were most suspect and are most adjusted, actually rather dramatically changes one's impression of the 20th century record--this is not the case for any similar period except perhaps well back in the 19th century when we know coverage was quite poor. Starting with about 1910 also seems to me to potentially introduce bias due to the strong volcanic eruptions during the first decade of the 20th century. So, I think that this first warming period is really being over dramatized as the time history is quite different than for the later warming, which could as accurately as for the first period, be said to extend from about 1950 to the present (see Figure SPM-3), accelerating over this time. Note also that here the rate of warming is given per decade--whereas elsewhere rates are given per century or per year--in the cases here I would urge instead indicating the amount of warming over the period. [Michael MacCracken (Reviewer's comment ID #: 152-258)]	
3-320	A	13:32	13:32	Replace "remarkably" by "fairly" [VINCENT GRAY (Reviewer's comment ID #: 88-371)]	
3-321	A	13:41	14:45	Similar remarks where appropriate about the REML method to those above. [Chris Folland (Reviewer's comment ID #: 71-15)]	
3-322	A	14:16	:23	Why include much discussion of trends computed over long periods when the records are better characterized by the variability of the embedded shorter period trends? Readers will concentrate on the long-term trends which when considerable shorter-term variability is present will be strong functions of the conditions at the start and end of the record and not indicative of the important changes. Thus, the discussion of this type of long-term trends should be limited in the text. [Govt. of United States of America (Reviewer's comment ID #: 2023-196)]	
3-323	A	14:18	14:19	It would be useful to elaborate a bit more on where the 0.65 figure comes from (e.g. 'the observed 0.65°C warming over the last century'). [Blair Trewin (Reviewer's comment ID #: 266-19)]	
3-324	A	14:26	14:26	0.7 instead of 0.65 [Govt. of Spain (Reviewer's comment ID #: 2019-22)]	
3-325	A	14:26		Why this formulation "slightly more than …"? What is the best estimate? [Fons Baede (Reviewer's comment ID #: 9-27)]	

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3-326	A	14:27	14:27	Replace "the 11th Century" with "the 16th Century, a rise associated with measurements made cloae to human habitation, with the resultant increases in building and enertgy usage" The 14th Century was certainly evn higher than the human induced recent surface record [VINCENT GRAY (Reviewer's comment ID #: 88-372)]	
3-327	A	14:29	14:29	Insert after "then", changes which could not possibly be attributed to increases in greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-373)]	
3-328	A	14:29	14:31	This analysis is simply misleading. The temperature rise since 1850 has not been smooth, in fact, as has been pointed out in lines 28-29, temperatures during the period from the mid-1940s to the mid-1970s were flat or even decreased slightly. The sum of the rise prior to the decrease and after the decrease will always add up to more than the total linear rise! This is a characteristic of any rising dataset that is interrupted in the middle by a leveling off or a decrease. You calculate that the 2001-2005 average is 0.78ºC above the 1850-1919 mean and go on to state that 0.5ºC of that occurred since the mid-1970s. This leaves the impression that the rise ending in the mid-1940s was only about 0.28ºC, but, in fact that is wrong. The five-year average from 1941-1945 is about 0.40ºC above the 1850-1919 mean—or, in other words, a full half of the overall rise occurred prior to the mid-1940s. The only reason that there appears to be a greater rise between the mid-1970s and current is that the temperatures are lower in the mid-1970s than during the mid-1940s. You can't count the decline against the earlier rise. For example, in a regular saw-tooth time-series that started at a low, then rose to a high, then went back to the low, and then ended on a high, the sum of the changes from the each low to each high would be twice the total linear rise. And if you only reported the value of the second increase, it would equal the total rise and leave no hint that a similar rise had occurred previously. Thus, leaving the reader with a false impression of what has taken place. This is the impression left by the discussion in this paragraph (lines 25-39). [Patrick Michaels (Reviewer's comment ID #: 176-2)]	
3-329	A	14:33	14:33	Delete "Section 3.2.2.2 and" [VINCENT GRAY (Reviewer's comment ID #: 88-374)]	
3-330	A	14:33	14:33	Insert after "the oceab" "the upward bias from ocean measurements must be comparable with" [VINCENT GRAY (Reviewer's comment ID #: 88-375)]	
3-331	A	14:34	14:34	Replace "on these estimates is expected to be small" with "land-based data" [VINCENT GRAY (Reviewer's comment ID #: 88-376)]	
3-332	A	15:1	15:3	Why this confusing sentence? Is this a convincing explanation for the lack of warming? The warming at many other locations could also be the result of changes in atmospheric	

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				circulation. [Fons Baede (Reviewer's comment ID #: 9-28)]	
3-333	A	15:5	15:6	"In the recent period, some regions have warmed substantially while a few have cooled slightly on an annual basis (Figure 3.2.9). Southwest China has cooled since the mid-20th Century." I do not see this cooling of Southwest China in Figure 3.2.9. Should I be able to see this in the fig.? [Melinda Marquis (Reviewer's comment ID #: 162-44)]	
3-334	A	15:6		The cooling in Southwest China is invisible from fig. 3.2.9 [Fons Baede (Reviewer's comment ID #: 9-29)]	
3-335	A	15:13	15:13	what is "SON"? Need to spell out as "September, October, and November". [Menglin Jin (Reviewer's comment ID #: 118-8)]	
3-336	A	15:17	15:19	A reference to Simmons et al.(2004) could be given at the end of the sentence that spans these lines. As noted in the comment above, Simmons et al. did discuss the difference in trends when computed with full data coverage and when sampled as in Jones and Moberg, and showed maps of both. Time series were included in a longer version of the work published by ECMWF as an ERA-40 Report. The northern hemisphere ERA-40 trend increases from 0.27K/decade (cf 0.30K/decade in CRUTEM2V) to 0.32K/decade when all land areas are included. [Adrian Simmons (Reviewer's comment ID #: 242-43)]	
3-337	A	15:27	15:28	The Australian Bureau of Meteorology has also undertaken trend analysis over the continent available at : http://www.bom.gov.au/cgi-bin/silo/reg/cli_chg/trendmaps.cgi , In these maps minimum temperatures in Western Australia show an increasing trend, not a decreasing trend, since 1950. Similarly for 1910-present. For trends since 1970 there is a region with decreasing minimum temperatures. Similarly, for maximum temperatures, the region in NW Australia with a cooling trend shows only a small trend (<0.1C/dec). Suggest removing references to Australia in these lines, or stating "The changes reported for minimum temperature for Australia differ from those calculated by the Australian Bureau of Meteorology using a locally developed high quality dataset. However recent data suggest an increasing trend in Australian DTR since the mid 1990's." [Govt. of Australia (Reviewer's comment ID #: 2001-200)]	
3-338	A	15:45	15:45	Insert the whole section 3.4.1. from page 3-25 line 3 to page 3-31 line 8. It is essential that the temperature records of the surface and of the free atmosphere should be placed adjacent to one another as they influence interpretation of global tewmperature change. By placing them so far apart it is possible to caonceal the fact that the temperature changes in the free atmosphere do not confirm the pattern found on the surface, so that pattern must be unrelated to changes in radiative forcing. [VINCENT GRAY (Reviewer's comment ID #: 88-380)]	

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3-339	A	15:46		Section 3.3. The title does not map well onto the subsection headings, and the subsection headings might not be optimal. "Drought" as used here is essentially a functional of precipitation (with temperature entering also), rather than a separate observable variable; perhaps it should be included within the precipitation section. "Hydrology" is an ambiguous term, better replaced by observables such as soil water and stream flow. This would be consistent with the report's avoidance of the vague and semantically incorrect term "climatology" to describe the statistical characteristics of a set of climate variables. [P.C.D. Milly (Reviewer's comment ID #: 179-6)]	
3-340	A	15:50	16:5	Delete this whole paragraph. It assumes that the increase in surface temperture is entiely due to "radiative forcig" which "expcts" certain results. This is by no means established. The absence of evidence of "radiative forcing in the MSU readings (from 1979 to 1999) and in the radiosonde records in the ;ower troposphere, indicate that the temperature rise shown by the surface record must be due to factors related to hun\man activity in the vicinity of the thermometer sites [VINCENT GRAY (Reviewer's comment ID #: 88-377)]	
3-341	A	15:54	15:57	The explanations on the effects of aerosols are on atttibution, and again are suitabel for Chapter 9 and not in Chapter 3 [Jose Marengo (Reviewer's comment ID #: 159-4)]	
3-342	A	15:54	15:54	Given that Celsius is used throughout should not this be expressed in Celsius rather than Kelvin? [Ian Simmonds (Reviewer's comment ID #: 241-4)]	
3-343	A	15:54	15:54	After 0.7% K-1, insert: "(see Question 3.2)" [Govt. of Spain (Reviewer's comment ID #: 2019-23)]	
3-344	A	15:55		Here you should cite Held and Soden (2000) as a good review of this entire concept of warming effects on the water cycle [Held, I.M., Soden, B, J., 2000. Water vapor feedback and global warming. Annual Review of Energy and the Environment 25, 441-475.] Also in this passage Huntington 2006 should be cited as a review of evidence that is consistent wth an ongoing intensification of the hydrologic cycle. Huntington, T. G. 2006, Evidence for intensification of the global water cycle: review and synthesis, Journal of Hydrology, 319:83-95. [Govt. of United States of America (Reviewer's comment ID #: 2023-197)]	
3-345	A	16:18	20:54	Section 3.3.2 As noted in the general comments above, the use of linear trends with respect to changes in rainfall pattern seems problematic as this generally oversimplifies the situation in a way that is often misleading, as well as not sufficiently taking into account multi-decadal variability. As an example, the linear trend rainfall for SE Australia from 1950 - 2000 shows a decline (the map of which often appears in popular articles on climate change), while the linear trend from 1900 - 2000 shows an increase. Study of the	

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				record shows a sharp transition between two "regimes" around 1947. This change in rainfall may be better represented as a change between two "states" - a dry period and a wet period - rather than by a linear trend. (Cont. below) [Govt. of Australia (Reviewer's comment ID #: 2001-201)]	
3-346	A	16:18	20:54	Also the relative shortness of the record and the occurrence of multi-decadal shifts means that any linear trends are very sensitive to the starting point (in or out of a "dry period"), and this makes it difficult to discern a climate change signal from a "natural" multi-decadal cycle (the "noise vs signal" problem). In summary, the use of linear trends in this section could be misleading in not sufficiently acknowledging that the response of rainfall to global warming at a particular location may not be a linear one. [Govt. of Australia (Reviewer's comment ID #: 2001-202)]	
3-347	A	16:20	18:52	The narrative leaves one with a strong sense of inconclusiveness. Suggest adding this paragraph at the end of section 3.3.2.1: [Govt. of United States of America (Reviewer's comment ID #: 2023-199)]	
3-348	A	16:20	18:52	"A plausible hypothesis to explain the equivocal trend statistics on global and regional rainfall trends based on a century or less of precipitation data is that the effects of greenhouse gases have not yet risen above the level of natural multidecadal variability having time scales that rival the lengths of the data records. The effects of the Atlantic ocean temperatures on multidecadal Sahel rainfall shifts are well known (Folland, 1986). There are indications for North America that multidecadal variations in precipitation are associated with natural oscillations in Pacific and North Atlantic sea surface temperatures (PDO, AMO, see section 3.6.6) (Enfield et al. 2001; McCabe et al. 2004) and at least one modeling study supports these findings and extends them to western Europe (Sutton and Hodson 2005). Figure 3.3.3 shows that multidecadal swings in precipitation are found at many locations around the world and that regional patterns are incoherently phased, making the identification of a global pattern impossible. Until the effects of greenhouse gases become dominant, or the data records sufficiently long, the identification of secular trends will probably remain uncertain." [Govt. of United States of America (Reviewer's comment ID #: 2023-200)]	
3-349	A	16:20	:23	Why include much discussion of trends computed over long periods when the records are better characterized by the variability of the embedded shorter period trends? Readers will concentrate on the long-term trends which when considerable shorter-term variability is present will be strong functions of the conditions at the start and end of the record and not indicative of the important changes. Thus, the discussion of this type of long-term trends should be limited in the text.) [Govt. of United States of America (Reviewer's comment ID #: 2023-198)]	
3-350	A	16:24		Does "statistically insignificant" mean "not statistically significant" (as used for example	

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				in Chapter 4, page 4-11, line18) or does it mean the trend is judged to be very small with a high statistical degree of confidence? If it is the former, I would prefer "not statistically significant" to be used. [Adrian Simmons (Reviewer's comment ID #: 242-66)]	
3-351	A	16:26	:27	Drop the sentence "Also the global land mean is not a very meaningful quantity as it is made up of much larger regional anomalies of opposite sign." since the global land mean is meanignful. It tells about global water cycle. It may be replaced by "The global mean land surface precipitation changes may not reflect local and regional changes, though." [Jürgen Grieser (Reviewer's comment ID #: 89-5)]	
3-352	A	16:29	16:42	too may technique details on data set. Suggest to move it to footnotes, as the main etxt only discuss the results. [Menglin Jin (Reviewer's comment ID #: 118-9)]	
3-353	A	16:38	16:38	Please replace "VASClim" by "VASClimO" as this is the name of the data set resulting from the research project VASClimO (Variability Analysis of Surface Climate Observations). [Christoph Beck (Reviewer's comment ID #: 17-1)]	
3-354	A	16:38	16:38	delete "VasClim", insert "VASClimO" [Govt. of Germany (Reviewer's comment ID #: 2011-3)]	
3-355	A	16:38		replace "VasClim" by "VASClimO" which is the correct name of the project and means Variability Analysis of Surface Climate Observations [Jürgen Grieser (Reviewer's comment ID #: 89-1)]	
3-356	A	16:40	3:16	It is helpfull to give more information about different application of these datasets. [Govt. of Germany (Reviewer's comment ID #: 2011-4)]	
3-357	A	16:44	16:47	The periods are 1951-2005 and 1979-2005, not 2004; see table 3.4 [Fons Baede (Reviewer's comment ID #: 9-30)]	
3-358	A	16:52	:54	Drop the sentence "This suggests …". It is pure speculation. One can hypothesize the other way around that the autocorrelation of CRU data is lower because of continous change of stations used. [Jürgen Grieser (Reviewer's comment ID #: 89-6)]	
3-359	A	16:56	17:2	delete "VasClim", insert "VASClimO [Govt. of Germany (Reviewer's comment ID #: 2011-5)]	
3-360	A	17:0		Table 3.4.a: Please replace "VASClim" by "VASClimO" as this is the name of the data set resulting from the research project VASClimO (Variability Analysis of Surface Climate Observations). [Christoph Beck (Reviewer's comment ID #: 17-2)]	
3-361	A	17:0		Table 3.4.a: Rudolf et al. 1994 is not the correct reference for the GPCC VASClimO data	

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				set. The correct reference is: Beck et al. 2005 - Beck, C., J. Grieser and B. Rudolf (2005): A new monthly Precipitation Climatology for the global land areas for the period 1951 to 2000. Climate Status Report, 2004: 181-190, German Meteorological Service – available via http://www.dwd.de/de/FundE/Klima/KLIS/prod/KSB/ksb04/28_precipitation.pdf [Christoph Beck (Reviewer's comment ID #: 17-3)]	
3-362	A	17:0		Table 3.4.a: The GPCC v.3 data set covers the period from 1951 to 2004, not from 1951 to 2002 [Christoph Beck (Reviewer's comment ID #: 17-4)]	
3-363	A	17:0		Table 3.4.b: Please replace "VASClim" by "VASClimO" as this is the name of the data set resulting from the research project VASClimO (Variability Analysis of Surface Climate Observations). [Christoph Beck (Reviewer's comment ID #: 17-5)]	
3-364	A	17:0		Table 3.4.b: The GPCC v.3 data set covers the period from 1951 to 2004, it does not end at 2002 [Christoph Beck (Reviewer's comment ID #: 17-6)]	
3-365	A	17:1	:7	replace 2 times "VasClim" by "VASClimO" which is the correct name of the project and means Variability Analysis of Surface Climate Observations [Jürgen Grieser (Reviewer's comment ID #: 89-2)]	
3-366	A	17:1	:1	replace "Spatial infilling" by a more common word like "Raster data" or "Spatial interpolation" [Jürgen Grieser (Reviewer's comment ID #: 89-4)]	
3-367	A	17:15	17:	In section 3.3.2.2. Suggest to introduce that some evidence of regional changes in the precipitation regimes are being observed in the last 50 years showing local to regional spatial changes as well as an amplification of the drougth / wet cycles (with wet cycles including extreme events (see H12. It seems by exploring better the origing of the precipitation events a regional scale some interesting process based conclusions can be gather. [Govt. of Spain (Reviewer's comment ID #: 2019-7)]	
3-368	A	17:15	18:52	The discussion in this section is very focused on averages for the regions defined in 11.3.1. As such, a number of important smaller-scale features have been missed (for example, in Australia, the division between northern and southern Australia means that the strong increasing trend in NW Australia and the strong decreasing trend in NE Australia since 1970 cancel each other out). Most importantly, there is no mention of the marked decline in precipitation (as indicated in Fig. 3.3.2) in most west coast areas centred on latitude 30-35 degrees in both hemispheres (e.g. SW Australia, W South Africa, Chile, Spain/Portugal/Morocco). Any changes here may also potentially flow through into the Executive Summary of this section, and into the TS and the SPM.	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-203)]	
3-369	A	17:38	18:9	There is no indication of the sources of th data presented in Fig. 3.3 (time series of annual precipitation for 19 regions) [Govt. of France (Reviewer's comment ID #: 2010-26)]	
3-370	A	17:38	18:9	In order to give evidence of the very high spatial variability of precipitation trends it could be useful to give reference to Brunetti, M., Maugeri, M., Nanni, T., Auer, I., Böhm, R., Schöner, W. 2005: Precipitation variability and changes in the greater Alpine region over the 1800-2003 period. Journal of Geophysical Research – Atmosphere, in press. This paper highlights that also in a small region as the Alpine one there are significant differences in long-term trends. [Teresa Nanni (Reviewer's comment ID #: 186-1)]	
3-371	A	17:40	18:1	Delete this list of regions. It is just a repetition of the caption of fig 3.3.3 [Fons Baede (Reviewer's comment ID #: 9-31)]	
3-372	A	18:21	18:26	For the purpose of the graphs, it may be better to split Australia east/west rather than north/south, as this tends to be the more important orientation of the divide, and shows the Pacific vs Indian Ocean influence. [Govt. of Australia (Reviewer's comment ID #: 2001-204)]	
3-373	A	18:25	18:26	It could be made clearer that the downward trend in SW Australia was a relatively abrupt transition around 1975 rather than a gradual trend over the last 30 or so years. Also should a comment on SE Australia be included in this paragraph? (eg, an abrupt upward shift around 1947, with the 50's and 70's being particularly wet, and with a more recent shift to drier conditions since around 2000). Reference Smith, Ian 2004. 'An assessment of recent rends in Australian rainfall, Aust Met Mag, 53 p163-173.. [Govt. of Australia (Reviewer's comment ID #: 2001-205)]	
3-374	A	18:36	18:39	Within this context, the results described in Brunetti et al. 2006 (Brunetti, M., Maugeri, M., Nanni, T., Auer, I., Böhm, R., Schöner, W. 2006: Precipitation variability and changes in the greater Alpine region over the 1800-2003 period. Journal of Geophysical Research – Atmosphere, Vol 111, doi:10.1029/2005JD006674) help to highlight both the different long-term behaviour of precipitation nothward and southward the Alps (with an increase in the total precipitation amount north of the Alpine chain, and a highly significant decrease south of the Alps), but also the different NAO-precipitation relationship in the two sides of the Alpine chain. It is interesting to highlight an existing marked influence of the Alps with respect to NAO-precipitation-correlation, with a clear transition between strong and weak NAO-influence which is very sharp along the zonal part of the Alpine chain [Michele Brunetti (Reviewer's comment ID #: 33-1)]	
3-375	A	18:54	19:29	This section should be consistent with but not overlap with Chapter 4.	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-206)]	
3-376	A	19:3	19:3	Presumably it's 'up until 1995' because that's when the published analysis ends? [Blair Trewin (Reviewer's comment ID #: 266-20)]	
3-377	A	19:31	20:8	The important points from this section are that urban environments can have a local and downwind effect on some climate variables which should not be confused with climate change effects arising from changes in atmospheric greenhouse gas concentrations. This could be stated more concisely than in the present text. [Govt. of Australia (Reviewer's comment ID #: 2001-207)]	
3-378	A	19:35	19:35	Insert after "20 to 70 Wm to minus 2" "These enrgy outputs obviously affect temperture readings and contribute to the upwards bias of the surface temperture record. [VINCENT GRAY (Reviewer's comment ID #: 88-378)]	
3-379	A	19:56	19:56	This process was strong suggested as dominant by Simmonds, I., and K. Keay, 1997: Weekly cycle of meteorological variations in Melbourne and the role of pollution and anthropogenic heat release. Atmospheric Environment, 31, 1589-1603. [Ian Simmonds (Reviewer's comment ID #: 241-5)]	
3-380	A	20:4	20:4	the comma is inappropriately placed – either there should be no comma in this line at all or it should be moved to after 'United States'. [Blair Trewin (Reviewer's comment ID #: 266-65)]	
3-381	A	20:45	20:46	This sentence needs rewording. Suggest 'During El Niño events, area-averaged precipitation increases over the ocean but decreases over land areas'. [Blair Trewin (Reviewer's comment ID #: 266-21)]	
3-382	A	20:48	:54	Authors should note that the ocean salinity data is consistent with an incease in rainfall over oceans at high latitudes (see Curry, R.G., Dickson, R.R., Yashayaev, I., 2003. A change in the freshwater balance of the Atlantic Ocean over the past four decades. Nature 426, 826-829.) [Govt. of United States of America (Reviewer's comment ID #: 2023-201)]	
3-383	A	20:51	20:51	'Northern mid-latitudes' are not defined here. [Blair Trewin (Reviewer's comment ID #: 266-22)]	
3-384	A	21:7	21:19	(Also affects 3-4, lines 48-56). Note that two very recent studies in Australia have independently attributed most observed changes in pan evaporation to changes in wind run (whether due to real climate changes or changes in site exposure) at pan level. These studies cannot be included in this section as they were published after the deadline (Feb and Apr 2006), but they will make the WG II deadline, so it is important that material in this section is not inconsistent with conclusions which may be drawn and included in WG II. With this in mind, suggest amending the wording in line 12 from 'any trends being more likely caused' to 'causes cited for the observed trends include'. This leaves the way	

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				open for additional causes to be added in later reports if necessary. [Govt. of Australia (Reviewer's comment ID #: 2001-208)]	
3-385	A	21:12	21:12	The semicolon should be a comma. [P.C.D. Milly (Reviewer's comment ID #: 179-7)]	
3-386	A	21:16	:19	This section should cite Walter et al.2004 that convincingly shows that for the conterminous USA that increases in precipitation have been much greater than in runoff – that indicates that evapotranspiration increased quite substantially. Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. [Govt. of United States of America (Reviewer's comment ID #: 2023-202)]	
3-387	A	21:21		The evidence for a lengthening of the growing season is consistent with an increasing ET because not only do you need moisture and energy for ET but on vegetated lands you need leaves with open stomates. For most of the northern hemisphere there are consistent reports of lengthening of the growing season by around 2 to 3 weeks in the 20th century. See for example the following refs. [Govt. of United States of America (Reviewer's comment ID #: 2023-203)]	
3-388	A	21:21		Menzel, A., Fabian, P., 1999. Growing season extended in Europe. Nature 397, 659. [Govt. of United States of America (Reviewer's comment ID #: 2023-204)]	
3-389	A	21:21		White, M.A., Running, S.W., Thornton, P.E., 1999. The impact of growing-season length variability on carbon assimilation and evapotranspiration over 88 years in the eastern US deciduous forest. International Journal of Biometeorology 42, 139 - 145. [Govt. of United States of America (Reviewer's comment ID #: 2023-205)]	
3-390	A	21:21		Schwartz, M.D., Reiter, B.E., 2000. Changes in North American spring. Intl. J. Climatol. 20, 929-932. [Govt. of United States of America (Reviewer's comment ID #: 2023-206)]	
3-391	A	21:21		Wolfe DW, Schwartz MD, Lakso A, Otsuki Y, Pool R, Shaulis N (2005) Climate change and shifts in spring phenology of three horticultural woody perennials in northeastern USA. Internat. J. Biometeor. 49: 303-309. [Govt. of United States of America (Reviewer's comment ID #: 2023-207)]	
3-392	A	21:31		Section 3.3.4. The title seems misleading and, on the basis of content, should refer to PDSI and LSMs instead of drought. However, the PDSI and LSM results are essentially precipitation results, and should seemingly be included in the precipitation section. The LSM-simulated soil moisture depends strongly on the precipitation reconstructions, whose quality is surely inadequate for global trend assessment; the input radiation time series are presumably even worse (from a trend standpoint). [P.C.D. Milly (Reviewer's comment ID #: 179-8)]	

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3-393	A	21:31		Section 3.3.4. The discussion of streamflow might benefit by reference to Figure 2a of Milly et al. (2005, Nature, 438, p 347), which gives a consistent, global picture of streamflow trends in many regions of the world (including some of those highlighted piecemeal in the draft, such as La Plata basin, Sahel, high latitudes--others that are also worthy of mention are decreased flow in southern Europe and in all but northern Australia), based on consistent methods, and because of the relevance of the paper to issues, treated elsewhere, of trend detection and attribution. Certainly it is more relevant than the reference to Milly et al. (2002) on page 3-23, line 14. [P.C.D. Milly (Reviewer's comment ID #: 179-11)]	
3-394	A	21:36	21:38	This result seems inconsistent with that on page 22, lines 7 to 18. [Chris Folland (Reviewer's comment ID #: 71-16)]	
3-395	A	21:40	21:53	This paragraph seems to say little and could be omitted. Section 3.3.4 as a whole is too long for its useful content compared to the general standard of the rest of the chapter. [Chris Folland (Reviewer's comment ID #: 71-17)]	
3-396	A	21:47	:51	Walter et al 2004 should be cited here for increasing precip, ET and streamflow for the conterminous USA Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. [Govt. of United States of America (Reviewer's comment ID #: 2023-208)]	
3-397	A	21:55	21:55	This sentence should introduce not just this paragraph but also the previous one. [P.C.D. Milly (Reviewer's comment ID #: 179-9)]	
3-398	A	21:55	24:7	The conclusions of this critical section are all based on the study by Dai et al. 2004 which used the PDSI (this is OK) but coupled this with potential evapotranspiration estimates computed using the Thornthwaite approach. The latter is not OK for reasons listed in Box 3.1, the advantage of the Thornthwiate approach is that it is easy to do the calculations. The disadvantage is that we know it is bad physics, and we know that it is wrong because it predicts increasing potential ET over time (in line with increasing air temperatures) while measurements (of pan evaporation) and calculations using a Penman approach show decreases (e.g. Chen et al. 2005, Climate Research, 28: 123-132). [Michael Roderick (Reviewer's comment ID #: 218-6)]	
3-399	A	21:57		After first sentence of paragraph add, ""Vinnikov and Robock (2002) showed that while there has been a slight upward trend in the PDSI for the US for the past century, there has been no trend in its variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-15)]	
3-400	A	22:22	22:22	insert comma after 'SSTs'.	

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				[Blair Trewin (Reviewer's comment ID #: 266-66)]	
3-401	A	22:24	22:24	should be Keetch (not Keech). [Blair Trewin (Reviewer's comment ID #: 266-67)]	
3-402	A	22:30	22:31	Suggest rewording this sentence: 'Although there was no significant trend over the full 1880-1998 period during summer (JJA) in eastern China, precipitation for 1990-1998 was higher than that for any other period of comparable length (Gong and Wang, 2000)'. [Blair Trewin (Reviewer's comment ID #: 266-5)]	
3-403	A	22:31	22:31	delete comma after 'Zou et al.'. [Blair Trewin (Reviewer's comment ID #: 266-68)]	
3-404	A	22:37	22:37	Can we be more specific than 'recent years'? [Blair Trewin (Reviewer's comment ID #: 266-23)]	
3-405	A	22:43	22:45	This sentence should be reworded and updated to capture different periods of rainfall decline. Suggest: 'There have been marked multi-year rainfall deficits since the mid- to late 1990s in several parts of Australia, particularly the far southwest, parts of the southeast and along sections of the east coast'. The reference to the June 2005 rains can be deleted as they are of only trivial importance to the multi-year deficits under discussion here (their main impact was to remove the possibility of a widespread growing-season drought in 2005). [Govt. of Australia (Reviewer's comment ID #: 2001-209)]	
3-406	A	22:53	24:7	In this text three terms are used: streamflow, run-off and river discharge. Do they have the same meaning? If so, use just one term: runoff; if not, the differences should be made clear. [Fons Baede (Reviewer's comment ID #: 9-32)]	
3-407	A	23:4	23:17	There is no mention of streamflow changes in Australia - although important work has been done e.g. by Bates et al in using streamflow in southwest WA as an indicator for changes in the local precipitation regime eg under the Indian Ocean Climate Initiative. Reference 'Climate variability and change in south west Western Australia.' Indian Ocean Climate Initiative. September 2002.. [Govt. of Australia (Reviewer's comment ID #: 2001-210)]	
3-408	A	23:4	:17	This paragraph leaves the impression that flooding has increased. Please cite these papers to the contrary; [Govt. of United States of America (Reviewer's comment ID #: 2023-209)]	
3-409	A	23:4	:17	USA (McCabe and Wolock, 2002; Vogel et al., 2002), Canada (Zhang et al., 2001b), Scandinavia (Lindstrom and Bergstrom, 2004; Hyvarinen, 2003), or central Europe (Mudelsee et al., 2003) [Govt. of United States of America (Reviewer's comment ID #: 2023-210)]	

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3-410	A	23:4	:17	Kundzewicz, Z.W., D. Graczyk, T. Maurer, I. Piskwar, M. Radziejewski, C. Svensson, and M. Szwed. 2005. Trend detection in river flow series: 1. Annual maximum flow. Hydrol. Sci. J. 50:797-810. [Govt. of United States of America (Reviewer's comment ID #: 2023-211)]	
3-411	A	23:4	:17	Vogel, R., Zafirakou-Koulouris, A., Matalas, N.C., 2002. Frequency of record-breaking floods in the United States. Water Resour. Res. 37, 1723-1731. [Govt. of United States of America (Reviewer's comment ID #: 2023-212)]	
3-412	A	23:4	:17	McCabe, G.J., Wolock, D.M., 2002. A step increase in streamflow in the conterminous United States. Geophys. Res. Lett. 29(24), 2185, doi:10.1029/2002GL015999,2002. 29, 38-1 to 38-4. [Govt. of United States of America (Reviewer's comment ID #: 2023-213)]	
3-413	A	23:4	:17	Lindstrom, G., Bergstrom, S., 2004. Runoff trends in Sweden 1807-2002. Hydrol. Sci. J. 49, 69-83. [Govt. of United States of America (Reviewer's comment ID #: 2023-214)]	
3-414	A	23:4	:17	Hyvarinen, V., 2003. Trends and characteristics of hydrological time series in Finland. Nordic Hydrology 34, 71-90. [Govt. of United States of America (Reviewer's comment ID #: 2023-215)]	
3-415	A	23:4	:17	Zhang, X., Harvey, K.D., Hogg, W.D., Yuzyk, T.R., 2001b. Trends in Canadian stream flow. Wat. Resour. Res. 37, 987-998. [Govt. of United States of America (Reviewer's comment ID #: 2023-216)]	
3-416	A	23:4	:17	Mudelsee, M., Börngen, M., Tetzlaff1, G., Grünewald, U., 2003. No upward trends in the occurrence of extreme floods in central Europe. Nature 425, 166 - 169. [Govt. of United States of America (Reviewer's comment ID #: 2023-217)]	
3-417	A	23:6	23:6	later' should be 'latter' [Ian Simmonds (Reviewer's comment ID #: 241-6)]	
3-418	A	23:8		This should include Garcia and Mechoso (2006) for increases in streamflow for all of South America Garcia, N.O., and C.R. Mechoso. 2006. Variability in the discharge of South American rivers and in climate. Hydrological Sciences Journal 50:459-478. [Govt. of United States of America (Reviewer's comment ID #: 2023-218)]	
3-419	A	23:9	23:10	On p. 17 of this chapter, it was noted that precipitation increased over Canada, the opposite of what has been stated here. [Michael Roderick (Reviewer's comment ID #: 218-7)]	
3-420	A	23:13	23:17	This passage seems to belong in the section on extremes and not here. [P.C.D. Milly (Reviewer's comment ID #: 179-10)]	
3-421	A	23:15	23:17	This conclusion comes out of nowhere! After reading the past two-to-three pages about differing precipitation, soil moisture, and stream flow trends all over the place, I was quite	

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				surprised to read "The global increase in both sever drought and large floods suggest that hydrologic conditions have become more extreme." Apparently my definition of "global" is quite different from yours. [Patrick Michaels (Reviewer's comment ID #: 176-3)]	
3-422	A	23:15	23:17	Where are the severe droughts, see comments 1-7 above. [Michael Roderick (Reviewer's comment ID #: 218-8)]	
3-423	A	23:19	:25	This paragraph is incomplete in its treatment of ice break up. In addition to Smith and Zhang you could have cited the following papers that show the geographic extent of these trends: [Govt. of United States of America (Reviewer's comment ID #: 2023-219)]	
3-424	A	23:19	:25	Beltaos, 2002, Hydrol. Proc. 16:789-804 [Govt. of United States of America (Reviewer's comment ID #: 2023-220)]	
3-425	A	23:19	:25	Borshch et al. 2001, Water Resour. 28:194-200 [Govt. of United States of America (Reviewer's comment ID #: 2023-221)]	
3-426	A	23:19	:25	Magnuson et al. 2000, Science 289:1743-1746. [Govt. of United States of America (Reviewer's comment ID #: 2023-222)]	
3-427	A	23:19	:25	Yoo & D'Odorico. 2002. J. Hydrol. 268:100-112. [Govt. of United States of America (Reviewer's comment ID #: 2023-223)]	
3-428	A	23:19	:25	Hodgkins et al. 2005 Climatic Change 71: 319-340 [Govt. of United States of America (Reviewer's comment ID #: 2023-224)]	
3-429	A	23:19	:25	Jasek, M J., 1999. Proc. 14th Intl. Symp. On Ice "Ice in Surface Waters" [Govt. of United States of America (Reviewer's comment ID #: 2023-225)]	
3-430	A	23:19	:25	Kuusisto &. Elo. 2000. Verh. Internat. Verein. Limnol. 27:2761-2764. [Govt. of United States of America (Reviewer's comment ID #: 2023-226)]	
3-431	A	23:19	:25	Include McClelland et al 2006. This paragraph is misleading, it suggests that the same caution should be applied to trends in Eurasian rivers draining to the Arctic Ocean as for agricultural areas in China or other parts of Asia where human influences are extreme. Also the increases in discharge in USA (Walter et al., 2004) and South America (Garcia and Mechosos, 2006) are in areas with major aricultural operations but in spite of this they show increases in discharge. [Govt. of United States of America (Reviewer's comment ID #: 2023-227)]	
3-432	A	23:19	:25	Garcia, N.O., and C.R. Mechoso. 2006. Variability in the discharge of South American rivers and in climate. Hydrological Sciences Journal 50:459-478. Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. [Govt. of United States of America (Reviewer's comment ID #: 2023-228)]	

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3-433	A	23:19	:25	McClelland, J., S.J. Dery, B.J. Peterson, R. Holmes, and E.F. Wood. 2006. A pan-arctic evaluation of changes in river discharge during the latter half of the 20th century. Geophysical Research Letters 33:10.1029/2006GL025753. [Govt. of United States of America (Reviewer's comment ID #: 2023-229)]	
3-434	A	23:27	23:27	"climate" can be deleted. [Chris Folland (Reviewer's comment ID #: 71-18)]	
3-435	A	23:36	23:36	Cross refer to Fig. 3.3.3 [Chris Folland (Reviewer's comment ID #: 71-19)]	
3-436	A	23:48	:53	To be parallel with the paragraphs above it should be noted that SST and teleconnections play a large role in Sahelian Rainfall. (see refs below) [Govt. of United States of America (Reviewer's comment ID #: 2023-230)]	
3-437	A	23:48	:53	Bader, J., and M. Latif, The impact of decadal-scale Indian Ocean sea surface temperature anomalies on Sahelian rainfall and the North Atlantic Oscillation, Geophys. Res. Lett., 30(22), 2169, doi:10.1029/ 2003GL018426, 2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-231)]	
3-438	A	23:48	:53	Giannini, A., Saravanan, R. and Chang, P. 2003. Oceanic forcing of Sahel Rainfall on interannual to interdecadal timescales, Science 302, 1027-1030. [Govt. of United States of America (Reviewer's comment ID #: 2023-232)]	
3-439	A	23:48	:53	Lu, J., and T. Delworth, 2005: Oceanic forcing of late-20th Century drought in the Sahel/Geophys. Res. Lett, submitted. [Govt. of United States of America (Reviewer's comment ID #: 2023-233)]	
3-440	A	23:55	23:57	PDSI using the inccorrect Thornthwaite approach show increasing droughts. [Michael Roderick (Reviewer's comment ID #: 218-9)]	
3-441	A	24:16	24:16	Box 3.1. This box is heavily biased towards drought indicators based on the Palmer Index. Many countries use percentiles and decile ranges to describe prolonged periods of rainfall deficiencies as drought indicators, particularly for meteorological drought. Suggest adding an additional paragraph: 'Other indices are also used for drought monitoring. These include the occurrence of rainfall below a specified fixed threshold, or below a specified level in the historical frequency distribution (e.g. the 10th percentile).' [Govt. of Australia (Reviewer's comment ID #: 2001-211)]	
3-442	A	24:38	25:6	Section 3.3.5 This section is mostly didactic and hence unnecessary. Further, it mainly refers to relationships between temperature and precipitation on interannual timescales, not to the coherence of long-term changes. We suggest deletion of section 3.3.5. [Govt. of Australia (Reviewer's comment ID #: 2001-212)]	
3-443	A	24:38		Section 3.3.5. Although the link between lack of soil moisture and recent extreme temperature events has been mentioned with reference to specific events e.g.page 70, lines	

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				31-33 and page 71, lines 50-52, and more indirectly at page 40, line 33, there appears to be no specific mention on the potential relationship between soil moisture defecit and extreme temperatures in the main text. This should be stressed and either inserted into the extremes section 3.8, or a some kind of link made between the extremes section and the relevant section on soil moisture. [John Caesar (Reviewer's comment ID #: 36-6)]	
3-444	A	24:38		Is there not also a feedback from aerosol concentration to precipitation (aerosol particles acting as condensation cores) ? [Govt. of Germany (Reviewer's comment ID #: 2011-125)]	
3-445	A	25:1		This relationship is not generally valid. So, insert after the word "precipitation" the words "over continents during the warm season". [Fons Baede (Reviewer's comment ID #: 9-33)]	
3-446	A	25:1		: 'This relationship' is opposite what has just been discussed (positive correlation between T and P). [David Rind (Reviewer's comment ID #: 214-20)]	
3-447	A	25:1		'This relationship' is opposite what has just been discussed (positive correlation between T and P), [need to clarify time period and areas]. [Govt. of United States of America (Reviewer's comment ID #: 2023-234)]	
3-448	A	25:3	31:8	This whole section (3.4.1) should be trnsferred to Page 3-15 line 45. It is essential that the temperature records of the surface and of the free atmosphere should be placed adjacent to one another as they influence interpretation of global tewmperature change. By placing them so far apart it is possible to caonceal the fact that the temperature changes in the free atmosphere do not confirm the pattern found on the surface, so that pattern must be unrelated to changes in radiative forcing. [VINCENT GRAY (Reviewer's comment ID #: 88-379)]	
3-449	A	25:10	31:8	Look, here's the deal. UAH has provided data for 15 years in a very easy way for everyone to access giving them the opportunity to scrutinize the data. There have been 7 revisions, and number 8 (version 6.0) is getting close to being finished (with a much more defensible and empirical diurnal correction, better signal-to-noise and trends which are well within current error bars.) UAH, of course, has discovered most of the changes needed through the years. However, RSS LT was only available in August 2005 and VG2 in Feb 2006. These datasets have not had the scrutiny of the community but have certainly received a warm welcome. Our two papers coming out this year are the first rigorous evaluation of RSS and the results clearly raise questions about RSS trends but will unfortunately not be noted in the AR4. VG2 data do not lend themselves to comparison analysis so I don't know how one can even do a decent job of evaluation … but the results will show some significant problems given what was shown in CCSP 1.1.	

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				So, my advice is to be as cautious as possible pending the potential contradictions that will be published this year in comparison with some of the statements in the present AR4 (see above). The CCSP 1.1 reports that there are still significant differences in tropospheric trends and press reports (i.e. Kerr in Science) did not present the correct picture. Contrary to the picture painted by Kerr, we did not sit around the campfire, hold hands and sing “kum bai ya” about a particular dataset. [John Christy (Reviewer’s comment ID #: 41-24)]	
3-450	A	25:12	25:22	Excellently done [John Christy (Reviewer’s comment ID #: 41-6)]	
3-451	A	25:12	25:22	Delete this whole paragraph. It is outrageous. It tries to cover up the very large upward bias in the surface record by suggesting that the tropospher temperature recordings are somehow inferior. By contrast with the surface record they are superior. The MSU record is truly global whereas the surface record is biased by its poor distribution over the earth's surface. The MSU record is much more accurate, and it has been the subject of very thorough correction, whereas the surface record has not. The radiosonde records are also rather unrepresentative, but is unfair to suggest that they are unreliable compared with the surface record [VINCENT GRAY (Reviewer’s comment ID #: 88-381)]	
3-452	A	25:15	25:21	Delete the sentences starting with "Historically" and ending with "ground-truth". This doesn't really belong here. [Melissa Free (Reviewer’s comment ID #: 76-1)]	
3-453	A	25:22	25:22	The CCSP (2006) reference on page 82 needs revision in line with guidance on how to reference this report, now given in its final version. [Chris Folland (Reviewer’s comment ID #: 71-20)]	
3-454	A	25:22		Is CCSP(2006) an acceptable reference according to IPCC rules? The report has only just been published in final form. If it is accepted, then reference to other recently published material, notably the Science paper of Turner et al. (see comment #51), should also be allowed. [Adrian Simmons (Reviewer’s comment ID #: 242-44)]	
3-455	A	25:28	26:2	Suggest substituting the following text: Comparisons of several adjustment methods showed that they gave disparate results when applied to a common set of radiosonde station data (Free et al. 2002). One of these methods, an approach based on the physics of heat transfer within the radiosonde, also performed poorly when evaluated against satellite temperature records (Durre et al. 2002). Another method, comparison with satellite data (HadRT, Parker et al. 1997) is limited to the satellite era and to events with available metadata, and causes a reduction in spatial consistency of the data. A comprehensive intercomparison (Seidel et al., 2004) showed	

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				that 5 radiosonde datasets yielded consistent signals for higher frequency events such as ENSO, QBO and volcanic eruptions, but inconsistent signals for long-term trends. The authors concluded that given these disparities in trends, multiple independent datasets are essential for assessment of longer-term change. Consistent with this need for multiple datasets, several approaches have been used to create new adjusted datasets since the TAR. The LKS (Lanzante et al. 2003a,b) dataset, using 87 carefully selected stations, has subjectively derived bias adjustments throughout the length of its record but terminates in 1997. It has been updated using the Integrated Global Radiosonde Archive (IGRA, Durre et al. 2006) by applying a different bias adjustment technique (Free et al. 2004b) after 1997, creating a new archive (Radiosonde Atmospheric Temperature Products for Assessing Climate, RATPAC). Another new radiosonde record, HadAT2 (successor to HadRT), uses a neighbor comparison approach to build spatial as well as temporal consistency. A third approach (Haimberger 2005) uses the bias-adjustments estimated during data assimilation into model-based reanalyses to identify and reduce inhomogeneities in radiosonde data. Despite the risk of contamination by other biased data or by model bias, the resulting adjustments agree with those estimated by other methods. Rather than adjusting the data, Angell (2003) tried to reduce data quality problems by removing several tropical stations from his radiosonde network. Despite these efforts to produce homogeneous datasets, two recent analyses of radiosonde data indicate that significant problems may remain. Sherwood... [Melissa Free (Reviewer's comment ID #: 76-4)]	
3-456	A	25:39	25:39	Angell was looking at unadjusted rather than LKS data, so not really relevant at this point. See below. [Melissa Free (Reviewer's comment ID #: 76-2)]	
3-457	A	25:40	25:43	Probably overstates the certainty of their conclusions. [Melissa Free (Reviewer's comment ID #: 76-3)]	
3-458	A	25:45	25:45	"...be applied 1979 to present." -> "...be applied from 1979 to present." [Pedro Ribera (Reviewer's comment ID #: 213-9)]	
3-459	A	25:53	25:53	remove full stop and reword 'apparent, so….'. [Blair Trewin (Reviewer's comment ID #: 266-69)]	
3-460	A	26:2	26:11	These studies shed light on relative differences between day/night and high/low quality radiosonde records. They do not address fundamental discrepancies which affect both day/night and high/low quality such as those documented in Christy and Spencer (2005,	

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				Science 310, pg 972) in which clear warm biases since 1979 appear in a significant portion of the radiosondes (detected by using both UAH and RSS satellite data). Responses in Science to Christy and Spencer do not challenge the numbers calculated in our letter. In other words, the two "adjusted" radiosonde results in the radiosonde papers mentioned here likely contain spurious warming. I would suggest adding one more sentence to this paragraph "However, known positive biases since 1979, for example in Australian radiosondes, may to some extent mitigate this apparent negative bias (Christy and Spencer 2005)." [John Christy (Reviewer's comment ID #: 41-7)]	
3-461	A	26:2	26:3	You need to make clear that Sherwood et al found this effect in the raw data and not in the homogenised datasets (they did not consider homogenised datasets at all). [Peter Thorne (Reviewer's comment ID #: 264-5)]	
3-462	A	26:2		The "finding" by Sherwood et al.(2005) that there has been a change over time in day-night difference in radiosonde temperatures is described as "a major new development". If this is really so from a climate community perspective, it shows a real disconnect with the NWP community, who have known about and corrected diurnally-varying radiosonde biases for many years, and have long been aware of changes over time in these biases. It was for this reason that in ERA-40 we paid particular attention to finding ways of correcting these biases, and devoted three of the Project Reports to this topic, the first of which, by Onogi, published in 2000, presented numerous time series showing trends (varying from country to country) in day-night differences in 100hPa temperatures due to the bias changes that resulted either from introduction of better sensors or from introduction of better adjustments of measurements by the station operators before insertion of data onto the GTS. [Adrian Simmons (Reviewer's comment ID #: 242-45)]	
3-463	A	26:2		Comment continued: For reasons outlined in Uppala et al.(2005), bias correction of radiosonde data was in fact applied in ERA-40 only from 1980 onwards, but this was probably a significant contributory factor to a better agreement between ERA-40 and MSU trends than between MSU and radiosonde trends for lower stratospheric temperature, despite radiance bias-correction problems in the early satellite years in ERA-40. This would have been evident had the ERA-40 curves shown by Santer et al.(2004) been included in Figure 3.4.2. [Adrian Simmons (Reviewer's comment ID #: 242-46)]	
3-464	A	26:10	26:11	Suggest substituting starting with "Night-time" the following; Randel and Wu used collocated MSU data to show that apparent cooling biases exist in some of the LKS/RATPAC adjusted radiosonde data for the tropical stratosphere and that these biases are likely to extend into the upper troposphere. They also identified problems in	

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				night data as well as day, indicating that negative biases are not limited to daytime observations. [Melissa Free (Reviewer's comment ID #: 76-5)]	
3-465	A	26:13		How about radiosonde stations on islands and from ships? [Fons Baede (Reviewer's comment ID #: 9-34)]	
3-466	A	26:19	26:19	Add at end. "Thorne et al (2005) have done an excellent job in resolving these difficulties (see Figures) The 500 hPg record from 1958 shows good agreement with the surface record and the MSU record in its detection of natural events, such as. Mt Agung (1962), Chichon (1981, and Pinatubo (1990), and the El Niño events of 1982 and 1998. It also shows clearly the fairly abrupt cool period between1965 and 1978 which also appears on the surface record and has been attributed to an ocean change. The rest of the record can therefore be trusted. Since it finds no evidence of an overall temperature change between 1958 and 2002, this means that there is no indication of radiative forcing as a result of greenhouse gas increases in the region where these are most expected rto be evident. The warming displayed in the surface record must therefore be caused by local surface effects from proximity of the measuring equipment to humann activity". [VINCENT GRAY (Reviewer's comment ID #: 88-382)]	
3-467	A	26:21	26:21	You MUST insert here a proper Figure showing the radiosonde records, preferably those from Figure 9 of the paper of Thorne et al (2005). Figure 3.4.2 is deliberately designed to conceal the true facts about both the radiosonde and the MSU records. The pretence that these three records are virtually identical is a plain lie. The true facts about the radiosonde record from 1958 to the present are 1. It gives a good representation of natural events such as such as. Mt Agung (1962), Chichon (1981, and Pinatubo (1990), and the El Niño events of 1982 and 1998. It also shows clearly the fairly abrupt cool period between1965 and 1978 which also appears on the surface record and has been attributed to an ocean change. It is therefore a reliable record of temperature change in the lower troposphere. 2. It shows that there was no temperature change between 1958 and 2004, so there is no evidence of an influence of greenhouse gas increases in the part of the atmosphere where it is supposed to happen. 3. This means that the temperature changes in the surface record are due to purely local surface effects, such as proxinity of the measuring equipment to human habitation. 3. It means that the assumption made by the models that greenhouse gas increases are responsible for all changes in climate is fundamentally wrong [VINCENT GRAY (Reviewer's comment ID #: 88-383)]	
3-468	A	26:40	26:	Change "decay" to "drift". [Qiang Fu (Reviewer's comment ID #: 78-6)]	
3-469	A	26:47	26:47	Add at end "The records, from 1978 to the present, agree with the radiosonde and surface records in showing the influence of natural events such as volcanic erupotions by Mt	

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				Chichon ((981) and Pinatubo (1990) and the El Niño events of 1982 and 1998. The 1998 event gave a particularly large peak. This means that the record is reliable, and probably the most reliable. since it is truly global, and has been subjected to thorough scrutiny. The finding, therefore , that there was no overall temperature change between 1978 and 1998 can be taken as proof, together with that of the radiosonde records, that there is no detectable radiative forcing in the lower atmosphere that might have resulted from increase in greenhouse gases, where the effects are the most prominent.. The temperature rise shown in the surface record since 1978 must, therefore have been solely local,due to proximity of the measuring equipment to human activity, and the assumption by the models that greenhouse gases are the only important climate influence is incoorect. The warm period since 2001 is difficult to explain, but it is steady, not increasing, so this too cannot be linked to an increasing greenhouse gas burden" [VINCENT GRAY (Reviewer's comment ID #: 88-387)]	
3-470	A	26:49	26:49	Figure 3.4.2 is a disgrace, as it is designed to conceal the very real differencves between the two temperature record from the troposphere, and the surface record. The assumption thatn they are virtually identical is a plain lie. You must show a seperate record for each of the different versions of the MSU record, NOT all plotted on top of one anothet to conceal the truth. The truth is that they show no evidenve of a "Greenhouse effect" and you are trying to cover up this undoubted fact. [VINCENT GRAY (Reviewer's comment ID #: 88-388)]	
3-471	A	27:5	27:11	This paragraph would be better as a table showing which channels have been produced by which providers. VG (or Umd) do not create a T4 product whereas your current text suggests they do. There are also missing efforts by Mitch Goldberg and colleagues and Prabhakara and colleagues. Each have produced different channel estimates and it would be so much easier to show this as a table with data group as columns and channels (2Lt, 2, 3, 4, T*T, T*G) as rows and filled with a linear trend value where it exists. You could then ditch this paragraph. [Peter Thorne (Reviewer's comment ID #: 264-6)]	
3-472	A	27:9	27:9	Remove "and surface". [Qiang Fu (Reviewer's comment ID #: 78-7)]	
3-473	A	27:9	27:10	Wording seems odd here:"75-80% from troposphere and surface, 15% from lower stratosphere and the remaining 5-10% from the surface". Why does the surface appear twice in this? Should the first "and surface" be there at all? [It is not there in the otherwise similarly worded sentence on page 3-122, line 35] Does 15% come from the stratosphere at all latitudes, bearing in mind the substantial changes in tropopause height from equator to pole. [Same question for page 3-122] What happens for Antarctica, where the surface is high and the tropopause low? In NWP and reanalysis we would generally not use low-	

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				sounding microwave channels over elevated terrain because of difficulties in handling surface emissivity. [Adrian Simmons (Reviewer's comment ID #: 242-47)]	
3-474	A	27:13	27:13	Show ALL the Figures in separate diagrams, not all lumped together to conceal the truth [VINCENT GRAY (Reviewer's comment ID #: 88-389)]	
3-475	A	27:13	27:14	Replace from "and" in line 13 to "Figure 3.4.3." in line 14. with "The records, from 1978 to the present, agree with the radiosonde and surface records in showing the influence of natural events such as volcanic erupotions by Mt Chichon ((981) and Pinatubo (1990) and the El Niño events of 1982 and 1998. The 1998 event gave a particularly large peak. This means that the record is reliable, and probably the most reliable. since it is truly global, and has been subjected to thorough scrutiny. The finding, therefore , that there was no overall temperature change between 1978 and 1998 can be taken as proof, together with that of the radiosonde records, that there is no detectable radiative forcing in the lower atmosphere that might have resulted from increase in greenhouse gases, where the effects are the most prominent.. The temperature rise shown in the surface record since 1978 must, therefore have been solely local,due to proximity of the measuring equipment to human activity, and the assumption by the models that greenhouse gases are the only important climate influence is incorrect. The period of cooling since 1999 is difficult to explain, but it is steady, not increasing, so this too cannot possibly be linked to an increasing greenhouse gas burden" Also delete Figure 3.4.3 as it gives a deliberately spurious version of the actual "trends" shown by the MSU and radiosonde records. Linear trends are deliberately msleading as they conceal the fact that for the main part of the record there was no "trend" whatsoever, and it assumes that the large E Niño peak in 1998 was part of a "trend". [VINCENT GRAY (Reviewer's comment ID #: 88-390)]	
3-476	A	27:13		13 radiosonde time series are also shown in Fig. 3.4.2 but not mentioned here. [John Christy (Reviewer's comment ID #: 41-8)]	
3-477	A	27:14	27:14	Replace "These show" by "There was" [VINCENT GRAY (Reviewer's comment ID #: 88-392)]	
3-478	A	27:15	27:15	Change 19792004 to 1979-2004 [Lisa Alexander (Reviewer's comment ID #: 1-3)]	
3-479	A	27:15	27:15	1979-2004. [Chris Folland (Reviewer's comment ID #: 71-21)]	
3-480	A	27:15	27:16	Replace from "of 0.04" on line 15 to "records" on line 16 with "of zero from 1978 to 1998, followed by a large peak attributed to El Niño in 1999, and a cooling with a steady temperature period between 2001 to 2005" [VINCENT GRAY (Reviewer's comment ID #: 88-391)]	

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3-481	A	27:15	27:15	should read 1979-2004, not 19792004. [Blair Trewin (Reviewer's comment ID #: 266-70)]	
3-482	A	27:18	27:18	Insert after "corrections"," But all of them show a zero temperature trend between 1978 and 1998". [VINCENT GRAY (Reviewer's comment ID #: 88-393)]	
3-483	A	27:21	27:34	The difference in global trends between UAH and RSS is 0.08 K/decade. As two papers coming out this year will show, the NOAA-11 period is the period of largest differences and likely relates to both the hot-target calibration adjustment and the diurnal adjustment. Indeed direct comparisons in the tropics between UAH and RSS show no trend difference for 1979-1991 (Christy and Norris, JTech in press, and Christy et al, JGR conditionally accepted – both have been sent to the appropriate Lead Authors.) I would highlight both NOAA-9 AND NOAA-11 as sources of the discrepancy and rephrase to accommodate the idea that RSS could have a significant error [John Christy (Reviewer's comment ID #: 41-9)]	
3-484	A	27:23	27:23	Insert after "effect" "However, before the 1999 El Niño peak all versions show no trend at all between 1978 and 1998" [VINCENT GRAY (Reviewer's comment ID #: 88-394)]	
3-485	A	27:34	27:34	Add at end "As before, these differences did not alter the absence of any trend from 1978 to 1998 for all versions" [VINCENT GRAY (Reviewer's comment ID #: 88-395)]	
3-486	A	27:42		UAH and RSS adjust the diurnal issue by latitude (and UAH adjusts the biases by latitude) and after that is done, the hot-target calibration is well-behaved on a global scale. I would suggest, " … is related to the diurnal cycle correction which is done on a latitude by latitude basis in UAH and RSS." [John Christy (Reviewer's comment ID #: 41-10)]	
3-487	A	27:46	27:47	Although the lower stratosphere has indeed undergone strong cooling since 1979, it might be remarked (see comment #7 for more information) that the top panel of FIGURE 3.4.2 actually shows a slight warming of the lower stratosphere since 1997. Indeed, the cooling since 1979 appears to have occurred in just two spells at the tail ends of the El Chichon and Pinatubo perturbations. [Adrian Simmons (Reviewer's comment ID #: 242-48)]	
3-488	A	27:50	27:50	Remove "-UW (for University of Washington)" to be consistent with Fig. 3.4.1 caption. [Qiang Fu (Reviewer's comment ID #: 78-8)]	
3-489	A	27:55	27:55	Consider to add "[The effective weighting function by combining T2 and T4 for the tropics is near zero throughout the stratosphere (Fu and Johanson 2005).]". [Qiang Fu (Reviewer's comment ID #: 78-9)]	

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3-490	A	27:56	28:2	The idea of simple statistical retrievals (SSRs) is interesting and you have probably spent all of the words you have available to spend. However, as Christy and Norris (2006, in press) show, UAH and VIZ radiosondes show virtually identical interlayer relationships using SSRs while RSS is clearly the outlier. Higher consistency is obtained if the RSS stratospheric trend is made more negative by 0.1 K/decade. However, one cannot say which (or all) of RSS time series are inconsistent with the other RSS products, but the three products (2LT, 2 and 4) together are not consistent in the same way as radiosondes and UAH. [John Christy (Reviewer's comment ID #: 41-11)]	
3-491	A	28:4	28:19	Important paragraph! The current UAH T2LT (5.2 version) is still problematic by noting that T2LT trend for the tropical mid-lower troposphere is smaller than both the surface temperature trend and the tropical tropospheric temperature trends as derived from UAH T2 and T4! [Qiang Fu (Reviewer's comment ID #: 78-10)]	
3-492	A	28:8	28:9	This has been addressed before. Swanson DID NOT show any impact of sea ice variability on ANOMALIES of MSU temperatures. His was a mean annual cycle comparison. As noted before, I compared both UAH and RSS anomalies with 6 radiosonde stations (used by Swanson) and found no problem (trends within ±0.05 K/decade and correlations >0.96 for both satellite vs. sonde comparisons). This sentence could be cut and all would be fine. [John Christy (Reviewer's comment ID #: 41-12)]	
3-493	A	28:11	28:11	Add at end "However, Thorne et al (2005) hhave successfully corrected most of these anomalies" [VINCENT GRAY (Reviewer's comment ID #: 88-384)]	
3-494	A	28:14	28:16	I strongly believe that this statement is not true. The UAH is infact now a more internally consistent channel set than RSS in the tropics. This can be simply verified. But I do not see what value this sentence adds unless it is to imply by innuendo that UAH is somehow a worse estimate. I would advocate removal of the sentence. [Peter Thorne (Reviewer's comment ID #: 264-7)]	
3-495	A	28:14		" … from satellite drift had the wrong sign in the UAH record over the tropics." (The rest of the globe was virtually unchanged when corrected, hence the reason for the small global correction of +0.035 K/decade.) [John Christy (Reviewer's comment ID #: 41-13)]	
3-496	A	28:16	28:19	State the actual trends. [Chris Folland (Reviewer's comment ID #: 71-22)]	
3-497	A	28:16		This statement is incorrect. UAH 2LT tropical trends through April 2006 are +0.07 K/decade and that of T2 is +0.05 K/decade indicating T2LT has a slightly warmer trend	

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				than T2. A problem here is that the SSR from Fu is valid for 30S-30N (where UAH 2LT is even more positive), not 20S-20N. Additionally, sondes and in the Reanalyses show whate appears to be a "bulge" of warming in the upper troposphere captured by the T2 and missed by T2LT (allowing the stratospheric cooling in T2 to be mitigated somewhat). Spencer et al. 2006 (JTech) and Christy and Norris (2006) both show how SSRs can give the wrong answers when applied to regions and time periods outside of their calibration regions and/or time period. [John Christy (Reviewer's comment ID #: 41-14)]	
3-498	A	28:17	28:18	Make clear that this sentence refers to an analysis of RSS data only and therefore should be treated with a degree of caution given the uncertainty inherently evident in the data processing from a comparison for other channels. [Peter Thorne (Reviewer's comment ID #: 264-8)]	
3-499	A	28:18	28:19	This sentence should be earlier in the paragraph where 2LT is discussed not added is an afterthought addendum. [Peter Thorne (Reviewer's comment ID #: 264-9)]	
3-500	A	28:19	28:19	Also add at the end of line 19 "The radiosonde record shows a good agreement with the surface record and the MSU record in its identification of natural events, such as as. Mt Agung (1962), Chichon (1981, and Pinatubo (1990), and the El Niño events of 1982 and 1998. It also shows clearly the fairly abrupt cool period between 1965 and 1978 which also appears on the surface record and has been attributed to an ocean change. It can therefore be regarded as a reliable record for temperture change in the loer and upper atmosphjere. The most inportant finding is that there is no temperature change from 1958 and 2002,. This means that there is no detectable increase in radiative forcing in the regions where it should be evident. It also means that the temperture rise from 1978 in the surface record must have had some other cause, such as proximity of the measuring equipment to human activities" [VINCENT GRAY (Reviewer's comment ID #: 88-385)]	
3-501	A	28:19		RSS trend is 0.07 K/decade warmer than UAH, not 0.10. They aren't too far apart, but their difference time series is indeed significant. [John Christy (Reviewer's comment ID #: 41-15)]	
3-502	A	28:25	28:26	As mentioned above, Christy and Norris 2004, Christy and Spencer 2005 and upcoming Christy et al. 2006 show several cases of spurious warming in radiosondes that would not have been detectable in Sherwood et al. and Randal and Wu. For example, as Christy and Spencer 2005 (Science) note, Sherwood's tropospheric trends for the southern 75% of the globe are impossibly too warm even assuming that models show the correct surface/troposphere relationship. [John Christy (Reviewer's comment ID #: 41-16)]	

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3-503	A	28:26	28:31	This does a good job of deflating the Earthshine "results", although Pallé et al have returned with more of their lunacy in a very recent paper in Eos. In addition to the Wielicki et al paper, I would refer readers to our analysis of the Earthshine inadequacies in the paper Kandel, R. & Viollier, M., 2005. Planetary radiation budgets. Space Science Reviews, 120, 1-26. [Robert Kandel (Reviewer's comment ID #: 123-19)]	
3-504	A	28:30	28:30	Redraw Figure 3.4.3 to show "trends" between 1979 and 1998. This is the only important issue, and it shows that the surface record is not influenced by greenhouse gas increases, but by purely local surface influences such as proximity of the measuring equipment to human activities [VINCENT GRAY (Reviewer's comment ID #: 88-399)]	
3-505	A	28:35	28:35	Add at end "The cooling at the North Pole as well as the South pole is partticularly interesting as it is the opposite of model predictions. However, the entire disgram is spurious because it is unfair to allocate a "trend" to such an irregular sequence as the MSU record, which shows no trend wahtsoever from 1979 to 1998, and is dominated by a single El Niño event in 1999" [VINCENT GRAY (Reviewer's comment ID #: 88-396)]	
3-506	A	28:36	28:36	Insert after "radiosondes", "and surface measurements" [VINCENT GRAY (Reviewer's comment ID #: 88-386)]	
3-507	A	28:37	28:37	It would be informative show the UAH figure as well in Fig 3.4.4 so the differences can be clearly seen. They are important to reduce or resolve. [Chris Folland (Reviewer's comment ID #: 71-23)]	
3-508	A	28:37		FIGURE 3.4.4 is very puzzling. The corresponding figure for T2 for the period 1979-2001 published by Santer et al. (their fig. 11) shows quite strong cooling over the central tropical Pacific (for RSS, UAH and ERA-40), and this is quite consistent with the SST trend there, as noted in Simmons et al.(2004). FIGURE 3.4.4, in contrast, shows weak warming there. Over the same region there is only a relatively weak trend in T4, so it is unlikely that the T4 correction can account for changing the cooling to a warming. So, are we looking only at a difference between a trend for the period 1979-2001 and one for 1979-2005 (perhaps influenced by the strong 97/98 El Nino), or is another explanation needed? [Adrian Simmons (Reviewer's comment ID #: 242-49)]	
3-509	A	28:37		Also on the subject of FIGURE 3.4.4, Santer et al showed large differences between UAH, RSS and ERA-40 over and near Antarctica (as noted rather weakly in the paragraph from lines 10 to 17 of page 3-29 - the text originally contributed was stronger on this, and the comment in the current version about surface emissivity over snow and ice implies that the discrepancy is in the far south, whereas the text just says "SH". In	

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				view of the discrepancies at high southern latitudes, maybe these latitudes should be blanked in FIGURE 3.4.4. Or have the discrepancies showed by Santer et al. since been resolved? [Adrian Simmons (Reviewer's comment ID #: 242-50)]	
3-510	A	28:52	28:56	There must be an error here. Surface trends of ERA-40 (and NCEP-50) are much cooler than HadCRU3v as shown in AR4 Fig. 3.4.3. When the difference time series are examined, ERA-40 and NCEP are significantly different from zero. [John Christy (Reviewer's comment ID #: 41-17)]	
3-511	A	28:55	28:56	UAH has published global "measurement" error bars of ±0.05 K/decade for 2LT and 2 with ±0.10 K/decade for 4. [John Christy (Reviewer's comment ID #: 41-18)]	
3-512	A	29:10	29:17	Aside from the above comment, this paragraph really does need rewriting. It has been edited incorrectly. FIGURE 3.4.4 does not to my eye show net cooling over the SH, so why the remark that "over the SH ERA-40 indicates no net cooling. Santer et al. show in fact that the patterns of tropospheric trends in ERA-40 are close to those of RSS and UAH down to about 45S, but that there are large discrepancies further south, not only between ERA-40 and the MSU estimates, but also between RSS and UAH. At these high southern latitudes ERA-40 does indeed show net warming whereas both MSU datasets show cooling. But the radiosondes also show mid-tropospheric warming over Antactica. Good agreement between ERA-40 and the radiosondes is noted in the final sentence of the paragraph, which should be brought forward (once "SH" is changed to "south of 45S" or "at high southern latitudes") so it appears before discussion of the stratosphere. The statement (presumably contributed by Bromwich) has since been confirmed in the recent Science paper of Turner et al., although they point out that ERA-40 slightly exaggerates the warming seen in the radiosondes. [Adrian Simmons (Reviewer's comment ID #: 242-51)]	
3-513	A	29:15	29:16	What direction are the trends in? [Blair Trewin (Reviewer's comment ID #: 266-24)]	
3-514	A	29:16	29:18	Explicitly mention the size and direction of the trends. [Chris Folland (Reviewer's comment ID #: 71-24)]	
3-515	A	29:18	29:18	There should be a Figure which shows the results of typical reanalyses [VINCENT GRAY (Reviewer's comment ID #: 88-397)]	
3-516	A	29:20	29:22	It might be useful to include a specific definition of the tropopause here. [Blair Trewin (Reviewer's comment ID #: 266-25)]	
3-517	A	29:38	29:56	This whole paragraph needs rewriting after you have revised Figure 3.4.3 to show the significant trends, which are from 1979 and 1998, and the proof that the surface record increase is due to purely local surface effects.	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-398)]	
3-518	A	29:39	30:50	Section 3.4.1.5 Most of this section could be deleted, keeping only the core factual statements. [Melissa Free (Reviewer's comment ID #: 76-6)]	
3-519	A	30:1	30:8	Delete entire paragraph. Trying to pretend that the records are almost similar is grossly dishonest. The differences reside in the long periods of zero temperature increase in the troiposphere, compared with a temperature rise in thew surface record, which must therefore not be attributable to increases in greenhouse gase, but to purely local surface influences such as proximity of the measuring equipment to human activities. [VINCENT GRAY (Reviewer's comment ID #: 88-400)]	
3-520	A	30:1	30:2	The statement made in the sentence that lies in these two lines applies equally to the post-1978 ERA-40 (Santer et al., 2004, again; fig. 9 this time), again pointing to a rather dubious omission of ERA-40 curves from the CCSP figure. [Adrian Simmons (Reviewer's comment ID #: 242-52)]	
3-521	A	30:10	30:10	Repl;ace "often not a very good" by "exceedingly misleading" [VINCENT GRAY (Reviewer's comment ID #: 88-401)]	
3-522	A	30:11	30:11	Replace "are to" with "have to" [VINCENT GRAY (Reviewer's comment ID #: 88-402)]	
3-523	A	30:11	30:11	Insert after "factor in" not only the zero tempertue change from 1958 to 2002 (from radiosondes), and 1978 to 1998 (from satellites), but also" [VINCENT GRAY (Reviewer's comment ID #: 88-403)]	
3-524	A	30:11		suggest replacing ", and" in "…2005a,b), and alternative…" to ";" [Richard Allan (Reviewer's comment ID #: 3-35)]	
3-525	A	30:13	30:13	Delete "confidence limits for" [VINCENT GRAY (Reviewer's comment ID #: 88-404)]	
3-526	A	30:14	30:14	Replace "very large" by "not appropriate'" [VINCENT GRAY (Reviewer's comment ID #: 88-405)]	
3-527	A	30:14	30:14	Insert after "from" " El Niño events, particularly that in 1999, and" [VINCENT GRAY (Reviewer's comment ID #: 88-406)]	
3-528	A	30:15	30:15	Delete "and" [VINCENT GRAY (Reviewer's comment ID #: 88-407)]	
3-529	A	30:15	30:15	Replace "not a very good fit to the data" with "extremely misleading" [VINCENT GRAY (Reviewer's comment ID #: 88-408)]	
3-530	A	30:17	30:17	Replace "not a good" by "a very poor" [VINCENT GRAY (Reviewer's comment ID #: 88-409)]	
3-531	A	30:25	30:25	Add at beginning "the very slight amount"	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-410)]	
3-532	A	30:26	30:27	It is very likely correct to write of ERA-40 "(which has a warm-biased stratospheric trend)", but in the interest of fairness, after the words "radiosonde and NRA datasets" one could equally justify writing "(which have cold-biased stratospheric trends)" in view of the earlier marks about declining day-night sonde differences, and the obvious problems of the NCEP reanalysis as depicted by Santer et al. (2004). The ERA-40 trend is, after all, closer to the MSU and UAH values than are the radiosonde and NRA trends. [Adrian Simmons (Reviewer's comment ID #: 242-53)]	
3-533	A	30:28	30:35	Suggest change to " The weakest tropospheric trends occur for NRA. However, unlike ERA-40 data, the NRA did not allow for changes in greenhouse gas increases over the record, resulting in errors in radiative forcing and in satellite retrievals in the infra-red (Randel et al. 2000); indeed upward trends at high surface mountain stations are stronger than NRA free atmosphere temperatures at nearby locations (Pepin and Seidel, 2005). The records suggest that since 1979 the global and tropical tropospheric trends are similar to those at the surface although RSS, and by inference VG2, indicate greater tropospheric than surface warming. The reverse is indicated by the UAH and the radiosonde record although these data are subject to significant imperfections discussed above." [Richard Allan (Reviewer's comment ID #: 3-37)]	
3-534	A	30:28	30:30	This is irrelevant. NRA is tied to observations, so as observations respond to any forcing, they will impact the reanalysis. [John Christy (Reviewer's comment ID #: 41-19)]	
3-535	A	30:28		Suggest new paragraph [Richard Allan (Reviewer's comment ID #: 3-36)]	
3-536	A	30:30	30:31	We are discussing microwave so this reference to infra-red effects is of very low utility and should be dropped. It is at best an aside in the context of the discussion. [Peter Thorne (Reviewer's comment ID #: 264-12)]	
3-537	A	30:37		RSS has a warmer trend after 1987 due to a shift to warmer temperatures around 1992 as documented in a number of independent comparisons in Christy and Norris 2006 and Christy et al. 2006. Indeed AR4 Fig. 3.4.5 provides even further independent evidence in that the 3-year post-1992 period is drier (cooler) than the 3-year pre-1992 period (thanks to Pinatubo). RSS is the only dataset which shows significantly warmer temperatures in the post-1992 period (comparisons include various sonde datasets, UAH LT, and even SURFACE TEMPERATURES!.) These words need to be recrafted to accommodate information that will be coming out without making direct reference to it rather than assuming there is increasing temperature trends with height. The evidence does not support this statement. [John Christy (Reviewer's comment ID #: 41-20)]	

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3-538	A	30:40		This is a prejudicial statement. As Christy et al. 2006 show, RSS is the only dataset with this characteristic while UAH, RATPAC, HadAT2, ERA-40, NRA, JRA and Haimberger all agree. How could these 7 be thought of as "only". [John Christy (Reviewer's comment ID #: 41-21)]	
3-539	A	30:40		. "…and only the radiosonde records and UAH are at odds for trends." "only" is an odd word choice as the UAH and the radiosondes make up 4 or the 5 records being compared. It would be more proper English to state that "only the RSS record shows a tropospheric amplification in the tropics." When 4 out of 5 indicate one thing, and 1 out of 5 indicates another, it is not proper to suggest that the "only" ones that are at odds are the 4 as opposed to the 1! [Patrick Michaels (Reviewer's comment ID #: 176-4)]	
3-540	A	30:40		UAH given for both sides of the argument. [David Rind (Reviewer's comment ID #: 214-21)]	
3-541	A	30:40		UAH given for both sides of the argument. [Govt. of United States of America (Reviewer's comment ID #: 2023-235)]	
3-542	A	30:40		Regarding "Apparent UAH conflict…" rewrite as follows: "In the tropics, the theoretically expected amplification of temperature perturbations with height is borne out by interannual fluctuations (ENSO) in radiosonde, UAH, RSS and model data (Santer et al. 2005) but it is not borne out in the trends of radiosonde records and UAH data." [Govt. of United States of America (Reviewer's comment ID #: 2023-236)]	
3-543	A	30:41	30:42	This is pure speculation. The sondes in these studies have not been corrected for instances where spurious warming occurs as shown in for example in Christy and Norris 2004, Christy and Spencer (2005) and the other papers to appear soon. [John Christy (Reviewer's comment ID #: 41-22)]	
3-544	A	30:41	30:42	Use the "likely" etc language here. [Chris Folland (Reviewer's comment ID #: 71-25)]	
3-545	A	30:41	30:41	Replace "would probably" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-411)]	
3-546	A	30:41	30:42	This statement is too strong. What does "probably" mean? Is that the same as "likely" (i.e. 66-90% chance of occurring)? Or does it mean more than a 50% chance of occurring? And what is this based upon? Are you referring to statistical differences in temperature trends with height or simply a numerical difference? In Table 2 from Sherwood et al., it is calculated that for the period 1979-1997 the radiational effect-adjusted LKS trend for the 850-300mb layer in the tropics is +0.16 K/yr greater than the uncorrected version. However, as can been seen in Sherwood et al. Figure 3, this radiational bias probably ended in the late 1990s, thus making the trend difference between the adjusted and unadjusted data maximal with data ending in the late 1990s (as reported in Sherwood et	

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				al.). Thus, this +0.16K/yr trend difference during the period 1979-1997 has probably declined for the period 1979-2004 (the period depicted in AR4 Figure 3.4.3 (bottom) making it harder to assess whether the tropospheric trends are “probably” greater than the surface trends. Or, is there a more solid reference for “probably”? If so, it should be included. [Patrick Michaels (Reviewer’s comment ID #: 176-5)]	
3-547	A	30:41	30:42	This is supposition at best and should be dropped. It adds no scientific value to the preceding discussion. Again, Sherwood et al radiation problems may have been accounted for in radiosonde datasets considered here, at least to some extent. [Peter Thorne (Reviewer’s comment ID #: 264-13)]	
3-548	A	30:44	30:44	Replace "Global mean trends" with "Comparison of surface and troposphere temperature records" [VINCENT GRAY (Reviewer’s comment ID #: 88-412)]	
3-549	A	30:46	30:49	Replace from "with weakening" on line 46 to 3.6.4) on line 49 with "increased heating in urban areas over the winter months" [VINCENT GRAY (Reviewer’s comment ID #: 88-413)]	
3-550	A	30:51	30:51	Replace "since 1958" by "1958 to 2002" [VINCENT GRAY (Reviewer’s comment ID #: 88-414)]	
3-551	A	30:51	30:51	Delete "overall" and "tropical" [VINCENT GRAY (Reviewer’s comment ID #: 88-415)]	
3-552	A	30:51	30:51	Replace "warming" by "temperature change" [VINCENT GRAY (Reviewer’s comment ID #: 88-416)]	
3-553	A	30:52	30:52	Replace "has slightly exceeded surface warming" by "was zerol, in contrast to the warming shown by the surface record" [VINCENT GRAY (Reviewer’s comment ID #: 88-417)]	
3-554	A	30:53	30:53	Replace "warming" with "sudden warming, but the radiosonde record then remained constant until 2002, whereas the surface record increased for all that period" [VINCENT GRAY (Reviewer’s comment ID #: 88-418)]	
3-555	A	30:53	30:54	Delete from "such variations" on line 53 to "unsurprising" on line 54". [VINCENT GRAY (Reviewer’s comment ID #: 88-419)]	
3-556	A	30:54	30:57	Replace "After" on line 54 to "trend" on line 57 with "The zero temperature change between 1979 and 1998 in both radiosonde and satellite records contrasts with the steady temperature increase in the surface record over the period, which could not, therefore be attributed to increases in greenhouse gases but to purely local surface influences from the proximity of measuring equipment to human activities.. The 1999 El Niño event appears in all three records, but after that they differ aagain. The Radiosonde record and the MSU	

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				record show a slght temperature jump which is sustained until 2005, but the surface record continued to increase" [VINCENT GRAY (Reviewer's comment ID #: 88-420)]	
3-557	A	31:3	31:3	What would a more appropriate fit be like? [Chris Folland (Reviewer's comment ID #: 71-26)]	
3-558	A	31:10	37:14	This whole section should be transferred to the beginning of Chapter 2.. Water Vapour is the most important greenhouse gas and it needs to be recognised as such, not put in a different Chapter. The claim that water vapour is a "feedback" is purely a device adopted by modelists because they lack adequate historic data, and they make the assumption that it can be related mathematically to other climate effects. There is no evidence, or justification for this assunption, and in any case, it should not inhibit adequate treatment of the effect of water vapour as a greenhouse gas. Clouds are intimately related to water vapour, and thus should also be treated in the same place. They behave in the same way as greenhouse gases, and their treatment as "feedbacks" is even less defensible than that of water vapour as there is not even theoretical arguments belief that the behaviour of clouds is related to other climate influences [VINCENT GRAY (Reviewer's comment ID #: 88-421)]	
3-559	A	31:12	31:38	Most of this background material seems unnecessary. [Melissa Free (Reviewer's comment ID #: 76-7)]	
3-560	A	31:12	31:14	Move from "Water is a key climate variable" on line 12 to "2003a,b)" on line 14 to start a new paragraph on line 18. [VINCENT GRAY (Reviewer's comment ID #: 88-422)]	
3-561	A	31:14	31:14	Delete "also" [VINCENT GRAY (Reviewer's comment ID #: 88-423)]	
3-562	A	31:15	31:15	Replace "about 60%" with "between 60 and 95%" [VINCENT GRAY (Reviewer's comment ID #: 88-424)]	
3-563	A	31:17	31:17	Add at end The assumption that water vapour can be treated as a "feedback" results from the lack of reliable historic data for its mean or varying concentration, but the assumption has no observational basis [VINCENT GRAY (Reviewer's comment ID #: 88-425)]	
3-564	A	31:20	31:20	Delete "sufficient" [VINCENT GRAY (Reviewer's comment ID #: 88-426)]	
3-565	A	31:20	31:20	sufficient' to 'suitable'? [Ian Simmonds (Reviewer's comment ID #: 241-7)]	
3-566	A	31:49	31:49	Delete "strongly" [VINCENT GRAY (Reviewer's comment ID #: 88-427)]	

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3-567	A	32:4	32:12	I would like to see an extra diagram showing surface specific humidity over the ocean against SST, and the notional specific humidity values expected e.g. for a constant 80% RH. This would not only be informative about humidity changes. but also about the consistency of recent SST variations. Possibly create a seasonally resolved global plot of anomalies which would show up ENSO variations. A trend could be fitted through the observed specific humidity on the figure. [Chris Folland (Reviewer's comment ID #: 71-27)]	
3-568	A	32:34	32:34	Replace "order" by " the order of" [VINCENT GRAY (Reviewer's comment ID #: 88-428)]	
3-569	A	32:35	32:35	Insert after "and", "about" [VINCENT GRAY (Reviewer's comment ID #: 88-429)]	
3-570	A	32:37	32:46	I left this comment to last for this chapter, as otherwise it would have come at the end of what looks like a long list of what appears to be special pleading for reanalysis. Nevertheless, I do think the comments in the second half of this paragraph are unduly negative, or at least present "a glass half empty" rather than "a glass half full". In Uppala et al.(2005) we certainly did not attempt to hide problems with the representation of water vapour in ERA-40, as correctly recognised in the paragraph. But we did also note an 83% correlation between the ERA-40 analyses and SSMI retrievals for TCWV over the tropical oceans, and an 88% correlation for 24-hour forecasts. TCWV maxima associated with the 1982/3 and 1997/8 El Ninos in the reanalysis products were shown to be in good agreement with SMMR and SSMI retrievals respectively. We also showed that correlations with SST time series indicate quite reasonable behaviour in capturing interannual variability back to 1973, demonstrating, for example, a pronounced maximum in 1973 itself, which I have just checked was indeed another El Nino year. [Adrian Simmons (Reviewer's comment ID #: 242-79)]	
3-571	A	32:40	32:40	Replace "quite good" with a more explicit assessment. [Chris Folland (Reviewer's comment ID #: 71-28)]	
3-572	A	33:3	33:3	Delete from "and is believed" to "Soden 2000)", The statement is unnecessary and it introduces the ambiguous concept "climate change" [VINCENT GRAY (Reviewer's comment ID #: 88-430)]	
3-573	A	33:3	33:5	Delete from "Changes" in line 3 to :debate" in line 5. The statement tells us nothing. [VINCENT GRAY (Reviewer's comment ID #: 88-431)]	
3-574	A	33:11		As I understand it, the Minschwaner and Dessler (2004) study showed an increase in moiture with temperature but at a smaller rate than expected for constant relative humidity. [Richard Allan (Reviewer's comment ID #: 3-38)]	
3-575	A	33:32	33:32	Insert after "warming" "after 1998"	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-432)]	
3-576	A	33:32	33:41	I am a bit puzzled by this paragraph. Why is the dashed line horizontal? The T2 curve alone should slope upwards at about 0.1K/decade on average because of the tropospheric warming measured by the T2 time series. Does T12 exhibit a temperature dependence that more-or-less mirrors that of T2? [Adrian Simmons (Reviewer's comment ID #: 242-54)]	
3-577	A	33:38	33:41	See comments on Fig. 3.4.6 below [John Christy (Reviewer's comment ID #: 41-23)]	
3-578	A	33:52	33:52	Insert after "which is" "partly" [VINCENT GRAY (Reviewer's comment ID #: 88-433)]	
3-579	A	33:52	33:52	Insert after "temperatures" "after 1998 [VINCENT GRAY (Reviewer's comment ID #: 88-434)]	
3-580	A	34:2	34:12	Stratospheric water vapour is a field with significant data quality caveats that should be given more prominence here. [Govt. of Australia (Reviewer's comment ID #: 2001-213)]	
3-581	A	34:12	34:12	This section should end with something like "Despite the large number of contributors and referees affirming the existence of the increase in stratospheric water vapour over the last 40 years (Kley et al 2000), because we have no complete explanation some workers remain sceptical, despite little contradictory evidence other than a reversal of the trend in recent years. One of the few items of contradictory evidence is given by Roscoe et al (2003), whose lead author was himself a referee of Kley et al (2000)." [Roscoe, H.K., S.R. Colwell, J.D. Shanklin, "Stratospheric temperatures in Antarctic winter: does the 40-year record confirm mid-latitude trends in stratospheric water vapour?", Quart. J. Roy. Met. Soc. 129, 1745-1759 (2003)] [Howard K. Roscoe (Reviewer's comment ID #: 219-15)]	
3-582	A	34:29	34:29	Insert after "period" "until the current steady value since 1998" [VINCENT GRAY (Reviewer's comment ID #: 88-435)]	
3-583	A	34:29	34:29	Replace "trend" by "trend in water vapour" - as written, it seems like trend in methane [Howard K. Roscoe (Reviewer's comment ID #: 219-16)]	
3-584	A	34:29	34:29	Replace "appears to be too large" by "is far too large". There is no doubt about this conflict. [Howard K. Roscoe (Reviewer's comment ID #: 219-17)]	
3-585	A	34:29		Seems odd to be talking about the trend being too large when both before and after this paragraph doubt is indicated concerning the validity of the trend (if the balloon data trend is inconsistent with satellite observations now, what confidence can we have in it for earlier time periods?).	

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				[David Rind (Reviewer's comment ID #: 214-22)]	
3-586	A	34:29		Seems odd to be talking about the trend being too large when both before and after this paragraph doubt is indicated concerning the validity of the trend (if the balloon data trend is inconsistent with satellite observations now, what confidence can we have in it for earlier time periods?). [Govt. of United States of America (Reviewer's comment ID #: 2023-237)]	
3-587	A	34:32	:33	The statement "Aviation emits a very small amount of water vapor directly into the stratosphere" needs to be expanded to put in context the direct injection from aviation with other water vapor sources already in that region. [Govt. of United States of America (Reviewer's comment ID #: 2023-238)]	
3-588	A	34:41	34:41	… importing higher water vapour values into the …' should be '…resulting in higher water vapour values when …' or "… importing more water vapour into the …' [Ian Simmonds (Reviewer's comment ID #: 241-8)]	
3-589	A	34:51	34:51	This sentence should emphasise that Fuglistaler & Haynes go a very long way towards explaining the trend observed since about 1985 [Howard K. Roscoe (Reviewer's comment ID #: 219-18)]	
3-590	A	35:14	35:14	Insert after "surface" It is likely that they change independently from other climate influences, so it is unsurprising that" Change "The" to "the" [VINCENT GRAY (Reviewer's comment ID #: 88-436)]	
3-591	A	35:14		Suggest inserting new 2nd sentence: "They are also integral to the atmospheric hydrological cycle via their integral influence on the balance between radiative and latent heating." [Richard Allan (Reviewer's comment ID #: 3-39)]	
3-592	A	35:20	35:20	What are "correlative data" [Chris Folland (Reviewer's comment ID #: 71-29)]	
3-593	A	35:34		change ";" to "," [Richard Allan (Reviewer's comment ID #: 3-40)]	
3-594	A	35:36	35:39	Suggest reducing to "...and a reduction in DTR (Dai et al., 2006). However, decreasing cloudiness over this period has been reported over China (Kaiser, 1998), Italy (Maugeri et al., 2001) and over Central Europe (Auer et al., 2006)." [Richard Allan (Reviewer's comment ID #: 3-41)]	
3-595	A	35:40		Suggest changing "more mixed" to "less coherent" or "less wide-spread" [Richard Allan (Reviewer's comment ID #: 3-42)]	
3-596	A	35:43	35:43	At the end of this paragraph, add the following statement " Downstream of the Tibet Plateau (Yu et al., 2004) monthly mean anomalous cloudiness and surface temperature vary in tandem. Surface warming leads to destabilization and desaturation in the boundary	

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				layer, suggesting a positive feedback between the continental stratus clouds and surface temperature through changing lower tropospheric relative humidity and stratification. The positive feedback mechanism is more robust during periods of surface cooling than during surface warming (Yu et al., 2004a)". The paper has already been listed in the References. The paper should be "Yu, R.,B. Wang, and T. Zhou, 2004b……", add another paper as "Yu Rucong, Bin Wang, and Tianjun Zhou, 2004a, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270" [Govt. of China (Reviewer's comment ID #: 2006-40)]	
3-597	A	35:47		Why is it that land cloudiness correlates so much better with precip in the SH? [Fons Baede (Reviewer's comment ID #: 9-35)]	
3-598	A	36:7	36:7	Change "supports, their validity" to "supports their validity" [Lisa Alexander (Reviewer's comment ID #: 1-4)]	
3-599	A	36:9	36:9	What does the 'Indo-Pacific Ocean' mean? – is it the Indian and Pacific combined, the ocean in the Indonesian region, or something else? [Blair Trewin (Reviewer's comment ID #: 266-26)]	
3-600	A	36:14	36:15	I believe that the word is "could"and not "cloud" [Jose Marengo (Reviewer's comment ID #: 159-5)]	
3-601	A	36:14	36:18	This sentence needs to be broken. Suggest a full stop after (Curtis and Adler, 2003), then 'Multi-decadal variations are…'. [Blair Trewin (Reviewer's comment ID #: 266-27)]	
3-602	A	36:18	36:19	A bibliographic reference for this sentence is needed here, because nothing is found in Section 3.6.4. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-4)]	
3-603	A	36:18	36:19	A bibliographic reference for this sentence is needed here, because nothing is found in Section 3.6.4. [Govt. of Spain (Reviewer's comment ID #: 2019-64)]	
3-604	A	36:26		The ISCCP data collection began July 1, 1983, not in June, 1983. [Govt. of United States of America (Reviewer's comment ID #: 2023-239)]	
3-605	A	36:35	36:39	The issue of the significance of the ERBS decadal - interdecadal changes in reflected SW and outgoing LW remains delicate, according to information received from recent CERES Science Team meetings. [Govt. of France (Reviewer's comment ID #: 2010-27)]	
3-606	A	36:41	36:56	lack of assessment here - does this problem affect the radiative fluxes or not? First paragraph leads to the impression they don't, second paragraph says it's uncertain. [David Rind (Reviewer's comment ID #: 214-23)]	

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3-607	A	36:41	:56	Lack of assessment here. Does this problem affect the radiative fluxes or not? First paragraph leads to the impression they don't; second paragraph says it's uncertain. [Govt. of United States of America (Reviewer's comment ID #: 2023-240)]	
3-608	A	36:49		At the end of the paragraph, Add "Note that the ISCCP total cloud amount data are a lot more reliable than the layered cloud amounts due to insufficient information discriminating cloud layers, especially for semi-transparent multi-layer clouds (Chang and Li 2005a). Often, overlapped high cirrus over low water clouds are mistakenly identified as single layer mid-level clouds by any satellite algorithms using visible and infrared data only. As a result, high and low clouds tend to be overestimated, whereas middle-level clouds are overestimated. Applying a new retrieval algorithm to the mutli-channel MODIS satellite data, Chang and Li (2005b) developed a global climatology of cloud layers showing some distinct features. A bi-mode cloud vertical structure was revealed with maximal cloud occurrence around 275 hPa and 725 hPa for high and low clouds, and an extremely low occurrence (< 4%) of mid clouds between 500-600 hPa. The global mean amounts of high, low and overlapped clouds were estimated to be 61%, 75%, 28%, respectively. The large fraction of overlapped clouds are likely to be attributed to mid-level clouds by the ISCCP or other similar products due to a lack of information content to differentiate them." Chang, F.-L., and Z, Li, 2005a: A new method for detection of cirrus overlapping water clouds and determination of their optical properties, J. Atmos. Sci., 62, 3993–4009, 2005a. Chang, F.-L., and Z. Li, 2005b: A near-global climatology of single-layer and overlapped clouds and their optical properties retrieved from Terra/MODIS data using a new algorithm, J. Climate, 18, 4752-4771. [Zhanqing Li (Reviewer's comment ID #: 147-9)]	
3-609	A	37:0		Section 3.4.4 Radiation. I suggest including a brief summary of the importance of this section, consistent with previous sections. For example insert a paragraph: "Measuring accurately the radiation balance is fundamental in quantifying the radiative forcing of the system as well as diagnosing the the radiative properties of the atmosphere and surface, crucial for understanding radiative feedback processes. At the top of the atmosphere, satellites provide excellent spatial coverage but poorer temporal sampling. The reverse is true at the surface with only a limited number of high quality point measurements but providing an excellent temporal coverage." [Richard Allan (Reviewer's comment ID #: 3-43)]	
3-610	A	37:0		Section 3.4.4 Radiation. This section fails to mention the many high quality satellite-based scanning radiometers that have been analysed since the TAR apart from a brief mention of the CERES instrument when providing an argument to suggest the shortcomings of the Earth Shine Palle et al. measurements. Since the use of these data was	

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				fundamental to the initial assessment of decadal changes in radiative fluxes in the Wielicki et al. 2002a study, which is central to this section, I advocate discussion of their part in the current assessment. I suggest inserting a new 2nd sentance (below). [Richard Allan (Reviewer's comment ID #: 3-44)]	
3-611	A	37:16	38:44	The issue of the significance of the ERBS decadal - interdecadal changes in reflected SW and outgoing LW remains delicate. [Robert Kandel (Reviewer's comment ID #: 123-18)]	
3-612	A	37:16	40:44	The note about the suggested decadal change in ERB for the zone 20N -20S is timely. However, satellite sampling and algorithm checks remain a healthy part of ongoing research. The first results could be negated - or simply unsupported by independent analyses. Citation of support from the "derived" ERB from ISCCP data by Zhang et al is very shaky. They estimate their ERB uncertainity of plus mius 10 to 15 watts/M*xZ! Their ERB calculations rely upon the ISCCP cloud data - currently under extensive reanalysis. Overall, the section 3.4.4 on Radiation p. 3-37 to 3-40 is acceptable if the comments above are included. In regard to the surface radiation budget (p 3-39 line 2) it should be noted that while a few well-calibrated surface instrument sites have been maintained over the years, their numbers each year range from a few dozen to (today) 100 to 200. [Thomas Vonder Haar (Reviewer's comment ID #: 278-4)]	
3-613	A	37:18		Section 3.4.4.1 This section discusses observations suggesting that the there has been an increase in insolation at the surface in the tropics. How does this observation relate to the changes in the tropical lapse rate? It seems to imply that the surface should be warming faster than the troposphere, which is what has been observed, but which you all have discounted by citing Sherwood et al. (see above comment). Why is this observation not used in support of the observed changes to the tropical lapse rate (i.e. that it has become larger)? [Patrick Michaels (Reviewer's comment ID #: 176-6)]	
3-614	A	37:20	37:20	For consistency, 'Wielicki et al., 2002a, 2002b' should be 'Wielicki et al., 2002a, b' [Ian Simmonds (Reviewer's comment ID #: 241-9)]	
3-615	A	37:20		I suggest inserting (see above argument): "This record is supported by independent scanning instruments on a variety of satellites including from the Clouds and the Earth's Radiant Energy System (CERES) and Scanner for Radiation Budget (ScaRaB) instruments. It appears to be related in part to changes in the nature of tropical cloud (Wielicki et al. 2002a), based on the smaller changes in the clear-sky component of the radiative fluxes (Wong et al. 2000; Allan and Slingo 2002), and appears to be statistically distinct from the spatial signals associated with ENSO (Allan and Slingo 2002; Chen et al. 2002). A recent reanalysis of the ERBS active cavity broadband data corrects for a 20 km	

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				change in satellite altitude between 1985 and 1999 and changes in the SW filter dome (Wong et al., 2006). This generally reduces agreement between the decadal variability from the ERBS record and additional scanner data from CERES and ScaRaB, which are subject to calibration uncertainty at a similar level to the decadal changes (Smith et al. 2006 - JGR 111 D04101, doi: 10.1029/2005JD006307).". [Richard Allan (Reviewer's comment ID #: 3-45)]	
3-616	A	38:0		Comment on Question 1.1: Climate is affected by the continental drift, mountain formation, and sea level (since this affects the ocean currents) as well as the causes you mention. Although these are very long term effects perhaps they should be mentioned. [Wilmer Anderson (Reviewer's comment ID #: 5-58)]	
3-617	A	38:20		add reference to Section 3.4.4.2 after "surface fluxes" [Richard Allan (Reviewer's comment ID #: 3-46)]	
3-618	A	38:26	38:31	This does a good job of deflating the Earthshine "results", although Pallé et al have returned with more on this+H33 in a very recent paper in Eos. In addition to the Wielicki et al paper, readerscould be referred to the analysis of the Earthshine inadequacies in the paper Kandel, R. & Viollier, M., 2005. Planetary radiation budgets. Space Science Reviews, 120, 1-26. [Govt. of France (Reviewer's comment ID #: 2010-28)]	
3-619	A	38:26	38:31	There is a new paper just revised for J. Climate by Norman Loeb et al. that has greatly expanded the evaluation of the earthshine albedo anomaly, including the best two calibrated sensors for stability, CERES (broadband) and SeaWiFS (monthly lunar stability scans) that show consistency to 0.2 W/m^2 for tropical mean ocean interannual variability. The paper further intercompares MISR, MODIS, and ISCCP, and none of the 5 satellite data sets confirm the earthshine changes. The paper also includes an analysis based on interannual variability in CERES data that show it will require 15 years of stable global data and 20 years of tropical mean data to detect a 50% cloud feedback in low cloud where reflected SW flux dominates cloud radiative forcing. This paper has been reviewed, revised, and recently resubmitted to J. Climate. Expected to be accepted in the next month or so. I can provide figures. This paper should put to bed the earthshine issue. [Bruce Wielicki (Reviewer's comment ID #: 287-6)]	
3-620	A	38:35		"times scales" --> "time-scales" [Richard Allan (Reviewer's comment ID #: 3-47)]	
3-621	A	38:40	38:44	Here or above, or cross referencing another chapter, is it possible to be say something about the extra global ocean heat storage that has happened due to the observed increase in greenhouse gases in the last few decades? [Chris Folland (Reviewer's comment ID #: 71-30)]	

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3-622	A	39:0		Box 3.2: I found that this box did not come across so clearly (although maybe I needed a break!). Perhaps a summary of the main points at the end would be beneficial. I have some specific suggestions below: [Richard Allan (Reviewer's comment ID #: 3-49)]	
3-623	A	39:16	39:18	The terrestrial data presented by Wild et al. (2005) show brightening over the land since about 1990, while the data of Pinker et al. (2005) show continued dimming over the land. The data of Pinker et al (2005) show brigthening over the ocean. These two studies are inconsistent over the land. This needs to be pointed out along with the other inconsistencies noted. [Michael Roderick (Reviewer's comment ID #: 218-10)]	
3-624	A	39:16	39:22	I think the discussion on trends in surface solar radiation is very well summarized in this section. The only part which needs revision is the sentence starting on line 19 "Nor are they consistent with continued decline...". This sentence implies inconcistencies between the noted overall tendency of an increase in surface solar radiation (solar brightening) during the 1990s with regional analyses in China and the Swiss Alps. However, recent studies analysing data in China, such as the cited study of Qian et al. 2006, do show a brightening also in China during the 1990s. Quian et al. explicitly mention the consistentcy of their findings with the brightening studies. With respect to the Swiss Alps, a recent (not yet published) reanalysis by Philipona et al, using newly homogenized data and including more years (1981-2002) (compared to 1995-2002 in the cited paper), now also find an increase in surface solar radiaton in the Swiss Alps in the last two decades, in line with the brightening studies. So the studies in China and the Swiss Alps are consistent with the brightening studies, and the sentence should be either omitted or reformulated. The study of Qian et al.2006 also points out that pan evaporation measurements in China show a transition from decrease to increase in the 1990s and closely follow the trends in surface solar radiation, and are therefore also consistent with the recent brightening. So the last sentence in the paragraph should be adjusted, as there is not a general continued decline in pan evaporation anymore. [Martin Wild (Reviewer's comment ID #: 288-1)]	
3-625	A	39:18	39:22	This seems a reasonable summation. However, it conflicts with the assertion in the executive summary that brigthening since about 1990 is occurring. [Michael Roderick (Reviewer's comment ID #: 218-11)]	
3-626	A	39:19		"observed LOW cloud" [Richard Allan (Reviewer's comment ID #: 3-48)]	
3-627	A	39:39	39:55	I was not sure what confusion you were referring to. The summaries in Roderick & Farquhar (2004, 2005) are consistent with the summary on lines 50-55 in this box. Pan evaporation measures potential evaporation and is descreasing (unlike the Thornthwaite-	

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				based estimates used by Dai et al 2004). [Michael Roderick (Reviewer's comment ID #: 218-12)]	
3-628	A	39:39	40:37	Box 3.2 This box needs reworking to ensure better consistency within it and with other 'dimming' (page39 lines 4-21) and evaporation (page 3-20 line 48) related sections. No mention is given to wind, and in particular wind run. This has a substantial impact upon evapotranspiration, equal, at least, to the two factors mentioned. [Govt. of Australia (Reviewer's comment ID #: 2001-214)]	
3-629	A	39:39	40:37	Include in discussion how it relates to data from the only location with trends in soil moisture from long-term observations. In the Ukraine, there was a strong upward trend in summer soil moisture without increases in precipitation (Robock et al., 2005). Furthermore, recent calculations (not yet published) show that solar dimming explains the trends due to changing evaporative demand. ref: Robock, Alan, Mingquan Mu, Konstantin Vinnikov, Iryna V. Trofimova, and Tatyjana I. Adamenko, 2005: Forty five years of observed soil moisture in the Ukraine: No summer desiccation (yet). Geophys. Res. Lett., 32, L03401, doi:10.1029/2004GL021914. -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-9)]	
3-630	A	39:46	39:49	Should the lines "…although the framework…urban areas" be better near the end as a way of summarising what is going on? [Richard Allan (Reviewer's comment ID #: 3-50)]	
3-631	A	40:0		Box 3.2: recent results from Gedney et al. (2006; Nature) suggest that increased CO2 may have reduced evapotranspiration since some plants may reduce the sizes of their pores and this appears detectable in the river-run off records. [Richard Allan (Reviewer's comment ID #: 3-53)]	
3-632	A	40:6		How do these changes in cloud fit in with Section 3.4.3 which deals with cloud changes? [Richard Allan (Reviewer's comment ID #: 3-51)]	
3-633	A	40:11		The compensation between cloud albedo and greenhouse effect takes place over a daily average but cloud generally cools the surface by day due to the albedo effect and heats the surface by night due to the greenhouse effect. [Richard Allan (Reviewer's comment ID #: 3-52)]	
3-634	A	40:27	40:27	As stated already in my FOD comments, please omit the Tyrrell (2003) reference; it is included in citation Snow (2003), and the Irish database is rather small in absolute numbers. Instead, please add the reference Snow (2001), contributing much more material on severe local storms in Europe: Snow, J. T. (Ed.), 2001: Special Issue: Conference on European Tornadoes and Severe Storms. Atmos. Res., 56, 409 pp. [Nikolai Dotzek (Reviewer's comment ID #: 59-3)]	
3-635	A	40:28	40:37	We pointed out in the Wild et al. (2004) study, that the decline of land surface solar radiation might have outweighed the increase in surface downwelling longwave radiation	

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				between 1960 and 1990, leading to a decrease rather than increase in land surface net radiation (surface radiation balance) over this period. Therefore less energy has been available for the turbulent fluxes of sensible and latent heat from 1960 to 1990. If the bowen ratio has not changed in favour for the latent heat flux, this implies that the latent heat flux also has decreased over this period. There is no indication that the bowen ratio should have changed in favour of the latent heat flux, since the new dataset of GPCC suggests rather a decrease of land precipitation over the period 1960-1990. So there is no evidence for an increase in soil moisture in this perio. The decrease in evaporation proposed by Wild et al (2005) for the solar dimming period 1960-1990 is therefore not in contradiction with major findings in AR4 on precipitation (Table 3.4), and there is no clear evidence why the bowen ratio should shift in favour of a higher evaporation and less sensible heat under decreasing tendencies of precipitation and surface radiative heating. I would reformulate the paragraph as follows: "Annother apparent paradox raised by Wild et al (2004) is that if surface radiative heating decreases (due to the decline in surface solar radiation which outweighed the increase in back radiation due to greenhouse gas increases over the period 1960 to 1990), then it should be compensated by an decrease in evaporation from a surface energy balance standpoint, especially given an observed increase in surface air temperature". and skip the folllowing sentences. [Martin Wild (Reviewer's comment ID #: 288-2)]	
3-636	A	40:50	40:51	"especially in the late 1970s with the introduction of satellite observations". Two points here. Firstly, satellite observations (VTPR) were introduced in ERA-40 at the beginning of 1973, and only a bit later in the NRA. Better satellite observations (TOVS, GEO) became available around the end of 1978, and this probably was the single most important change at the time. There were, however, other important changes then - introduction of drifting buoys over the southern ocean and much more aircraft data. Moreover, for the NH oceans, the Atlantic in particular, the availability of radiosonde data from the ocean weather ships compensates for the lack of satellite data in the earlier years of the NRA and ERA-40: inhomogeneity in the quality of reanalyses is much more marked in the SH than in the NH. [Adrian Simmons (Reviewer's comment ID #: 242-55)]	
3-637	A	41:6	41:6	This cross-references to Figure 3.5.1, but that figure doesn't show MSLP. [Blair Trewin (Reviewer's comment ID #: 266-28)]	
3-638	A	41:12	41:12	The citation "Wang et al., 2006" should be replaced by "Wang et al., 2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-11)]	
3-639	A	41:45	41:45	remove 'the' from 'in the magnitude'. [Blair Trewin (Reviewer's comment ID #: 266-71)]	

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3-640	A	41:48	41:55	It could be interesting to cite Gallego et al., 2005. In that paper a new objective method for detecting tropospheric jets was developed and a complete climatology of the SH jet stream was calculated between 1958 and 2002. From that climatology a poleward displacement and an acceleration of the polar front jet is detected, which is in agreement with the results presented in this section and in section 3.5.7 (p46 lines 11 to 14). Complete reference: Gallego D., Ribera P., García-Herrera R., Hernández E. and Gimeno L., 2005: A new look at the Southern Hemisphere jet stream. Climate Dynamics, 24, 607-621. DOI: 10.1007/s00382-005-0006-7. [Pedro Ribera (Reviewer's comment ID #: 213-10)]	
3-641	A	42:2	42:2	Suggest rewording to 'storm track location, and an increased storm intensity'. [Blair Trewin (Reviewer's comment ID #: 266-29)]	
3-642	A	42:5	42:6	Suggest rewording of 'storm track location, increased storm intensity' to 'storm track locations and storm intensities'. [Govt. of Australia (Reviewer's comment ID #: 2001-215)]	
3-643	A	42:9	42:12	Is this really a shift or just multidecadal variability? Do the Wang et al data take full account of the recent downturn in the winter NAO? [Chris Folland (Reviewer's comment ID #: 71-31)]	
3-644	A	42:10	42:10	The citation "Wang et al., 2006" should be replaced by "Wang et al., 2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-12)]	
3-645	A	42:14	42:34	Another example - has activity increased or not? Perhaps an introductory sentence should explain that there is conflicting evidence, before painting the pro and con arguements. [David Rind (Reviewer's comment ID #: 214-24)]	
3-646	A	42:14	:34	Has activity increased or not? Perhaps an introductory sentence should explain that there is conflicting evidence, before painting the pro and con arguments. [Govt. of United States of America (Reviewer's comment ID #: 2023-241)]	
3-647	A	42:47	42:49	Suggest reversing this sentence to make cause-and-effect clearer. [Blair Trewin (Reviewer's comment ID #: 266-30)]	
3-648	A	42:52	42:52	The citation "Wang et al., 2006" should be replaced by "Wang et al., 2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-13)]	
3-649	A	43:5	43:6	"...whereas the blockings of 5–10 day duration exhibit no such relationship…." The wording "no such relation ship" is easily miss understood. It should be replaced by "a geographically dependent relations ship" [Christof Appenzeller (Reviewer's comment ID #: 7-1)]	

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3-650	A	43:5	43:5	remove ‘out’ from ‘pointing out to’. [Blair Trewin (Reviewer’s comment ID #: 266-72)]	
3-651	A	43:16	43:16	Australian Bight' should read 'Great Australian Bight'. [Govt. of Australia (Reviewer’s comment ID #: 2001-216)]	
3-652	A	43:16	43:16	insert ‘Great’ before ‘Australian Bight’. [Blair Trewin (Reviewer’s comment ID #: 266-73)]	
3-653	A	43:22	43:23	"in the late 1970s, apparently related to the introduction of satellite observations at that time". See comment #55. [Adrian Simmons (Reviewer’s comment ID #: 242-56)]	
3-654	A	43:33	43:35	Big disagreement in the magnitude of the wintertime stratospheric jet in the extratropics between CIRA and SPARC climatologies - contrary to this sentence. [David Rind (Reviewer’s comment ID #: 214-25)]	
3-655	A	43:33	:35	Big disagreement in the magnitude of the wintertime stratospheric jet in the extratropics between CIRA and SPARC climatologies - contrary to this sentence. [Govt. of United States of America (Reviewer’s comment ID #: 2023-242)]	
3-656	A	44:2	44:2	... 1980 during summer...' probably including which months are referred (DJF) would help to better understand this sentence. [Pedro Ribera (Reviewer’s comment ID #: 213-11)]	
3-657	A	44:4	44:4	idem for spring [Pedro Ribera (Reviewer’s comment ID #: 213-12)]	
3-658	A	44:7	44:50	Reference Scaife et al (2005) (in your ref list), somewhere in Box 3.3 [Chris Folland (Reviewer’s comment ID #: 71-32)]	
3-659	A	45:7		"Because ICOADS winds [are] assimilated into reanalyses, these too will suffer biases". This is true to a point, but if one is going to bring in reanalysis here, it should be explained that many data other than ICOADS (some of which don't even get a mention in this section) are also assimilated into reanalyses, so that the net bias of reanalysis winds may be quite different to the biases of the ICOADS winds, especially after 1978. Reanalyses use wind information over sea that is implicit in the surface pressure observations, and that comes from ocean-buoy measurements, from satellite-borne microwave imagers and scatterometers, and (via vertical structure functions) from low-level cloud-tracked winds. Moreover, where the height of the ship anemometer is known, the measured wind is applied at that height (in ERA-40 at least), reducing one possible source of bias in ICOADS data. [Adrian Simmons (Reviewer’s comment ID #: 242-57)]	
3-660	A	45:8	45:9	Suggest rewording to ‘does not support the existence of any significant globally averaged trends in marine wind speeds, but reveals….’.	

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				[Blair Trewin (Reviewer's comment ID #: 266-31)]	
3-661	A	45:13	45:13	Suggest rewording to 'By comparison with marine winds, visual VOS…'. [Blair Trewin (Reviewer's comment ID #: 266-32)]	
3-662	A	45:30	45:30	The period given in the text (1958-2002) and the period given in the corresponding Figure caption (1950-2002) do not agree. [Andreas Sterl (Reviewer's comment ID #: 253-1)]	
3-663	A	45:35	45:35	Suggest replace "Global and basin-scale model wave hindcasts of Wang and Swail (2001," with "Analyses of global and basin-scale model wave hindcasts of Wang and Swail (2001," [Xiaolan L. WANG (Reviewer's comment ID #: 282-8)]	
3-664	A	45:36	45:36	Suggest replace "2002) and ..." with "2002, 2006) and ..."; see Comment# 10 Below [Xiaolan L. WANG (Reviewer's comment ID #: 282-9)]	
3-665	A	45:38	45:38	'increasing' (not increased). [Blair Trewin (Reviewer's comment ID #: 266-74)]	
3-666	A	45:39	45:39	'show' (not shows). [Blair Trewin (Reviewer's comment ID #: 266-75)]	
3-667	A	46:3	46:6	State the periods covered by the trend and changes. [Chris Folland (Reviewer's comment ID #: 71-33)]	
3-668	A	46:17		"… in the reanalyses." All the quoted evidence (page 3-42, lines 26 to 34) with regard to the NH storm tracks refers to the NRA. If the same comments apply also to ERA-40, then the summary is correct. If the situation is not known for ERA-40, "in the renalyses" should be changed to "in the NRA". In any case, the uncertainties are probably much larger for the North Pacific than the North Atlantic, as the latter had much better coverage by weather ships. [Adrian Simmons (Reviewer's comment ID #: 242-58)]	
3-669	A	46:19		The sentence that begins on this line seems a bit of a non-sequitur, especially with the appearance of the "however". We jump from SH storms to NH sudden warmings. Has a bridging sentence, perhaps referring to the lack of sudden warmings in the 1990s, been lost? [Adrian Simmons (Reviewer's comment ID #: 242-59)]	
3-670	A	46:25	46:25	'decades are dynamically' (not 'is'). [Blair Trewin (Reviewer's comment ID #: 266-76)]	
3-671	A	46:34	54:42	Sec 3.6 Patterns of Circulation Variability. These are the strong indicators of climate change but this section is far too long in a descriptive sense for an assessment of climate change. It needs to be shortened to give a brief description of each pattern and then focus on the changes that have occurred in each.	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-217)]	
3-672	A	46:34		Section 3.6: This section on patterns of variability could be shortened considerably by emphasizing those features that plausibly are related to climate change. Is the Antarctic Circumpolar Wave relevant for climate change? Not only is its utility as a concept describing a coherent phenomena questioned, but the argument that changes in this phenomenon are relevant to climate change are even more tenuous. The PNA is a less-obvious subject for de-emphasis, but there is little evidence that observed trends or model projections are efficiently described in terms of this pattern of internal variability. With regard to the annular modes, the situation is more confusing. But I personally find the idea that the observed system has moved towards a more positive phase of these "modes" unhelpful, typically amounting to no more than a statement that the stormtracks and the associated circulations and momentum fluxes have moved polewards. As a result, the text is effectively redundant, describing the poleward shift in terms of variables such as surface pressure and then in terms of "modes". In models, it is interesting and important that when the models are perturbed to move the westerlies polewards ("exciting the positive phase"), the internal annular variability is still symmetric about this new state rather than being skewed to one side as one would expect if there were a mode that had some fixed spatial structure. The picture I have is that the annular variability exists because the stormtrack latitude fluctuates a lot, and climate responses look like this because they displace the storm track latitude. A "modal" language does not help particularly. I am not suggesting that this kind of thing be discussed here, but I think, because of issues like these, this discussion could be shortened. Discussing trends in sea level pressure, latitude of storm tracks, etc, is not only easier to understand but also more appropriate, in my view. [Isaac Held (Reviewer's comment ID #: 105-25)]	
3-673	A	46:36	46:	Section 3.6.1. Other teleconetions are being well sumarise recenly, for example the altered precipitation in the Mediterranean and some of the consequences like the posibility of a saline Valve are included in the overview of M. Kemp (H. J. Schellnhuber's map of global "tipping points" in climate change), 2005: Nature, 437, 1238. [Govt. of Spain (Reviewer's comment ID #: 2019-8)]	
3-674	A	46:45	46:47	This sentence can be removed as it provides no additional information on the science relevant for WG1. Such a sentence is more applicable elsewhere in the report, or in WG2. [Govt. of Australia (Reviewer's comment ID #: 2001-218)]	
3-675	A	46:46	46:46	Replace 'heat waves' by 'heat and cold waves' -it is more objective. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-5)]	
3-676	A	46:46	46:46	Replace 'heat waves' by 'heat and cold waves' -it is more objective. [Govt. of Spain (Reviewer's comment ID #: 2019-65)]	

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3-677	A	46:55	46:56	The reference (Palmer 1999) is misplaced. It refers to changes in regime (or pattern) frequencies (it doesn't refer to change "in the nature or numbers of states"). This reference should be placed at line 55 after "pattern". In this place (after "states" line 56) other(s) reference(s), which refer specifically "to change in nature or number of states", should be placed. I can give some examples Molteni and Corti (1998) [Molteni F. and Corti S., 1998: Long term fluctuations in the statistical properties of low-frequency variability: dynamical origin and predictability. Q. J. R. Meteorol. Soc. 124, 495-526], Straus and Molteni (2004) [Straus, D. and Molteni F., 2004: Circulation regimes and SST forcing: Results from large GCM ensembles. J. Climate, 17, 1641-1656], Molteni et al. (2003) [Molteni, F., Corti S., Ferranti L. and Slingo J.M., 2003: Predictability Experiments for the Asian Summer Monsoon: Impact of SST Anomalies on Interannual and Intraseasonal Variability. J. Climate., 16 , 4001-4021]. [Susanna Corti (Reviewer's comment ID #: 47-1)]	
3-678	A	47:5	47:7	The sentence "For instance, … the positive NAO index then" doesn't read well. Please reword it. Suggest something like "For instance, … is not as great as it is indicated by the positive NAO index for the same period." [Xiaolan L. WANG (Reviewer's comment ID #: 282-17)]	
3-679	A	47:17	47:18	This sentence needs qualification. Does it mean all teleconnections are strongest in the NH winter, or that NH teleconnections are strongest in the NH winter and SH ones in the SH summer? I suspect the latter, but where does that leave equatorial teleconnections? [Blair Trewin (Reviewer's comment ID #: 266-33)]	
3-680	A	47:21	47:21	References should be cited in chronological order [Ian Simmonds (Reviewer's comment ID #: 241-10)]	
3-681	A	47:26	48:10	Box 3.4 could be replaced by a shorter description of the major circulation indices in the text or the reader could be referred to the glossary. [Govt. of Australia (Reviewer's comment ID #: 2001-219)]	
3-682	A	47:26	48:9	Box 3.4: It could be useful to add that circulation indexes mentioned here are MSLP anomaly difference usually averaged over a season or a month. [Govt. of France (Reviewer's comment ID #: 2010-29)]	
3-683	A	47:47	47:48	Remove: , but this series is less easily updatable in real time. [Govt. of Spain (Reviewer's comment ID #: 2019-24)]	
3-684	A	47:47	47:48	Insert after 1865: . NAO indices defined using Lisbon and Gibraltar are adequated for winter season. NAO index based on Azores data must be used for a season different to winter, or more generally for seasonal studies (Pozo-Vázquez et al., 2000)". [Govt. of Spain (Reviewer's comment ID #: 2019-25)]	
3-685	A	48:9	48:9	should be (IPO; Power et al, 1999b) for consistency with style elsewhere. [Blair Trewin (Reviewer's comment ID #: 266-77)]	

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3-686	A	48:12		Enso is clearly described here for its impact on atmospheric teleconnections; in chapter 5, only PDO is cited for the Pacific ocean. There seems to be a gap between the ocean and the atmosphere in observations. Note that in other chapters (8, 9…), the evolution of ENSO is largely questioned. What is missing is what atmospheric and oceanic observations tell us about the evolution of ENSO in the late decades. [Pascale DELECLUSE (Reviewer's comment ID #: 58-39)]	
3-687	A	48:14	48:35	A dynamical mechanism describing the connection between ENSO and PNA; and ENSO and Southern Annual Mode and Antarctic Circumpolar Wave are provided in Ribera and Mann, 2002 and Ribera and Mann, 2003. This references could be interesting to illustrate the tropical/extra-tropical interactions (complete references: Ribera P. and Mann M.E., 2002: Interannual variability in the NCEP Reanalysis 1948–1999. Geophys. Res. Lett., 29 (10), 1494, doi :10.1029/2001GL013905. and 16. Ribera, P. and Mann M. E. 2003: ENSO related variability in the Southern Hemisphere, 1948–2000, Geophysical Research Letters, 30 (1), 1006, doi:10.1029/2002GL015818.) [Pedro Ribera (Reviewer's comment ID #: 213-13)]	
3-688	A	48:21	48:26	These lines describe the normal state in the equatorial Pacific. How ENSO modifies this state is not described. [Pascale DELECLUSE (Reviewer's comment ID #: 58-37)]	
3-689	A	48:31	48:31	In Australia at least (and probably in other regions) 'winter and spring' would be more appropriate than 'winter'. [Govt. of Australia (Reviewer's comment ID #: 2001-220)]	
3-690	A	48:35	48:35	Add after (2002b).: In the North Atlantic area, during ENSO cold events a statistically significant anomaly SLP pattern resembling the positive phase of the NAO is found. The temperature shows statistically significant negative anomalies during cold events over the Iberian Peninsula and positive anomalies over the British Isles and southern Scandinavia, consistent with the SLP anomalies (Pozo-Vázquez et al., 2001, 2005). [Govt. of Spain (Reviewer's comment ID #: 2019-26)]	
3-691	A	48:39	48:42	The terminology used for ENSO is inconsistent and ambiguous in this section. In particular, it is unclear whether 'strong ENSO events' refers only to strong El Nino events, or to strong departures from normal in either direction. Perhaps start the sentence 'Large-amplitude ENSO events, both warm and cool, occurred....' [Govt. of Australia (Reviewer's comment ID #: 2001-221)]	
3-692	A	48:44	48:44	the statement "shift to … above normal SST… i.e. more El Ninos" is not straghtforward and should be written with care, because of the difficulty to separate ENSO from a non stationary neam state. [Pascale DELECLUSE (Reviewer's comment ID #: 58-38)]	
3-693	A	48:44		Replace "more" by "longer". While the shift to higher SSTs in the eastern and central	

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				Pacific has resulted in an average state that is more El Nino like, this does not imply a higher ENSO frequency. Actually the list of NCEP/CPC of El Nino events (Google "Cold Warm Episodes by Season" to find the webpage) gives the same number of El Nino's for the period 1950-1975 as for the period 1978-2003; other methods of defining El Nino's may give slightly different numbers, but I doubt that they will be significantly different. Note: I have a similar comment to Chapter 5, page 36, line 16-17. [Gerrit Burgers (Reviewer's comment ID #: 34-3)]	
3-694	A	48:44		It appears that the global warming trend over the past century has interacted with the SST signal of EN, giving a perception of stronger and more freqent EN events, which in the equatorial Pacific is the sum of interannual warming due to EN with the global long-term warming trend. The long-term trend in equatorial Pacific SST has contributed to an apparent 30-50% increase in the magnitude of recent El Nino events (Mendelssohn et al. 2005). Full citation - Mendelssoh, R., S.J. Bograd, F.B. Schwing, and D.M. Palacios, 2005. Teaching old indices new tricks: a state space analysis of El Nino related climate indices. Geophys. Res. Lett. 32: L07709, doi:10.1029/2005GL022350. [Franklin SCHWING (Reviewer's comment ID #: 230-5)]	
3-695	A	48:44		It appears that the global warming trend over the past century has interacted with the SST signal of EN, giving a perception of stronger and more frequent EN events, which in the equatorial Pacific is the sum of interannual warming due to EN with the global long-term warming trend. Add "The long-term trend in equatorial Pacific SST has contributed to an apparent 30-50% increase in the magnitude of recent El Niño events (Mendelssohn et al. 2005)". [Govt. of United States of America (Reviewer's comment ID #: 2023-243)]	
3-696	A	48:44		Full citation- Mendelssohn, R., S.J. Bograd, F.B. Schwing, and D.M. Palacios. 2005. Teaching old indices new tricks: a state-space analysis of El Niño related climate indices. Geophys. Res. Lett. 32: L07709, doi:10.1029/2005GL022350. [Govt. of United States of America (Reviewer's comment ID #: 2023-244)]	
3-697	A	48:46	49:3	The statement on Pg 3-48, lines 51-53: " …it is likely that global climate change will interfer and alter El Nino, just as El Nino changes global mean temperature." does not seem justified, and should be deleted, in light of the discussion just above indicating that ENSO involves heat fluxes of the order of 50 W/m sq. These heat fluxes are an order of magnitude larger than the projected effects of human activities over the next century. It is far from obvious why the relatively small change in heat flux that is projected to result from human activites should impact on any part of the ENSO cycle. [Jeff Kueter (Reviewer's comment ID #: 137-51)]	
3-698	A	48:50	49:3	Delete "... it is likely ... temperatures." on Pg, 3-48, lines 51-53. In light of the statement on Pg 3-49, lines 2-3, that determining "…whether observed changes in ENSO are	

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				physically llinked to global climate change is a research question of great importance." the statement on Pg 3-48, lines 51-53: "… it is likely that global climate change will interfere and alter El Nino just as El Nino changes the global mean temperature." is not justified. Likely is defined as a 66-90% probability of being correct, yet the authors are willing to prejudge the outcome of what they define as a research question of great importance. The text (Pg 3-48, line 51) also states that ENSO is involves heat fluxes of the order of 50 W/m sq. Doubling CO2 concentration involved changing heat flux by only 4.4 W/m sq. The water vapor feedback is estimated to increase this effect by 40-50%. However, these effects are an order of magnitude lower than the effect of ENSO, leaving open the question of whether projected climate change would, in fact, affect the El Nino phase of ENSO. [Lenny Bernstein (Reviewer's comment ID #: 20-54)]	
3-699	A	48:50	49:3	Delete "… it is likely … temperatures." On Pg, 3-48, lines 51-53. In light of the statement on Pg 3-49, lines 2-3, that determining "…whether observed changes in ENSO are physically linked to global climate change is a research question of great importance." The statement on Pg 3-48, lines 51-53: "… it is likely that global climate change will interfere and alter El Nino just as El Nino changes the global mean temperature." Is not justified. Likely is defined as a 66-90% probability of being correct, yet the authors are willing to prejudge the outcome of what they define as a research question of great importance. The text (Pg 3-48, line 51) also states that ENSO is involves heat fluxes of the order of 50 W/m sq. Doubling CO2 concentration involved changing heat flux by only 4.4 W/m sq. The water vapor feedback is estimated to increase this effect by 40-50%. However, these effects are an order of magnitude lower than the effect of ENSO, leaving open the question of whether projected climate change would, in fact, affect the El Nino phase of ENSO. [Govt. of United States of America (Reviewer's comment ID #: 2023-245)]	
3-700	A	48:50	49:3	The statement on Pg 3-48, lines 51-53: " …it is likely that global climate change will interfer and alter El Nino, just as El Nino changes global mean temperature." Does not seem justified, and should be deleted, in light of the discussion just above indicating that ENSO involves heat fluxes of the order of 50 W/m sq. These heat fluxes are an order of magnitude larger than the projected effects of human activities over the next century. It is far from obvious why the relatively small change in heat flux that is projected to result from human activites should impact on any part of the ENSO cycle. [Govt. of United States of America (Reviewer's comment ID #: 2023-246)]	
3-701	A	48:53	48:54	Suggest rewording from 'and 1998 was the warmest year for the global mean' to 'and the global mean temperature in 1998 was the highest on record'. [Govt. of Australia (Reviewer's comment ID #: 2001-222)]	

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3-702	A	48:53	48:54	1998 is quoted here as the warmest year for the global mean, without qualification. This is at odds with page 3-3, lines 15 to 19, which point out that NCDC and GISS have 2005 warmer than 1998, in contrast to the CRU/UKMO estimate. [Adrian Simmons (Reviewer's comment ID #: 242-60)]	
3-703	A	48:57	48:57	This should refer to 2002-2003 (not 2001-2002). [Blair Trewin (Reviewer's comment ID #: 266-34)]	
3-704	A	49:5	49:14	Also show the PSA pattern in Fig 3.6.1, to reduce Northern Hemisphere bias. [Chris Folland (Reviewer's comment ID #: 71-34)]	
3-705	A	49:28		The title of this para should be "Pacific Decadal Variability", rather than "Decadal Pacific Variability". This is the term used e.g. in lines 34-35 and defined in the Glossary. [Fons Baede (Reviewer's comment ID #: 9-36)]	
3-706	A	50:6	50:6	Figure 3.6.4 is not divided in a) and b) but only in Top and Lower, please see page 154 (Figure 3.6.4) [ILEANA MARES (Reviewer's comment ID #: 161-3)]	
3-707	A	50:11	50:11	Here is mentioned Figure 3.6.4b, but Figure 3.6.4 is presented only with Top and Lower [ILEANA MARES (Reviewer's comment ID #: 161-4)]	
3-708	A	50:15	50:23	Do these articles demonstrate attribution of the decadal climate change to changes in tropical ENSO evolution, or merely show they coincide? It is equally plausible that mid - and high -latitude changes on decadal scales force the changes in ENSO teleconnections, or they are simultaneously driven by the same variability in forcing. This is quite different from originating in the tropics. [Franklin SCHWING (Reviewer's comment ID #: 230-6)]	
3-709	A	50:15	:23	Do these articles demonstrate attribution of the decadal climate change to changes in tropical ENSO evolution, or merely show they coincide? It is equally plausible that mid- and high-latitude changes on decadal scales force the changes in ENSO teleconnections, or they are simultaneously driven by the same variability in forcing. This is quite different from originating in the tropics. [Govt. of United States of America (Reviewer's comment ID #: 2023-247)]	
3-710	A	50:31	50:31	Is the NAO a 'teleconnection pattern'? [Ian Simmonds (Reviewer's comment ID #: 241-11)]	
3-711	A	50:52	50:53	This reference to positive and negative intervals of the NAO is very vague – how long are the 'intervals'? [Govt. of Australia (Reviewer's comment ID #: 2001-223)]	
3-712	A	51:3	51:3	please add the following ref after Rodwell, 2003: Xoplaki, E., González-Rouco, J.F., Luterbacher, J., and H. Wanner, 2003: Mediterranean summer air temperature variability and its connection to the large-scale atmospheric circulation and SSTs. Clim. Dynam., 20,	

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				723-739. [Jürg Luterbacher (Reviewer's comment ID #: 151-3)]	
3-713	A	51:5	51:13	There is also evidence of a possible anthropogenic impact (Shindell, D. T., G.Schmidt, R.L. Miller and D. Rind, 2001: Northern Hemispheric climate response to greenhouse gas, ozone, solar and volcanic forcing. J. Geophys. Res.; Shindell, D., 2003, Whither Arctic Climate?, Science, 299: 215-216. [Michael Mann (Reviewer's comment ID #: 156-47)]	
3-714	A	51:10		"there may be [monthly-scale] predictability from stratospheric influences." Why is monthly predictability discussed here for the NAO? Is this really the business of the IPCC? Monthly predictability is not discussed (eg of surface temperature or surface winds) in sections of this Chapter dealing with other phenomena or variables. [Adrian Simmons (Reviewer's comment ID #: 242-61)]	
3-715	A	51:11	51:13	Interannual predictability of the NAO also comes from SSTs in the N. Atlantic (Rodwell and Folland, 2002),now used in operational long range forecasting in UK. A similar pattern of N. Atlantic SSTs suggest some potential interdecadal NAO predictability (Rodwell et al, 1999) with as correctly stated, further NAO predictability from the the tropics. Rodwell, M.R. and C.K. Folland, 2002: Atlantic air-sea interaction and seasonal predictability. Q. J. Roy. Met. Soc., 128, 1413-1443; Rodwell, M., Rowell, D.P. and C.K. Folland, 1999: Oceanic forcing of the wintertime North Atlantic Oscillation and European climate. Nature, 398, 320-323. [Chris Folland (Reviewer's comment ID #: 71-35)]	
3-716	A	51:16	51:16	Replace the phrase "and on storminess and precipitation over Europe and North Africa" with "and on storminess and precipitation over North America, Europe and North Africa", because the NAO also exerts a dominant influence on storminess over Canada, according to "Wang et al., 2006b" (see Comment #2 below). [Xiaolan L. WANG (Reviewer's comment ID #: 282-1)]	
3-717	A	51:20	51:20	Insert the following right after the phrase "...over the northwest Atlantic": ", with decreased cyclone activity and increased number of extreme cold days in eastern Canada (as well as increased cyclone activity and increased number of mild winter days in western Canada; Wang et al., 2006b; Shabbar and Bonsal, 2004)" [Xiaolan L. WANG (Reviewer's comment ID #: 282-2)]	
3-718	A	51:23	51:27	There is an error in the 'Norway' gridbox - should read 55-65 (not 60) N. This box might also be better described as 'western Norway' as it excludes much of eastern and northern Norway. (It also includes large parts of Denmark). The 'Spanish' gridbox also includes Portugal and parts of coastal North Africa and might be better described as 'Spain and Portugal' or 'Iberian Peninsula'. (In both cases the current terminology is taken from the	

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				original paper). The citation also incorrectly refers to Part I, not Part II, of the Thompson et al. paper. The correct reference is: Thompson, D.W.J, Wallace, J.M. and Hegerl, G.C. 2000. Annular modes in the extratropical circulation. Part II: Trends. J.Climate 13, 1018-1036. [Blair Trewin (Reviewer's comment ID #: 266-9)]	
3-719	A	51:24	51:24	'out of 3.0' – prefer 'out of the observed 3.0'. [Blair Trewin (Reviewer's comment ID #: 266-35)]	
3-720	A	51:27	51:27	Insert after -0ºW).: The influence of the NAO in the temperature variability in southern Europe is more complex than over central and northern Europe, being extremely sensitive to the location of the SLP anomaly centers (Castro-Díez et al., 2002). [Govt. of Spain (Reviewer's comment ID #: 2019-27)]	
3-721	A	51:28	51:28	At the end of this paragraphy, add the following statements "The NAO also play active roles in modulating the recent cooling downstream of the Tibetan Plateau and the drought south to the Yangtze River (Yu and Zhou, 2004; Li et al., 2006; Xin et al., 2006)". For the reference, see: (1) Yu Rucong, Tianjun Zhou, 2004, Impacts of winter-NAO on March cooling trends over subtropical Eurasia continent in the recent half century, Geophysical Research Letters, 31, L12204, doi:10.1029/2004GL019814. (2) Li Jian, Rucong Yu, Tianjun Zhou, et al. 2005, Why is there an early Spring cooling shift downstream of the Tibetan Plateau, Journal of Climate, 18 (22), 4660–4668. (3) Xin Xiaoge, Rucong Yu, Tianjun Zhou, and Bin Wang, 2006, Drought late spring of South China in recent decades, Journal of Climate, in press. [Govt. of China (Reviewer's comment ID #: 2006-37)]	
3-722	A	51:38	51:38	Insert the following right after "2000).": The correlation between the NAO index and cyclone activity is highly negative in eastern Canada and positive in westhern Canada, with the NAO accounting for over 40% of the total interannual variance of winter (JFM) and fall (OND) cyclone activity over the Canadian east coast (and about 31% for winter cyclone activity over the Canadian Arctic; Wang et al., 2006b)." [Xiaolan L. WANG (Reviewer's comment ID #: 282-3)]	
3-723	A	51:40	51:40	Add "Wang and Swail, 2001" right after the citation "Carter, 1999", because this study also shows the relationship between the NAO and northeast Atlantic wave heights changes (see top of page 2212 in Wang and Swail, 2001). [Xiaolan L. WANG (Reviewer's comment ID #: 282-6)]	
3-724	A	51:46	51:46	apart from Dickson et al. 2000 it should also Xoplaki et al. 2004 be cited, as this is the most extensive and recent publication showing the clear impact of AO/NAO on precipitation in the larger Mediterranean area using more than 100 years of data. Xoplaki, E., Gonzalez-Rouco, J. F., Luterbacher, J., and H. Wanner, 2004: Wet season Mediterranean precipitation variability: influence of large-scale dynamics and trends,	

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				Climate Dynamics, 23, 63-78 [Jürg Luterbacher (Reviewer's comment ID #: 151-4)]	
3-725	A	51:48	51:48	Insert after 2000).: The large inter-annual variability in the flows of the main Iberian rivers is largely modulated by the NAO phenomenon (Trigo et al., 2004). [Govt. of Spain (Reviewer's comment ID #: 2019-28)]	
3-726	A	51:50	51:50	Insert after see WGII report).: A significant influence of the NAO on the winter solar radiation spatial and temporal variability in the European North Atlantic region has been found. Positive NAO index-solar radiation correlations are found for southern Europe and negative for Northern Europe. A stronger influence is found during the NAO negative phase; particularly, the northern British Isles, Norway and the Iberian Peninsula present a significant non- linear response, with higher anomalies (10% to 20%) during this negative phase (Pozo-Vázquez et al., 2004). [Govt. of Spain (Reviewer's comment ID #: 2019-29)]	
3-727	A	52:18		What does this say about the O3 depletion contribution to the SAM trend? [David Rind (Reviewer's comment ID #: 214-26)]	
3-728	A	52:27	3:27	You claim that Turner et al. (2005) found '... a cooling over much of the rest of the continent'. But that paper was only concerned with station data and there are only two stations with long records in the interior of the Antarctic. In that paper we were careful to point out that few of the annual temperature changes around East Antarctic were statistically significant. Only South Pole has a statistically significant cooling in the annual data. [John Turner (Reviewer's comment ID #: 272-2)]	
3-729	A	52:27	52:27	A careful reading of this sentence shows it to say that the SAM contributes a cooling over the rest of the continent. However, a hasty reader would imagine that Turner et al (2005) also showed a cooling trend over the rest of the continent, which they specifically refute (there is no trend over the rest of the continent, warming or cooling). The sentence should be split. [Howard K. Roscoe (Reviewer's comment ID #: 219-19)]	
3-730	A	52:32	52:32	"...autumn and summer..." including months probably helps (MAMJJA); or modify the text into: "...southern hemisphere autumn and summer..." [Pedro Ribera (Reviewer's comment ID #: 213-14)]	
3-731	A	52:44	52:45	Has summer sea ice extent actually decreased? Annual Antarctic mean sea ice extent has not declined in the last 25 years as discussed in CH4. [Chris Folland (Reviewer's comment ID #: 71-36)]	
3-732	A	52:45	52:45	At the end of this paragraphy, add the following statements "The interannual variation of the SAM is also significantly forced by the tropical oceans (Zhou and Yu, 2004)." For the reference, see: 14. Zhou Tianjun, Yu Rucong, 2004, Sea-surface temperature induced	

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				variability of the Southern Annular Mode in an atmospheric general circulation model, Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Govt. of China (Reviewer's comment ID #: 2006-38)]	
3-733	A	52:49	52:49	To make this statement should not we define what is meant by 'modes' here? [Ian Simmonds (Reviewer's comment ID #: 241-12)]	
3-734	A	52:49		What is meant by "sometimes mislabelled as modes"? My confusion as to this remark is heightened by the fact that in a following subsection (3.6.6.3 on the very next page) the Indian Ocean Dipole is persistently referred to as the IOZM, where the M stands for Mode. The Oxford English Dictionary offers the following possible meaning for the word "mode": "A way or manner in which something is done or takes place". It also offers "Any of the distinct kinds or patterns of vibration that an oscillatory system can sustain". In either sense, mode is a perfectly reasonable word to describe recurrent patterns of variability. [Adrian Simmons (Reviewer's comment ID #: 242-62)]	
3-735	A	52:50	52:51	I disagree with the comment that other teleconnection patterns are not relevant to understanding regional climate change or that they are not robust. It is in fact in the regional scale (versus the Hemispheric point of view which would be most adequate for the NAM, for instance) that some teleconnection patterns (East Atlantic, EA, to name just one) is even more relevant that the NAM to explain winter temperature variability over southwestern Europe (Sáenz et al., 2001). EA is not as important as the NAO on a Hemispheric scale, but is more important at a regional scale when dealing with temperature variability. Reference: J. Sáenz, Rodríguez-Puebla, C., Fernández, J., Zubillaga, J., 2001, Interpretation of interannual winter temperature variations over southwestern Europe, Journal of Geophysical Research 106D18:20641-20651. If we consider ozone variations, several patterns appear which must be accounted for in order to explain the full features of the ozone variability over the Euro-ATlantic sector (Orsolini and Doblas-Reyes, 2003). Full reference: Y. J. Orsolini and Doblas-Reyes, F. J., 2003, Ozone signatures of climate patterns over the Euro-Atlantic sector in the spring, QJRMS 129:3251-3263. Finally, when considering the "robustness" of the patterns, some of them have been identified by several different statistical techniques by different analysts (Wallace and Gutzler, 1981; Barnston and Livezey, 1987; Kimoto and Ghil, 1993), to name a few. This means, in my opinion, that they are "robust", in the sense that they appear very frequently in observational data, even though we are not still able to dinamically explain all of them, which is a problem of our knowledge, not about their robustness. J. M. Wallace and D. S. Gutzler. 1981: Teleconnections in the Geopotential Height Field during the Northern Hemisphere Winter. Monthly Weather Review: Vol. 109, No. 4, pp. 784–812. A. G. Barnston and R. E. Livezey. 1987: Classification,	

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				Seasonality and Persistence of Low-Frequency Atmospheric Circulation Patterns. Monthly Weather Review: Vol. 115, No. 6, pp. 1083–1126. M. Kimoto and M. Ghil. 1993: Multiple Flow Regimes in the Northern Hemisphere Winter. Part I: Methodology and Hemispheric Regimes. Journal of the Atmospheric Sciences: Vol. 50, No. 16, pp. 2625–2644. [Govt. of Spain (Reviewer's comment ID #: 2019-147)]	
3-736	A	52:55	53:21	This discussion of the AMO is excellent but lacks a final paragraph to put it into perspective vis-à-vis greenhouse warming: [Govt. of United States of America (Reviewer's comment ID #: 2023-248)]	
3-737	A	52:55	53:21	"The multidecadal oscillations seen in the North Atlantic SST (Figure 3.6.8) mirror very closely the similar variations seen in the average Northern Hemisphere temperatures. This, plus the influence of the AMO on North Pacific temperatures as well as in the North Atlantic (Schlesinger and Ramankutty, 1994; Enfield et al., 2001) suggests strongly that the AMO is a natural influence on global temperatures and that it has alternately obscured and exaggerated generational trends in the warming due to greenhouse gases. Recognition of this is essential as we move out of the current warm phase of the AMO because the tendency of Northern Hemisphere temperatures over the coming decades may once again appear less severe than predicted by models." [Govt. of United States of America (Reviewer's comment ID #: 2023-249)]	
3-738	A	52:55	53:21	In view of the apparent dominance of the AMO in global temperatures, one cannot help but wonder why the AMO has been relegated to the status of "Other Indices" (section 3.6.6) instead of having a section of its own. [Govt. of United States of America (Reviewer's comment ID #: 2023-250)]	
3-739	A	53:1	53:1	Should also cite here: Mann, M.E., Park, J., Global scale modes of surface temperature variability on interannual to century time scales, Journal of Geophysical Research, 99, 25819-25833, 1994. This study was contemporaneous with Schlesinger and Ramankutty, 1994 and comes to the same conclusion. [Michael Mann (Reviewer's comment ID #: 156-48)]	
3-740	A	53:6	53:6	Should also cite here: Mann, M.E., Park, J., Bradley, R.S., Global Interdecadal and Century-Scale Climate Oscillations During the Past Five Centuries, Nature, 378, 266-270, 1995. This study preceded these others, and demonstrated evidence for a spatially-coherent multidecadal signal centered in the North Atlantic region. [Michael Mann (Reviewer's comment ID #: 156-49)]	
3-741	A	53:13	53:14	AMO? Unlikely. More recent work (e.g. Emanuel, 2005a cited in the chapter) finds compelling evidence that the long-term increases in Hurricane destructive potential are	

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				closely related to SST increases which are likely more related to anthropogenic warming than to any natural oscillation. The phenomenon of the "Atlantic Multidecadal Oscillation" has been widely taken out of context and mis-applied to phenomena for which any explanatory role is dubious. Defined as in Mann and Park (1994) and Schlesinger and Ramankutty(1994) which used spatiotemporal signal separate techniques or models, respectively, to separate a possible oscillatory signal from trend, the phenomenon is observed to have little amplitude over the tropical North Atlantic (and therefore is unlikely to have any role in tropical cyclone frequency or intensity). However, a false apparent 'oscillation' is easily 'detected' in studies which define the AMO simply as the residual after linear detrending, as is the case for studies (e.g. Goldenberg et al, 2001) attributing tropical North Atlantic SST changes to the AMO. It has been shown (Mann and Emanuel, Eos, in press) that in such cases, the apparent "AMO" signal is likely an artifact of the linear detrending, since the forced changes in SST are not linear in time. There is a very strong sulphate aerosol cooling impact over the main development region (6-18N, 20-60W) during the crucial Aug-Oct season, estimated as -1.1 degrees C in one recent study [Hansen, J. et al (2005), Efficacy of climate forcings, J. Geophys. Res.,110, D18104,doi:10.1029/2005JD005776.] The competition between long-term GHG forcing, and this regionally and seasonally very strong negative forcing late in the 20th century, leads to a false apparent 'oscillation'. Other submitted work by Trenberth and by Santer et al comes to a very similar conclusion (i.e., that there is no evidence for an "AMO" influence on tropical Atlantic SSTs or tropical cyclone activity. [Michael Mann (Reviewer's comment ID #: 156-51)]	
3-742	A	53:17	53:19	How can a "multidecadal" pattern possibly be meaningfully determined from a few decades of data? How does one distinguish a multidecadal variations from a century-scale trend? The claims that these phenomena can be related to the AMO seems implausible, and it would seem imprudent to draw such specific conclusions based on one study. [Michael Mann (Reviewer's comment ID #: 156-50)]	
3-743	A	53:25	53:47	The Antarctic Circumpolar Wave is discussed in this section (section 3.6.6.2) and mentioned on line 52-53 on page 52. As correctly described in section 3.6.6.2, there are considerable problems with the ACW, and hence the ACW is no longer widely accepted as a distinct mode of coupled variability. It would appear that the ACW, as a widely disputed and non-accepted part of climate science should be given far less weight, and probably should be cut from having a section on its own (ie., section 3.6.6.2 should be removed). Mentions of its ability to drive the climate should, at least, be deleted, as careful reading of the papers themselves reveal the relationships only exist between 'filtered' data and not real data, and hence the variance explained (of either temp. or rain) by the ACW is minute and of little practical value. [Govt. of Australia (Reviewer's comment ID #: 2001-224)]	

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3-744	A	53:35	53:42	ENSO modulation of the ACW is suggested, as well, in Ribera and Mann, 2003 [Pedro Ribera (Reviewer's comment ID #: 213-15)]	
3-745	A	53:50	54:14	Needs a summary assessment statement about the Indian Ocean dipole here or in section 3.6.7. [Chris Folland (Reviewer's comment ID #: 71-37)]	
3-746	A	53:56	53:56	This description is ambiguous - does it refer to IOZM events of a particular sign or strong events of either sign? [Govt. of Australia (Reviewer's comment ID #: 2001-225)]	
3-747	A	54:6	54:14	There appears to be decadal variability of the IOZM-ENSO relation, I suggest the following two sentences after "…with ENSO": Decadal variability in the interannual correlations between the SST based indices of IOZM and ENSO has been documented by Clark et al. (2003) who found alternating decades of high and low correlation. Model studies suggest that advection of decadally varying thicker or thinner mixed layers from the Pacific through the Indonesian passages may affect the intensity of the upwelling off Sumatra and thus the SST of the IOZM (Annamalai et al., 2005) Ref.: Annamalai, H., J. Potemra, R. Murtugudde, and J.P. McCreary, 2005: Effect of preconditioning on the extreme climate events in the tropical Indian Ocean. J. Climate, 18, 3450–3469. [Friedrich Schott (Reviewer's comment ID #: 228-15)]	
3-748	A	54:6		Two recent papers (Terray P., S. Dominiak and P. Delecluse, 2004 : Role of the southern Indian Ocean in the transitions of the monsoon-ENSO system during recent decades. Climate Dyn, DOI: 10.1007/s00382-004-0480-3 and Terray P. and Dominiak S., 2005 : Indian ocean Surface Temeperature and ENSO : a new perspective, Climate Dynamics; 1351-1368) discuss the correlation patterns between ENSO and Indian ocean and note the change in correlation patterns afer he 70's shift, and proposes a precursor of ENSO in the SE Indian ocean. These results have to be included in this paragraph. [Pascale DELECLUSE (Reviewer's comment ID #: 58-40)]	
3-749	A	54:26		Is the trend in the NAO/NAM really "a major factor" in the observed change in storm tracks, or is the change in storm tracks simply an inherent part of the NAO/NAM trend. [Adrian Simmons (Reviewer's comment ID #: 242-63)]	
3-750	A	54:27	54:28	The SAM changes are described as warming over the Antarctic Peninsula and cooling over the interior of Antarctica, and for surface temperature at least that appears to include all the coastal zone other than the Peninsula (see FIGURE 3.6.7). This seems at first sight to be at odds with the mid-tropospheric warming seen in radiosonde data (see comment #51). Or is the vertical structure of the SAM such that the temperature anomaly changes sign just above the surface? Or are the radiosondes (and consequently ERA-40, which	

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				assimilated their data) wrong? It appears there is something to be explained here, or at least flagged as uncertain. See also comment #73, and several later comments. [Adrian Simmons (Reviewer's comment ID #: 242-64)]	
3-751	A	54:28	54:29	Nowhere in the text previous to this has the SAM been shown to be linked to greenhouse gases. In Section 3.6.5 ("The Southern Hemisphere and Southern Annular Mode"), it says "As for the NAM, the structure and variability of the SAM results mainly from the internal dynamics of the atmosphere although with ozone depletion also playing a role." [references omitted for clarity]. Nowhere are greenhouse gases mentioned. Therefore, they should not be included as potential influences to the SAM in this summary section. [Patrick Michaels (Reviewer's comment ID #: 176-7)]	
3-752	A	54:35	54:37	Re. the previous comment, the text here implies that the IPO-PDO changed ENSO behavior after 1976-77. [Franklin SCHWING (Reviewer's comment ID #: 230-7)]	
3-753	A	54:35	:37	Do these articles demonstrate attribution of the decadal climate change to changes in tropical ENSO evolution, or merely show they coincide? It is equally plausible that mid- and high-latitude changes on decadal scales force the changes in ENSO teleconnections, or they are simultaneously driven by the same variability in forcing. This is quite different from originating in the tropics. The text here implies that the IPO/PDO changed ENSO behavior after 1976-77. [Govt. of United States of America (Reviewer's comment ID #: 2023-251)]	
3-754	A	54:55	54:55	I don't believe the southwest US monsoon meets this definition either – most of this region has a dual rainfall maximum in summer and winter (with the summer one usually being the weaker of the two), and could hardly be described as 'intense' (the mean monthly rainfall in Phoenix in July and August is about 25mm) – the monsoon in this area manifests itself as a sharp increase in humidity from May/June to July/August. This should be referred to the US for a better definition. [Blair Trewin (Reviewer's comment ID #: 266-10)]	
3-755	A	54:56	54:56	(also 3-55, lines 26 and 28). Prefer 'southern Africa' to 'South Africa', unless the reference is only intended to be to the country of South Africa (which I doubt, especially as the rainfall seasonality is more pronounced in Zambia and Zimbabwe than it is in South Africa itself). [Blair Trewin (Reviewer's comment ID #: 266-36)]	
3-756	A	55:2	55:3	This sentence on the global monsoon system does not fit well with those around it. It is information that would be better left for a text book, and not in the IPCC report. The reference to Trenberth et al. (2000) could also be removed. [Govt. of Australia (Reviewer's comment ID #: 2001-226)]	
3-757	A	55:8	55:8	bracket should be after 3.3.2, not after 2004.	

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				[Blair Trewin (Reviewer's comment ID #: 266-78)]	
3-758	A	55:30	55:34	This paragraph seems inappropriate for this chapter, being more relevant for Chs 8 and 9 on models and attribution. [Isaac Held (Reviewer's comment ID #: 105-26)]	
3-759	A	55:30	55:34	There should be a mention to soil moisture variability together with snow, since soil moisture is important in regions other than the Indian monsoon (Himalayas). [Jose Marengo (Reviewer's comment ID #: 159-6)]	
3-760	A	55:31	55:31	The monsoon predictability may also depen on the IOD [Pascale DELECLUSE (Reviewer's comment ID #: 58-41)]	
3-761	A	55:46		What means "not representative" here? Why should the period after 1920 be "representative" of the longer record? One might as well say that the period before 1920 is not representative of the longer record. One could say perhaps that the period 1850-1920 has a character different from that of the period 1920-present. [Fons Baede (Reviewer's comment ID #: 9-37)]	
3-762	A	55:48		Insert sentence: "Vinnikov and Robock (2002) showed that there has been no trend in either South Asian monsoon precipitation or its variability for the past century." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-13)]	
3-763	A	55:49	55:49	Fig 3.7.2 shows a downturn around 1976 as well as clear prior variability. So please link this figure better with the paragraph starting at line 51 [Chris Folland (Reviewer's comment ID #: 71-38)]	
3-764	A	55:55	55:55	insert setence: … change in Korea. Most changes are attributed to increasing heavy rain rate greater than 30 mm/day (Ho et al., 2005). These occurred … ; Ho, C.-H., J.-H. Kim, Y.-B. Lee, K.-M. Lau, K.-M. Kim, and D.-Y. Gong, 2005: Interdecadal changes in heavy rainfall in China during the northern summer. The Journal of Terrestrial, Atmospheric and Oceanic Sciences, 16 (5), 1163-1176. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-4)]	
3-765	A	55:56	55:56	change reference: Gong et al., 2002 -> Ho et al., 2004 ; Ho, C.-H., J.-J. Baik, J.-H. Kim, D.-Y. Gong, and C.-H. Sui, 2004: Interdecadal changes in summertime typhoon tracks. Journal of Climate, 17(9), 1767-1776. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-5)]	
3-766	A	55:56	55:56	replace dual brackets with semicolon. [Blair Trewin (Reviewer's comment ID #: 266-79)]	
3-767	A	56:6	56:6	An error is found, the reference should be: " (Yu et al., 2004b)". For the detail, see: Yu	

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				Rucong, Bin Wang, and Tianjun Zhou, 2004, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270 [Govt. of China (Reviewer's comment ID #: 2006-39)]	
3-768	A	56:8	56:9	This sentence doesn't read well. Prefer 'Rainfall during the Indian monsoon season, which runs from June to September and accounts for about 70% of annual rainfall, exhibits decadal variability'. [Blair Trewin (Reviewer's comment ID #: 266-37)]	
3-769	A	56:18	56:19	State more explicitly what these shifts in the Walker Circulation are. [Chris Folland (Reviewer's comment ID #: 71-39)]	
3-770	A	56:20	56:20	this result, about IOD and Indian rainfal was also discussed in Terray P., P. Delecluse, S. Labattu, L. Terray, 2003 : Sea Surface Temperature Associations with the Late Indian Summer Monsoon. Climate Dyn, vol. 21, 593-618. [Pascale DELECLUSE (Reviewer's comment ID #: 58-42)]	
3-771	A	56:34	56:37	The sentence as it currently reads suggests that a 2004 reference describes events in 2005. Suggest rewording: 'A data set of Northern Australian rainfall (Jones et al., 2004), updated through 2004-2005 (Figure 3.7.3), shows the positive trend…' [Govt. of Australia (Reviewer's comment ID #: 2001-227)]	
3-772	A	56:37	56:38	The sentence "These two wet periods also constitute a large amount of the decadal variability present in the monsoon" can be removed, as strong decadal variations are mentioned a sentence later (with reference to Fig.3.7.3). [Govt. of Australia (Reviewer's comment ID #: 2001-228)]	
3-773	A	56:38	56:40	The information on the work of Wardle and Smith (2004) does not belong in chapter 3, as it is an attempt to attribute the rainfall changes to temperature changes. This sentence should be removed. Chapter 3 should just concentrate on the observed changes. It is not appropriate to draw inferences and make linkages between changes occurring concurrently without undertaking rigorous analysis. This attribution work is the scope of Chapter 9. [Govt. of Australia (Reviewer's comment ID #: 2001-229)]	
3-774	A	56:41	56:42	Prefer 'Latif et al. (1997) have shown that northeastern Australian rainfall was much increased….'. [Blair Trewin (Reviewer's comment ID #: 266-38)]	
3-775	A	56:43	56:43	replace semicolon with comma. [Blair Trewin (Reviewer's comment ID #: 266-80)]	
3-776	A	56:46	56:47	I don't think CAPE is defined at this point; this sentence does not flow from the rest of the paragraph either.	

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				[Chris Folland (Reviewer's comment ID #: 71-40)]	
3-777	A	57:24	57:26	Is the statement that SSTs are responsible for East African rainfall variaibility appropriate for this observational chapter? [Isaac Held (Reviewer's comment ID #: 105-27)]	
3-778	A	57:35	57:35	It would be useful to state on what timescale (eg decadal, multi-decadal) this variability occurs. [Govt. of Australia (Reviewer's comment ID #: 2001-230)]	
3-779	A	57:37	57:37	Suggest inserting 'relatively' before 'uniform'. [Blair Trewin (Reviewer's comment ID #: 266-39)]	
3-780	A	57:55	57:57	It should be noted that Giannini et al (2003) added further detail to many earlier papers of about SST influences. [Chris Folland (Reviewer's comment ID #: 71-41)]	
3-781	A	57:55	57:58	The Giannini et al paper is a modeling study and is discussed in several other chapters -- I don't think we need it here in the spirit of avoiding discussion of attribution in this chapter [Isaac Held (Reviewer's comment ID #: 105-28)]	
3-782	A	57:57	57:57	Held et al. (2005) report that anthropogenic forcing in their historical climate model simulations contributed substantially to precipitation decreases associated with the severe drought conditions in the Sahel during the 1970s and 80s. Ref: Held, I. M., T. L. Delworth, J. Lu, K. L. Findell, and T. R. Knutson, 2005: Simulation of Sahel drought in the 20th and 21st centuries. Proceedings of the National Academy of Sciences, 102(50), 17891-17896. [Thomas Knutson (Reviewer's comment ID #: 132-7)]	
3-783	A	58:10	58:10	Remove 'subtropical highs', because nothing is said about in this subsection [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-6)]	
3-784	A	58:10	58:10	Remove 'subtropical highs', because nothing is said about in this subsection [Govt. of Spain (Reviewer's comment ID #: 2019-66)]	
3-785	A	58:10	59:5	The detail on the Hadley Circulation etc. in this section is unnecessary in an IPCC Assessment Report. Readers can be referred to an appropriate text. This section should focus on changes. [Govt. of Australia (Reviewer's comment ID #: 2001-232)]	
3-786	A	58:10	59:5	The HC is defined and described here in detail; the WC is not at all defined nor decsribed. [Fons Baede (Reviewer's comment ID #: 9-38)]	
3-787	A	58:19	58:19	The references to Trenberth et al. (2000) and Trenberth and Stepaniak (2003a,b) are not necessary as this basic information on the Hadley Circulation is dealt with in text books and numerous other journal publications. [Govt. of Australia (Reviewer's comment ID #: 2001-231)]	

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3-788	A	58:26	58:26	The apparent precision of the quoted 31 latitude doesn't seem to be supported by the results presented in the original reference. 'about 30 latitude' may give a more realistic indication of the accuracy with which the location of the boundary can be determined. [Blair Trewin (Reviewer's comment ID #: 266-11)]	
3-789	A	58:27	:28	Suggest to modify this sentence and add another, as follows: "Tropical SSTs usually determine where the upward branch of the HC is located. However, during the transition from boreal summer to winter, the heating source for the HC in the Western Hemisphere shifts from the Western Hemisphere warm pool centered near the Caribbean Sea (Wang and Enfield, 2003) to the Amazon region in northern South America (Chelliah and Bell, 2004)." [Govt. of United States of America (Reviewer's comment ID #: 2023-252)]	
3-790	A	58:27	:28	Reference: Wang, C., and D.B. Enfield, 2003: A Further Study of the Tropical Western Hemisphere Warm Pool. J. Climate. 16(10), 1476-1493. [Govt. of United States of America (Reviewer's comment ID #: 2023-253)]	
3-791	A	58:31	58:31	Should this be 'towards the equator' rather than 'to the equator'? [Blair Trewin (Reviewer's comment ID #: 266-40)]	
3-792	A	58:38	58:40	Some of us have started looking at this trend towards a stronger Hadley cell in reanalyses because it is diametrically opposed to a robust weakening signal in models. No one I have talked to believes it. The fact that the NCEP and ERA-40 results are so different in the paper cited confirms the conventional wisdom that it is not strongly constrained by the observations. ERA40 has a huge global or tropical mean precipitation trend, totally inconsistent with the observations discussed elsewhere in this chapter, that are undoubtedly driving the trend in the ERA40 Hadley cell. Mitas et al have another paper coming out that is more critical of the reanalysis trends as well. [Isaac Held (Reviewer's comment ID #: 105-29)]	
3-793	A	58:38	:40	Add two more sentences: "It is also possible to interpret the recent upward trend in HC strength to a natural multidecadal oscillation having a time scale that transcends the length of reliable sounding records (Chelliah and Bell, 2004). Until the issues of data integrity and natural variability are satisfactorily resolved, we can conclude little regarding the possible relationship of the HC trend to greenhouse warming." [Govt. of United States of America (Reviewer's comment ID #: 2023-254)]	
3-794	A	58:53		Insert sentence: "Vinnikov and Robock (2002) showed that while there has been a slight downward trend in the SOI (more El Niños) for the past century, there has been no trend in its variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-14)]	

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3-795	A	59:1	59:1	Chang et al: is the reference listed on p82 the correct one? A good reference for decadal variability of the ITCZ is: Chang, P., J. Link and L. Hong, 1997: A decadal climate variation inthe tropical Atlantic Ocean from the thermodynamic air-sea interactions. Nature, 385, 516-518 [Chris Folland (Reviewer's comment ID #: 71-42)]	
3-796	A	59:3	59:3	Add Folland et al (2001) which discusses the influences of interannual and interdecadal tropical Atlantic SST dipole variability on northeastern Brazil rainfall, as well as ENSO. Folland, C.K., Colman, A., Rowell, D.P., and M.K. Davey, 2001: Predictability of North East Brazil rainfall and real-time forecast skill, 1987-1998 J. Climate, 14, 1937-1958. [Chris Folland (Reviewer's comment ID #: 71-43)]	
3-797	A	59:3	59:5	How does a study of only a half century of data distinguish interdecadal (e.g.30 year and longer timescale) variability in one phenomenon from other potentially related or unrelated trends in other phenomena. Implausible claims such as this, especially those which rest on one study of half a century of reanalysis data, should not serve as the basis for conclusions in an assessment report. [Michael Mann (Reviewer's comment ID #: 156-52)]	
3-798	A	59:17	60:26	It should be made more clear here that the type of extremes with are addressed are extremes occurring between 1% and 10% of the time (not really exceptional), and not record values, or with several decades return periods. IPCC statements about the evolution of extremes are generally misinterpreted by decision makers, medias, and the general public, for whom extremes mean really exceptional events. [Govt. of France (Reviewer's comment ID #: 2010-30)]	
3-799	A	59:17		Section 3.8. Relevant here are analyses of streamflow by Lins and Slack (1999, GRL) and by Milly et al. (2002). The former has occasionally been mis-characterized as showing decreasing extremes in streamflow, but because it looks at trends in annual percentiles of flow, it is really looking at changes in more central (not extreme) measures of seasonal distribution of flows. The latter study, in contrast, found an upward trend in extreme flows in large basins globally. Consistent with Lins and Slack (albeit on a different spatial domain), the latter found no significant trend in more central measures of flow (i.e., shorter return period floods, in this case). [P.C.D. Milly (Reviewer's comment ID #: 179-12)]	
3-800	A	59:21	59:30	The Working Group I assessment is about the science aspects of climate change. This section on Changes in extremes should be focussed on this. The first para could be reduced to the second sentence 'Climate Change may be perceived most through ….. ; topics addressed by WGII.' [Govt. of Australia (Reviewer's comment ID #: 2001-233)]	

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3-801	A	59:21	59:21	Delete "increasing" [VINCENT GRAY (Reviewer's comment ID #: 88-437)]	
3-802	A	59:22	59:22	"Climate change may be perceived most through the impacts of extremes…". I don't feel this reads well, maybe could be "Climate change may be most conspicuous through the impacts of extremes…." [John Caesar (Reviewer's comment ID #: 36-3)]	
3-803	A	59:22	59:22	Delete from "Climate Change" to "through" [VINCENT GRAY (Reviewer's comment ID #: 88-438)]	
3-804	A	59:23	59:23	Delete "although these" [VINCENT GRAY (Reviewer's comment ID #: 88-439)]	
3-805	A	59:23	59:23	Delete "to a large degree" [VINCENT GRAY (Reviewer's comment ID #: 88-440)]	
3-806	A	59:27	59:27	Suggest making the meaning of this clearer by changing to '…are increasing in frequency, whether they are or not'. [Blair Trewin (Reviewer's comment ID #: 266-41)]	
3-807	A	59:33	59:33	insert comma after 'TAR'. [Blair Trewin (Reviewer's comment ID #: 266-81)]	
3-808	A	59:48		The Second GCOS Adequacy Report is referred to here as Mason et al. (2003), whereas the GCOS Implementation Plan (prepared in a way similar to the Adequacy Report under Paul's chairmanship) is referred to on line 3 of page 3-60 as GCOS(2004). I think it would be more consistent to refer to both as either GCOS(200x) or Mason et al.(200x), but maybe I've missed a subtle distinction. [Adrian Simmons (Reviewer's comment ID #: 242-65)]	
3-809	A	59:57	59:57	It is more appropriate to say 'hot Western and Central European summer of 2003' here and in other sections. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-7)]	
3-810	A	59:57	59:57	It is more appropriate to say 'hot Western and Central European summer of 2003' here and in other sections. [Govt. of Spain (Reviewer's comment ID #: 2019-67)]	
3-811	A	60:2	60:2	Perhaps it is more appropriate and realistic to say '…has improved a bit'. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-8)]	
3-812	A	60:2	60:2	Perhaps it is more appropriate and realistic to say '…has improved a bit'. [Govt. of Spain (Reviewer's comment ID #: 2019-68)]	
3-813	A	60:10		Add here, "Since the TAR, Vinnikov and Robock (2002) presented a fundamentally new technique for analyzing trends in variance and higher order moments of climatic time series. They illustrated the technique by showing that there have been no trends in the	

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				past 100 years in the variance of US precipitation, US Palmer Drought Index, or All-India Monsoon rainfall. ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025." - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-11)]	
3-814	A	60:28	60:28	Title of the 3.8.2 paragraph is not clear for me [ILEANA MARES (Reviewer's comment ID #: 161-5)]	
3-815	A	60:32	60:34	This sentence on its own does not make sense. Would be better if sentence read "For temperature extremes in the 20th century, the TAR highlighted the lengthening of the growing or freeze-free season in most mid- and high-latitude regions, a reduction in the frequency of extreme low monthly and seasonal average temperatures and smaller increases in the frequency of extreme high average temperatures." [Lisa Alexander (Reviewer's comment ID #: 1-5)]	
3-816	A	60:47	60:47	I suggest to explain the meaning of the Australasia [ILEANA MARES (Reviewer's comment ID #: 161-6)]	
3-817	A	60:56	60:56	Is the most appropriate the use of Australasia word ? [ILEANA MARES (Reviewer's comment ID #: 161-7)]	
3-818	A	61:8	61:9	Prefer 'twice as fast' to '2 times faster'. [Blair Trewin (Reviewer's comment ID #: 266-6)]	
3-819	A	61:13	61:15	There are some concerns about the confidence attached to the results about pre-1900 instrumental temperature extremes, given the issues with homogeneity of daily climate time series as discussed on 3-118. [Govt. of Australia (Reviewer's comment ID #: 2001-234)]	
3-820	A	61:19	61:19	Insert the following before "Vincent and Mekis": For the second half of the 20th century, Shabbar and Bonsal (2003) find less frequent and weaker cold spells in winter (JFM) in western Canada and significantly enhanced winter warm spells across most of Canada. [Xiaolan L. WANG (Reviewer's comment ID #: 282-18)]	
3-821	A	61:21	61:22	The results of Robeson (2004) are mischaracterized. Quoting directly from Robeson (2004): "Using these methods, intense warming is found in the lowest minimum temperatures over western and central North America. During the months of January through March, the lower tail of the daily minimum air temperature distribution over western North America has warmed at rates exceeding 3ºC/50yr…Other times of year in western North America, as well as much of eastern North America, show little change in either minimum or maximum air temperature during the last half-century." This is a far cry from lines 29-30 which state "…as well as intense warming of the lowest daily minimum temperatures over North America (Robeson, 2004)." Further, this section completely ignores the results of Robeson (2003—Climate Research,	

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				22, 205-213) that in the United States, there is an inverse or weak relationship between the mean and the standard deviation of daily air temperature and that implies that that interdiurnal variability of air temperature should either decrease or remain unchanged under warming conditions. And, the section on temperature extremes ignores the results of Knappenberger et al. (2001, Climate Research, 17, 45-53) who find that in the United States, from 1970-1997, the predominance of the warming has occurred during the coldest days and coldest nights of the year, while the temperatures during warmest days and warmest nights have changed relatively less. This is opposite in sense to the warming the occurred from 1910-1939 which was manifest more strongly as a warming of the warmest days. This result supports the result of Robeson (2003) which indicates that the recent increase in mean temperatures has been associated with declining temperature variability. [Patrick Michaels (Reviewer's comment ID #: 176-8)]	
3-822	A	61:21	61:21	Should be 'Robeson (2004) finds' (not 'find'). [Blair Trewin (Reviewer's comment ID #: 266-7)]	
3-823	A	61:22	61:24	The cause-and-effect relationship seems muddled here. Would prefer line 23 be worded: 'were associated with significant increases in the frequency of warm nights'. [Govt. of Australia (Reviewer's comment ID #: 2001-235)]	
3-824	A	61:26	61:37	The text would flow better if the global description came before the regional illustrations. This para should precede para p6- line 38-57. [Govt. of Australia (Reviewer's comment ID #: 2001-236)]	
3-825	A	61:26	61:27	Caesar et al. (2006) have gridded temperature data for 1946-2000 in the cited reference, not 1951-2003 as in the Alexander et al. paper. The Caesar et al. dataset is in the process of being updated for 1946-present. This could be changed to "Alexander et al. (2006) and Caesar et al. (2006) have brought all these and other regional results together, gridding the common indices for the period 1951-2003 and daily data for 1946-2000." [John Caesar (Reviewer's comment ID #: 36-4)]	
3-826	A	61:27	61:27	"76%" should be "74%" as comment 1. [Lisa Alexander (Reviewer's comment ID #: 1-6)]	
3-827	A	61:29	61:29	"72%" should be "73%" as comment 2. [Lisa Alexander (Reviewer's comment ID #: 1-7)]	
3-828	A	61:31	61:33	The current wording implies no trend in maximum temperatures. Would prefer "This is consistent with minimum temperatures increasing more rapidly than maximum temperatures, leading to a reduction in DTR….'. [Govt. of Australia (Reviewer's comment ID #: 2001-237)]	
3-829	A	61:33	61:37	The change in shape is not described (incresae or decrase of variance). The link between	

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				the "almost equal change in indices" and the conclusion about the "warming of the cold tail higher than the warm tail" is difficult to understand (period 1951-2003). There is no analysis for 1979-2003 where the values are quite different for cold and warm tails (and in this case suggest increase of variability). [Govt. of France (Reviewer's comment ID #: 2010-31)]	
3-830	A	61:35	61:37	It is not obvious from the text why an equal change in the cold and warm indices would imply that the cold tails of the distribution have warmed more than the warm tails. [Govt. of Australia (Reviewer's comment ID #: 2001-238)]	
3-831	A	61:39	61:49	Table 3.6 Why are precipitiations presented in this chapter? (this line of the table is not analysed further) [Govt. of France (Reviewer's comment ID #: 2010-32)]	
3-832	A	61:39	61:49	Table 3.6 (legend) : global trends -> precision of number of stations (202?) [Govt. of France (Reviewer's comment ID #: 2010-33)]	
3-833	A	61:39	61:49	Table 3.6 (legend) : difficult to understand how are built the global indices TN10, TN90 TX10 and TX90 which are usually built station by station [Govt. of France (Reviewer's comment ID #: 2010-34)]	
3-834	A	62:18	62:18	Author is spelt 'Ratnayatke' here but 'Ratnayake' in the references. [Govt. of Australia (Reviewer's comment ID #: 2001-239)]	
3-835	A	62:18		After first sentence in paragraph, add "Vinnikov and Robock (2002) showed that while there was a significant upward trend in US mean annual precipitation for the past century, there has been no trend in precipitation variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-12)]	
3-836	A	62:19	62:19	Suggest using 'moderately' (not 'moderate'). [Blair Trewin (Reviewer's comment ID #: 266-8)]	
3-837	A	62:20	62:20	It should be stated whether the 75th and 95th percentile quoted here refer to all days or only days with precipitation. [Govt. of Australia (Reviewer's comment ID #: 2001-240)]	
3-838	A	62:25	62:28	This statement about greater precipitation increases in the extremes when compared with the mean in the United States is not supported by work by Michaels et al. (2004, Int. J. Climatology, 24, 1873-1882), who set out specifically to determine whether the extremes were changing at a different rate than the mean—an issue that is being directly discussed in this section of Chapter 3. Michaels et al. conclude that: "Our results support the contention that, where changes are significant, there is an	

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				increase in the amount of rain occurring on heavy rain-days. However, our results provide no support for the contention that the increase in total annual rainfall observed across the United States is disproportionately occurring on the wettest days—a contention that may have arisen from methodological constraints rather than true changes in the nature of precipitation delivery. After allowing for the total rain increases within each of our seven regions, we find no consistent national behavior in the U.S. precipitation record. Increases are indeed disproportionate for ranked days four through ten in the Southeast, but there is a balancing disproportionate decrease in the Northwest and in the Pacific Southwest. Our results argue strongly that the increase in rainfall in the coterminous 48 states that has been observed in the last 100 years has not resulted in any systematic disproportion in the percent of that increase allocated to the heaviest rain days." At the very least, this conflicting finding should be mentioned, considering, again, that Michaels et al. (2004) set out specifically to determine whether the extremes were changing at a different rate than the mean—an issue that is being directly discussed in this section of Chapter 3—and concluded that they were not over the United States as a whole. How can this result be ignored in a comprehensive review such as the IPCC AR4? [Patrick Michaels (Reviewer's comment ID #: 176-9)]	
3-839	A	62:56		Section 3.8.2.2 I don't believe that the results for global precipitation extremes are 'robust'. Indeed I have yet to be convinced that any study has sufficiently addressed data quality and homogeneity issues, properly identified what is 'extreme' and/or used appropriate statistical techniques for analysis. The spatial patterns are just not coherent enough globally to confirm a robust picture of change. In fact there are still many regions that do not have sufficient data for analysis and most of the individual regional results show mixed patterns of change or changes that are not consistent with 'disproportionate' change. While Alexander et al., 2006 do show that the contribution from very wet days has increased, there are still many land areas that were not included in the analysis (Fig. 3.8.2a). And while Groisman et al., 2005 analysed a larger area (Fig. 3.8.2c), data for India, for example, only have relatively short records and did not exist prior to the early 1970s (if, as I believe, the GHCN-Daily dataset was used). I agree that there are now strong indications that Europe and North America are showing these disproportionate changes in extremes vs mean but I can't agree that this is a robust global finding. [Lisa Alexander (Reviewer's comment ID #: 1-15)]	
3-840	A	63:0		General comment: This is much improved over the first version [Kerry Emanuel (Reviewer's comment ID #: 61-1)]	
3-841	A	63:2	60:2	Figure 3.8.2a does not correspond with the description of the Figure given at the page 164	

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				[ILEANA MARES (Reviewer's comment ID #: 161-8)]	
3-842	A	63:6	63:6	Do not think the word "robust" is appropriate given that you appear to be only referring to the results from one study. See comment 15. [Lisa Alexander (Reviewer's comment ID #: 1-8)]	
3-843	A	63:6	63:6	Figure 3.8.2b does not correspond with the description of the Figure given at the page 164 [ILEANA MARES (Reviewer's comment ID #: 161-9)]	
3-844	A	63:23	63:23	Figure 3.8.2b might be Figure 3.6.2c, namely Lower Figure from Figure 3.8.2 from page 164 [ILEANA MARES (Reviewer's comment ID #: 161-10)]	
3-845	A	63:28	63:33	Note that one interesting result from this analysis that you have not mentioned here is that longer duration extremes (i.e. 5 and 10 day extremes) are changing much more than the shorter duration (1 and 2 day extremes). Different patterns are seen in different seasons with reductions in the summer (related to reductions in mean rainfall?) and increases in all other seasons, particularly autumn. [Hayley Fowler (Reviewer's comment ID #: 74-1)]	
3-846	A	63:38	63:40	The conclusion lines 38-40 is not consistent with the synthesis page 3-76 lines 30-32 .I think the latter is the right conclusion, as demonstrated theoretically by Frei and Schär (2001). These authors show e.g. that using a 100 year record of seasonal counts, a frequency change by a factor of 1.5 can be detected with a probability of 0.6 for events with a return period of 30 days, which drops to 0.2 for events with a return period of 100 days. For a return period of several decades, the probability becomes nearly zero. We+H47 recommend to replace conclusion of 3.8.2.2 by "Increases have been also reported for rarer precipitation events (several decades return period), but practically no regions have sufficient data to assess such trends reliability (Frei and Schär, 2001, already in the existing bibliography)." [Govt. of France (Reviewer's comment ID #: 2010-35)]	
3-847	A	63:42	68:9	In that part dealing with cyclones, one should not privilege too much the potential risk, which is real, but not due 100% to global warming (see e.g. F. Chauvin, J.F. Royer and M. Déqué, 2006: Response of hurricane-type vortices to global warming as simulated by ARPEGE-Climat at high resolution. Climate Dynamics, DOI 10.1007/s00382-006-0135-7) [Govt. of France (Reviewer's comment ID #: 2010-36)]	
3-848	A	63:44	63:44	Delete "Change" [VINCENT GRAY (Reviewer's comment ID #: 88-441)]	
3-849	A	63:44	64:34	Box 3.5 deals with a very topical issue and will attract much interest. It does begin to describe how greenhouse gases will affect tropicalcyclones, but should go further, with clear statemetns of what we can expect and what we don't know. This is what the title of	

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				the box appears to promise. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-18)]	
3-850	A	63:47	63:47	In this context 'high water vapour' should be hyphenated. [Blair Trewin (Reviewer's comment ID #: 266-42)]	
3-851	A	63:53	63:53	Briefly explain the Carnot cycle. [Chris Folland (Reviewer's comment ID #: 71-44)]	
3-852	A	63:53	63:54	What does mean "Hence tropical cyclones appear to play a key role in ameliorating the heat from the summer sun over the ocean" ? Does that mean "alleviating", which should be more appropriate [Govt. of France (Reviewer's comment ID #: 2010-37)]	
3-853	A	63:56	63:19	The moist static energy that fuels convection is only available if there exist relatively colder and drier air in the middle troposphere. In addition, it cannot be assessed a priori that the extension of the areas where SST exceeds 26°C will lead to an extension of the areas over which such storms can form. This presentation is certainly not appropriate and potentially misleading: it is said that SST is increasing, and that consequently the areas where cyclones form will expand, before modulating that by saying that other factors must be taken into account. There is a strong risk that only the first or two first sentences will be retained, e.g. in the Summary for policy makers. [Govt. of France (Reviewer's comment ID #: 2010-38)]	
3-854	A	63:56	63:56	Replace "As" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-442)]	
3-855	A	63:56	63:56	Delete "continue to" [VINCENT GRAY (Reviewer's comment ID #: 88-443)]	
3-856	A	63:56	64:19	CAPE is given attention beyond its importance for tropical storms. [Govt. of Australia (Reviewer's comment ID #: 2001-241)]	
3-857	A	64:3	64:19	We should avoid mentioning the magic "26C" SST requirement for tropical storm genesis unless it is explained carefully as simply corresponding roughly to the moist enthalpy near the ground needed to convect to the tropopause, a number that everyone agrees grows as the climate warms. The naive impression that is sometimes given that tropical storms will become more frequent simply because more of the ocean rises above 26C is simply wrong. It might be best to just avoid this number. Once again, wouldn't this chapter be better off sticking to description of trends/variability, especially avoiding mention of "subgrid scale convection" in models for example. [Isaac Held (Reviewer's comment ID #: 105-30)]	
3-858	A	64:15	64:15	"aloft" is too vague. [Chris Folland (Reviewer's comment ID #: 71-45)]	

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3-859	A	64:24	64:24	'velocity' – of what? I presume the reference is to wind speed defined in some way (peak gusts? maximum sustained winds, and if so, defined over what period? some measure of the overall cyclone wind field?). This sentence needs to be more precise. [Blair Trewin (Reviewer's comment ID #: 266-43)]	
3-860	A	64:39	64:39	should read '…therefore required (see also Box 3.5).' [Blair Trewin (Reviewer's comment ID #: 266-82)]	
3-861	A	64:45	64:45	The word 'velocity' needs clarification,. Is it a reference to wind speed defined in some way (peak gusts? maximum sustained winds, and if so, defined over what period? some measure of the overall cyclone wind field?). [Govt. of Australia (Reviewer's comment ID #: 2001-242)]	
3-862	A	64:45	64:47	The ACE is indeed a good measure of the integrated intensity of cyclones, but it relies on indirect (extrapolation of airborne winds to surface winds) or very indirect (Dvorak method using satellite images) estimated winds [Govt. of France (Reviewer's comment ID #: 2010-39)]	
3-863	A	64:45	64:45	I think the cross-reference to Figure 3.8.3 here is unnecessary as this sentence contains no reference to the results in the figure and it's cross-referenced again later in the paragraph. 'see Box 3.5' should also be in brackets. [Blair Trewin (Reviewer's comment ID #: 266-44)]	
3-864	A	64:52	64:52	Because ACE only squares the maximum sustained wind speeds, it DOES NOT integrate size and intensity. Suggests deleting this sentence. [Johnny Chan (Reviewer's comment ID #: 39-1)]	
3-865	A	64:54		There is reference here to the "intensity and strength" of tropical storms. Is there some subtle difference between what is meant by intensity and what is meant by strength? Or is this a simple mistake, with "intensity and size" intended. [Adrian Simmons (Reviewer's comment ID #: 242-67)]	
3-866	A	65:2	65:3	Potential Intensity does not depend on CAPE [Kerry Emanuel (Reviewer's comment ID #: 61-2)]	
3-867	A	65:2	65:3	It is inappropriate and possibly misleading to start the sentence by saying that th PI index exhibits a strong positive trend, before saying just after that the uncertainties are very large, and may lead to erroneous long term trends. [Govt. of France (Reviewer's comment ID #: 2010-40)]	
3-868	A	65:8	65:8	This appears to be an inappropriate use of the term 'likely' in the context of its formal definition. The authors should review this sentence. [Govt. of Australia (Reviewer's comment ID #: 2001-243)]	
3-869	A	65:8	65:8	Appears to be an inappropriate use of 'likely' - see general comments. [Blair Trewin (Reviewer's comment ID #: 266-87)]	

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3-870	A	65:13	65:14	"the initial Emanuel report has been revised to show the PDI increasing by about 75% (versus about 100%) since the 1970s (Emanuel 2005b)." The reference cited does not appear to present this information. [Thomas Knutson (Reviewer's comment ID #: 132-8)]	
3-871	A	65:15	65:19	In "These relatioships have been reinforced …… in the North Pacific, Indian and Southwest Pacific oceans.", suggest to specify the database used as results are sensitive to the dataset used. [Chiu-Ying LAM (Reviewer's comment ID #: 139-1)]	
3-872	A	65:21	65:30	Chan (Science 311, 1713b, 2006).also challenged Webster et al.'s (2005) result in two aspects. First, while it is true that the ACE in the western North Pacific has increased during the period 1970-1995, the number falls after 1995 and the peak value is very similar to that from 1960-1974. Thus, the increase is nothing but part of the large interdecadal variation. Second, the increase stopped after the mid 1990s and the ACE values have been on the decrease since. [Johnny Chan (Reviewer's comment ID #: 39-2)]	
3-873	A	65:22	65:22	Propose after ".....date of the 1970s." add "As pointed out by, e.g., Lander (2006), different intensities may be assigned to the same tropical cyclone by different centers. Thus the results obtained may also depend on the tropical cyclone best track database that is used.". It is suggested that this should also be reflected in the paragraph starting from line 52 on SPM-8 and line 16 on TS-22. [Chiu-Ying LAM (Reviewer's comment ID #: 139-2)]	
3-874	A	65:24	65:30	I have several comments on these statements. It is not clear that adjustments to the data have had little effect on overall trends. I believe Emanuel's adjustments of the NW Pacific data did have substantial effects on the trend. As an aside, Knaff and Sampson presented a paper at the AMS meeting in Monterey reporting that in their reanalysis of the NW Pacific data over 1968-1986, they found a strong reduction in the trends compared to the original data. This grey literature study is too late to include in IPCC however. But it does at least raise the question of how robust the reported trends are for that basin. It will be interesting to see whether Knaff and Sampson's result will hold up to scrutiny. Also the comment that the Atlantic PDI was not as high in the earlier years references Emanuel's Nature paper (2005a) which was revised based on Landsea's (2005) comment. In Landsea's revision (Fig 1b) the most recent years seem similar to the late 1940s and 1950s, especially if one does not adjust the data (Landsea has changed his mind since his earlier papers and now recommends against the adjustment...) [Thomas Knutson (Reviewer's comment ID #: 132-11)]	
3-875	A	65:28	65:30	This sentence seems odd. The paragraph in which it appears at the end, and the preceding paragraph, refer to the PDI, with an early definition and reference to Emanuel(2005a). Yet	

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				in this sentence there is a reference to the "power dissipiation index (Emanuel, 2005a)" as if it is something new. [Adrian Simmons (Reviewer's comment ID #: 242-68)]	
3-876	A	65:42	65:51	It is a bit of a stretch to state that the global ACE values is "not considered sufficiently reliable" to display, but then go on to describe how they corresponds to global temperatures. That is, "the highest ACE year through 2005 is 1997, when a major El Nino event began and surface temperatures were subsequently the highest on record (see Section 3.2)." Actually, Section 3.2.2.4 lists 2005 and/or 1998 as the hottest year on record. I would contend that "global" temperatures are not the best metric of "tropical" cyclone activity. Klotzbach (GRL, 2006) just published a paper in which he calculated the global ACE from 1986-2005 as well as the temperature averaged over the tropics (23.5S to 23.5N). He found little correspondence between tropical temperatures and global ACE. While he did find that the global ACE value was highest in 1997 (and 1992 was the second highest), tropical temperatures during 1997 have been exceeded many times since then, and tropical temperatures in 1992 have been were well below the 1986-2005 mean. I propose that in light of these concerns (unreliability of your calculations, and the calculations of Klotzbach that show low correlations between tropical temps and global ACE) that this entire section be removed. As it now stands it is far from being a robust analysis. [Patrick Michaels (Reviewer's comment ID #: 176-10)]	
3-877	A	65:42		Seems odd to say that the figure is not shown because it is not reliable, yet then discuss it for several more sentences. Why should we conclude that the discussion is reliable? [David Rind (Reviewer's comment ID #: 214-27)]	
3-878	A	65:42		Seems odd to say that the figure is not shown because it is not reliable [nor publication referenced], yet then discuss it for several more sentences. Why should we conclude that the discussion is reliable? [Govt. of United States of America (Reviewer's comment ID #: 2023-255)]	
3-879	A	65:48		Likewise, here change "Emanuel's (2005a) power dissipiation index" to The PDI" [Adrian Simmons (Reviewer's comment ID #: 242-69)]	
3-880	A	65:50	65:51	This statement is NOT TRUE. Their plot shows a flattening of the number within the last two 5-year periods. The table is misleading. See also Chan's (Science 311, 1713b, 2006) table that shows almost the same number in the period 1960-1974. Thus, this sentence should be modified to reflect (1) the interdecadal nature of such variations, and (2) the flattening of the curve after the mid 1990s. [Johnny Chan (Reviewer's comment ID #: 39-3)]	
3-881	A	65:55	66:22	Which correlation between Greater PDI and more Cat-4 & 5, vs. Niño years which are favouring them? In other terms, there is a confusion between long term trend and more	

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				frequent « sporadic occurrence » due to a preferred ENSO phase during the considered period. [Govt. of France (Reviewer's comment ID #: 2010-41)]	
3-882	A	65:55		Section 3.8.3.1 The observations in the Western Pacific suffer from the lack of quality observations prior to about the mid-1980s. Even when satellite images became available, the techniques for assessing wind speeds from them (Dvorak technique) did not become perfected until the mid-1980s. As Section 3.8.3.2 begins with a paragraph on the history and reliability of observations in the North Atlantic, so too should section 3.8.3.1 begin with a history and reliability of the observations for the Western North Pacific. Further, Klotzbach (GRL, 2006) shows no trend in Western North Pacific ACE from 1986-2005 and a significant decline in Eastern North Pacific ACE over the same time period. Further, Klotzbach shows virtually the same number of category 4 and 5 storms in the 10-yr period from 1986-1995 (75) as he does for the period 1996-2005 (76) in the Northwest Pacific basin. This tells a completely different story to the one told by Webster et al. The text should be modified to reflect this contrasting result. [Patrick Michaels (Reviewer's comment ID #: 176-11)]	
3-883	A	66:5	66:5	I prefer either 'of the order of' or 'of approximately' rather than 'of order'. [Blair Trewin (Reviewer's comment ID #: 266-45)]	
3-884	A	66:6	66:6	After '….1975-1989.' add 'As noted above, these conclusions may vary depending on the best track data set used'. [Chiu-Ying LAM (Reviewer's comment ID #: 139-3)]	
3-885	A	66:9	66:10	This sentence needs reordering. Suggest '…associated with ENSO, and not local SSTs, is the dominant factor in hurricane activity (Chan and Liu, 2004).' [Blair Trewin (Reviewer's comment ID #: 266-46)]	
3-886	A	66:18	66:22	There is some debate about whether there was a weak El Niño in 2004 (and if one did occur it peaked after the bulk of the tropical cyclone season was over). This section does not really add anything to the discussion of climate change and could be removed, although the record number of Japanese landfalls could be mentioned as a case study of an extreme event. [Govt. of Australia (Reviewer's comment ID #: 2001-244)]	
3-887	A	66:22	66:22	insert reference: (Kim et al., 2005; Levinson, 2005); Kim, J.-H. and C.-H. Ho, and C.-H. Sui, 2005: Circulation features associated with the record-breaking typhoon landfall on Japan in 2004. Geophysical Research Letters, 32, L14713, doi:10.1029/2005GL022494. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-6)]	
3-888	A	66:24	67:16	There exist a certain and proven correlation between the Atlantic Multi-decadal Oscillation (AMO) and cyclonic activity, with a warm phase from 1930 to 1970, a cool	

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				one from 1970 to 1990, and again a warm phase since 1990. The other factors (QBO, high subtropical pressures, activity of the African monsoon) go in the same way, with an enhanced inter-annual variability. It cannot be stated that global warming is as responsible as AMO of the cyclonic activity. [Govt. of France (Reviewer's comment ID #: 2010-42)]	
3-889	A	66:41	66:42	This statement is likely wrong. First define what is meant by "recent": 1985-2005?.Knight et al (2005) show that in the last 20 years a substantial part of the warming in the N Atlantic is very likely associated with AMO - which is why the N. Atlantic north of 30N especially is by about 0.3C the fastest warming part of the global ocean over this period. Figure 3.6.8 needs redrawing to approximately remove global warming so as to show the AMO magnitude in the two regions. This will allow the relative magnitude of the AMO component and the residual global warming effect to be assessed. We would expect the global warming component in the N Atlantic to look closely like the global mean warming of the SSTs over the same period, but with the N Atlantic left out. The remainder of the warming is likely mostly the AMO. Fig 3.6.8 could be recast without any increase in size to show the estimated AMO and global warming components clearly. [Chris Folland (Reviewer's comment ID #: 71-46)]	
3-890	A	66:41	66:42	"…rather than the AMO." This statement seems misleading to me and in any case needs some reference to substantiate. It appears that Main Development Region SSTs have warmed roughly similarly to global mean temperature (e.g., Knutson et al. 2006, and Emanuel has shown in a recent AMS conference paper that MDR SST tracks NH mean late summer temperatures quite closely). Knutson et al. 2006 conclude that an anthropogenic warming signal may now be emerging in the MDR. However, I don't believe we are presently able to determine the relative contributions of anthropogenic forcing and internal variability (e.g., AMO) to the latest warming of the past few decades in the tropical Atlantic region with high confidence. Ref: Knutson, T. R., T. L. Delworth, K. W. Dixon, I. M. Held, J. Lu, V. Ramaswamy, D. Schwarzkopf, G. Stenchikov, and R. J. Stouffer, 2006. Assessment of twentieth-century regional surface temperature trends using the GFDL CM2 coupled models. J. Climate, 19(9), 1624-1651. [Thomas Knutson (Reviewer's comment ID #: 132-10)]	
3-891	A	66:41	66:42	How can you build a case for AMO impacting the Atlantic SSTs in the previous several lines and then virtually dismiss it in lines 41-42 without any reference? What evidence is this dismissal based upon? For instance, Knight et al. (2005) show that a good 0.4ºC of the observed rise in tropical Atlantic SSTs during the past several decades can be attributed to the AMO—that is more than half of the rise depicted by Webster et al. (2005). [Patrick Michaels (Reviewer's comment ID #: 176-12)]	

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3-892	A	66:41	:42	There is no basis for saying that "most of the present warming is associated with global SST increases rather than the AMO". Suggest the following modification of this sentence: "Nevertheless, it appears that a significant though lesser portion of the present warming is associated with global SST increases rather than the AMO." [Govt. of United States of America (Reviewer's comment ID #: 2023-256)]	
3-893	A	66:44	66:51	I think this section might read better if we have the contrasting numbers from 1995-2004 and 1970-1994 as close as possible to each other. My suggested reordering: 'During 1995-2004, hurricane seasons averaged 13.6 tropical storms, 7.8 hurricanes and 3.8 major hurricanes, and had an average ACE index of 159% of the median. In contrast, during the previous 1970-1994 period, hurricane seasons averaged 8.6 tropical storms, 5 hurricanes and 1.5 major hurricanes, and had an average ACE index of only 70% of the median. NOAA classifies all but two of the 1995-2004 seasons (the exceptions being the El Niño years of 1997 and 2002) as above normal (with respect to a 1981-2000 average), whilst in the 1970-1994 period, twelve of the 25 seasons were classified as below normal, ten as near-normal and only three (1980, 1988 and 1989) as above normal. (The record-breaking 2005 season is documented in more detail in Box 3.6.6).' [Blair Trewin (Reviewer's comment ID #: 266-47)]	
3-894	A	66:46	66:47	Repetition. The third sentence of this paragraph simply repeats what is said in the previous paragraph. [Adrian Simmons (Reviewer's comment ID #: 242-70)]	
3-895	A	67:1	67:16	Hoyos et al. (2006) also showed that the moist static stability significantly decreased (dramatically starting in 1995) across the North Atlantic tropical cyclone basin which also is a change favorable to hurricane development and intensification. This should be mentioned here. [Patrick Michaels (Reviewer's comment ID #: 176-13)]	
3-896	A	67:9	67:9	Multidecadal variation - of SST? If so, this is likely the AMO. This multidecadal SST/ Sahel relationship in the observations was orginally shown by Folland, C.K., Parker, D.E. and T.N. Palmer, 1986: Sahel rainfall and worldwide sea temperatures 1901-85. Nature, 320, 602-607. [Chris Folland (Reviewer's comment ID #: 71-47)]	
3-897	A	67:15	67:16	The 2005 Atlantic hurricane records are now pubished, and the PDI was a little higher than 2004, making it the highest year on record. [Kerry Emanuel (Reviewer's comment ID #: 61-3)]	
3-898	A	67:15	67:16	"In 2004, the Power…" this statement would be acceptable if referring to US landfalling PDI and referenced to Landsea (2005). However, it is not clear from Landsea's (2005) Atlantic basin-wide results that 2004 is by far higher than the data since 1949 (and it	

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				doesn't even go back to 1930). I think that Landsea now recommends using the original (unadjusted) data from which I would infer from his (2005 reference) Fig. 1 a and b, that the statement that 2004 is far higher than other years since 1949 is not correct. It appears that 1949 would be roughly comparable to 2004 but its hard to tell for certain from those figures. [Thomas Knutson (Reviewer's comment ID #: 132-9)]	
3-899	A	67:18	67:44	The conclusion is that within North-Eastern and South Western Pacific, and Indian Ocean, which amount for a significant part of the worldwide statistics, the trend is probably a decrease, with a strong modulation by ENSO. No long term trend appears on the total number of cyclones, and the study by Webster et al. (2005) is the only one giving so definitive results about the increase of Cat-4 and 5 cyclones [Govt. of France (Reviewer's comment ID #: 2010-43)]	
3-900	A	67:18		Section 3.8.3.3 Klotzbach (GRL, 2006) found a recent decrease in the number of category 4 and 5 storms from 37 in the period 1986-1995 to 23 in the period 1996-2005. This casts the findings of Webster et al. in a different light and suggests that analyses such as these are very sensitive to the time period being examined. The Klotzbach results indicate that whatever increase there has been in category 4 and 5 storms in the Northeast Pacific basin, the increase has not been maintained in recent years. [Patrick Michaels (Reviewer's comment ID #: 176-14)]	
3-901	A	67:28		How is the eastern North Pacific affected by the AMO? [David Rind (Reviewer's comment ID #: 214-28)]	
3-902	A	67:28		How is the eastern North Pacific affected by the AMO? [Govt. of United States of America (Reviewer's comment ID #: 2023-257)]	
3-903	A	67:31		Section 3.8.3.4 More description should be given to the data limitations in these areas, both currently and historically. [Patrick Michaels (Reviewer's comment ID #: 176-15)]	
3-904	A	67:37	67:57	There is inconsistency (both in the text here and in the original source) between the eastern boundary of the South Indian Ocean region and the western boundary of the Australian region (both 105 and 110 E are used in different places of the original source). A western boundary (which appears from the source to be 160 E, although not explicitly stated) of the South Pacific region also needs to be defined in line 53. [Govt. of Australia (Reviewer's comment ID #: 2001-245)]	
3-905	A	67:38	67:38	'calendar year' and 'season' are the wrong way round in this sentence and should be swapped. [Blair Trewin (Reviewer's comment ID #: 266-48)]	

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3-906	A	67:46		Section 3.8.3.5 More description should be given to the data limitations in these areas, both currently and historically. [Patrick Michaels (Reviewer's comment ID #: 176-16)]	
3-907	A	68:2	68:9	I suggest the usse of the word possibly (as… "possibly the first….") [Jose Marengo (Reviewer's comment ID #: 159-7)]	
3-908	A	68:3	68:3	Give the latitude and longitude of the landfall. [Chris Folland (Reviewer's comment ID #: 71-48)]	
3-909	A	68:13	69:17	This section on changes in extratropical storms could be consolidated with the section on storm tracks (p3-42 line 1-52.) to avoid overlap. [Govt. of Australia (Reviewer's comment ID #: 2001-246)]	
3-910	A	68:18	68:18	The citation "Wang et al., 2006" should be replaced by "Wang et al., 2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-14)]	
3-911	A	68:23	68:31	I've no problem with what is written in this paragraph, but it is noteworthy that no question is brought up here as to the reliability of reanalyes for study of NH cyclone statistics. This is in contrast to the "significant uncertainties" referred to (with reference to storm tracks) in line 16 of Page 3-46. See comment #58 also. Some consistency of view would be helpful, but if different contributors have different views as to the reliability of reanalyses, this should perhaps be stated. [Adrian Simmons (Reviewer's comment ID #: 242-71)]	
3-912	A	68:23	68:25	It is not true that "only the North Pacific trend is statistically significant (Simmonds and Keay, 2002; Wang et al., 2006)", because Wang et al. 2006 (updated to Wang et al., 2006a) show highly significant trends over both the North Atlantic and the North Pacific in winter (JFM) (see their Figures 5 and 10). The statement also controdicts with lines 41-43 on this page. This sentence must be modified. Actually, I pointed this out in my comment (#20) on the First Order Draft. [Xiaolan L. WANG (Reviewer's comment ID #: 282-7)]	
3-913	A	68:25	68:25	The citation "Wang et al., 2006" should be replaced by "Wang et al., 2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-15)]	
3-914	A	68:39	68:39	Is there a succinct definition for 'explosively-deepening' which could be used here? [Blair Trewin (Reviewer's comment ID #: 266-49)]	
3-915	A	68:56	69:1	Replace the sentence of "A study of rapid pressure changes … over Iceland (Alexander et al., 2005)." with "Studies of rapid pressure changes at stations indicate an increase in the frequency, duration, and intensity of winter cyclone activity over the lower Canadian Arctic and in the number and intensity of severe storms over the southern U.K. since	

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				1950s, but a decrease over southern Canada and Iceland (Wang et al., 2006b; Alexander et al., 2005).", because Wang et al. (2006b) also analyzed rapid pressure changes at 83 Canadian stations (one of the very few studies analyzing rapid pressure changes since 1950s to study cyclone activity). [Xiaolan L. WANG (Reviewer's comment ID #: 282-4)]	
3-916	A	69:1	69:3	"Thus the station pressure…" This conclusion is unwarranted. The reports reviewed suggest increasing storminess from the late-1950s to the late 1980s followed by a decline to more average levels over the past decades. [Patrick Michaels (Reviewer's comment ID #: 176-17)]	
3-917	A	69:7	69:7	Would prefer '…in extreme westerly wind events'. [Blair Trewin (Reviewer's comment ID #: 266-50)]	
3-918	A	69:21	69:44	I am Contributing Author for WG1 Ch3, Sec. 3.8.4.2. For this review phase, I have prepared an updated version of that Section, which I will submit to my LA and the TSU in a separate eMail. [Nikolai Dotzek (Reviewer's comment ID #: 59-2)]	
3-919	A	69:21	69:38	The section on severe local storms is quite short, most likely due to the still inadequate volume of available severe local storm observations worldwide. However, the mere statement "data not adequate to draw conclusions" is quite poor. Let me suggest that the new IPCC report makes a clear and strong claim that the present lack of homogeneous data on severe local storms worldwide must be overcome in the near future. Such a supporting claim by the IPCC would greatly help ongoing initiatives to provide better-quality and homogeneous data on such events, e.g. in Europe with the European Severe Weather Database ESWD (www.eswd.eu), or even in the USA where homogeneity of the data remains an issue despites the large volume of data there. [Nikolai Dotzek (Reviewer's comment ID #: 59-4)]	
3-920	A	69:21		Verbout et al. (2006) [Verbout, S. M., H. E. Brooks, L. M. Leslie, and D. M. Schultz, 2006: Evolution of the US tornado database: 1954-2003. Wea. Forecasting, 21, 86-93.] showed that changes in procedures for evaluating the intensity of tornadoes in the US introduced significant discontinuities in the record, leading to a reduction in the effective length of the climatological record for some purposes. In particular, the apparent decrease in strong tornadoes in the US from the early period of the official record (1950s-1970s) to the more recent period is, in large part, a result of the way damage from the earlier events was evaluated. [Harold Brooks (Reviewer's comment ID #: 31-2)]	
3-921	A	69:26	69:27	The Tyrrell (2003) reference is a strange choice to reference here. It would be much better to reference the special volume from the First European Severe Storms Conference (Snow, J. T. (ed.), 2001: Special Issue: Conference on European Tornadoes and Severe	

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				Storms. Atmospheric Research, 409 pp.) Between those two volumes, the subject is covered very well. The Tyrrell reference covers one country and the story is more dramatic in other countries (e.g., the German database) discussed in more than one paper in those two special volumes. [Harold Brooks (Reviewer's comment ID #: 31-3)]	
3-922	A	69:35	69:38	An IPCC workshop (IPCC, 2002: IPCC Workshop on Changes in Extreme Weather and Climate Events Workshop Report, Beijing, 11-13 June 2002, 107 pp.) recommended another approach to the problem. The suggested approach was to develop relationships between severe thunderstorm occurrence and larger-scale environmental conditions in places where the observations of events are fairly good and then consider the distribution of those environments. To date, that has been done with NCAR/NCEP reanalysis data (Brooks, H. E., J. W. Lee, and J. P. Craven, 2003: The spatial distribution of severe thunderstorm and tornado environments from global reanalysis data. Atmos. Res., 67-68, 73-94.) to estimate the mean distribution, but time trends have not been identified. [Harold Brooks (Reviewer's comment ID #: 31-4)]	
3-923	A	69:35	69:38	Would it be appropriate one of the fundamental problems explicitly here-the lack of reporting systems to collect severe thunderstorm information in most countries? A significant limitation to implementing the IPCC (2002) Workshop recommendation is the lack of databases to do the developmental work relating environments to events. Even without a long record of reports, a reasonably short record could be useful in enabling researchers to utilize the existing records of large-scale environmental conditions. [Harold Brooks (Reviewer's comment ID #: 31-5)]	
3-924	A	69:44	69:44	delete full stop after 'speed'. [Blair Trewin (Reviewer's comment ID #: 266-83)]	
3-925	A	69:46	72:22	We query the value of having a box of this length on post 2001 Specific Extreme events. The events chosen are not balanced geographically neither is there evidence provided that as a whole they constitute an exceptional collection of extreme events. The first para 46-57 with a short list of recent extreme events (the edited sentence below), and a reference to the annual WMO Climate Statements may be sufficient to make the point. The last sentence (56-57) could be replaced with 'These events are not inconsistent with expectations arising from climate change.' [Govt. of Australia (Reviewer's comment ID #: 2001-247)]	
3-926	A	69:46		Box 3.6: I have serious misgivings about this list of extreme events. Why not discuss the US dust bowl period, or the Sahel drought of the 80's. I would be more comfortable with only discussing those singular events for which a case could be made, from observations in isolation from models, for its probability of occurrence as having been plausibly increased due to warming. Otherwise, this discussion should be moved to the attribution	

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				chapter. [Isaac Held (Reviewer's comment ID #: 105-31)]	
3-927	A	69:46		Box 3.6 This whole Box is entirely unacceptable and should be removed. Have there never been extreme droughts or floods or heat waves or tropical storms prior to the post-TAR period? Whya are none of these described? Is the IPCC AR4 a special issue of "Monthly Weather Review"? [Patrick Michaels (Reviewer's comment ID #: 176-18)]	
3-928	A	70:8	70:9	I am sceptical of this statement - similar statements regularly appear in the Australian media which have no basis in fact. This should be checked with Iran. [Blair Trewin (Reviewer's comment ID #: 266-12)]	
3-929	A	70:22	70:38	If this box on specific events is retained some clarification of the Australian example is required. The rainfall data seems to be based on calendar years. This is not really the best time-base to compare the severity of Australian droughts because of their tendency to start in the SH autumn (in association with ENSO). In particular 1961 is a poor example to use as a damaging Australian drought, as the very low national annual rainfall average was primarily driven by low rainfall in central and north-eastern Australia, with near-normal rainfall in most major agricultural areas. A better event for comparison is 1982. It also appears that the 1940s were a worse dry period than the 1930s. The only drier March to January period was 1946, with 1902 just slightly wetter. We suggest this section commence with "A severe drought affected Australia from March 2002 until January 2003, associated with a moderate El Nino event (Watkins 2002; Watkins and Salinger, 2003). Droughts in 1994 and 1982 were about as dry as the 2002 drought. Droughts in the first half of the 20th century may well have been even drier. The 2002-03 drought came after several years of good rainfall (averaged across the country), rather than during an extended period of low rainfall such as occured in the 1940s. If only rainfall is considered, the 2002-03 drought alone does not provide evidence of Australian droughts becoming more extreme. However daytime and mean temperatures during March 2002 to January 2003 were the highest on record (high quality temperature records commenced 1950). If just the calendar year is considered, the mean annual maximum temperature in 2002 was 0.5oC warmer than 1994 and 1.0oC than 1982.' Maximum' should also be inserted before temperature/temperatures in lines 33 and 35, and the last section could be brought up to date with 'Severe long-term drought, stemming from at least three years of rainfall deficits, continued during 2005, especially in the eastern third of Australia, although above-normal rainfall in winter and spring 2005 brought some relief. These conditions were also accompanied by high temperatures, with Australian mean annual temperatures at record high levels in 2005 and mean maximum temperatures equalling the record set in 2002'. " REFERENCES: Watkins, A.B., and J. Salinger, (2004) Australia	

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				and the Southwest Pacific, In: State of the Climate in 2003 ed. D.H. Levinson and A.M. Waple, Bull. Amer. Met. Soc, 85(6) pp.72. [Govt. of Australia (Reviewer's comment ID #: 2001-248)]	
3-930	A	70:38	70:38	Remove "(a comparable national series is only available since 1951)" if including the above text. [Govt. of Australia (Reviewer's comment ID #: 2001-249)]	
3-931	A	71:21	71:21	Add: " Minimum temperatures were most abnormal at lower elevation whereas maximum temperatures where most abnormal at the sunniest sites (Rebetez M., 2004)" [Rebetez, M, 2004: Summer 2003 maximum and minimum daily temperatures over a 3300 m altitudinal range in the Alps. Clim. Res. 27: 45-50] [Martine Rebetez (Reviewer's comment ID #: 209-2)]	
3-932	A	71:25	71:25	Add: "Insolation was generally above normal and highly abnormal in Northern and mountainous regions (Rebetez et al., 2006)." [Rebetez M, Mayer H, Dupont O, Schindler D, Gartner K, Kroppe J, Menzel A, 2006: Heat and drought 2003 in Europe : a climate synthesis. Ann. For. Sc., Vol 63 Nr. 6, in press] [Martine Rebetez (Reviewer's comment ID #: 209-1)]	
3-933	A	71:36	71:52	Box 3.6.5. Also reference the paper by Stott et al in Nature:.Stott, P.A., D.A. Stone and M.R. Allen, 2004: Human contribution to the Eiropean heatwave of 2003. Nature, 432, 610-614 and cross refer to CH9. [Chris Folland (Reviewer's comment ID #: 71-49)]	
3-934	A	71:36	71:52	Box 3.6.5: add the following reference to the text: Cassou C., L. Terray and A. S. Phillips, 2005: Tropical Atlantic influence on European Heatwaves J.Climate, 18, 2805-2811. This particular study shows that the 2003 summer is characterized by large anomalies in the occurrence frequency of specific weather regimes (The Blocking and Atlantic Low regimes) traditionally associated with warm conditions over Western Europe. Moreover, it suggests that atmospheric teleconnections associated with latitudinal shifts of the Atlantic ITCZ may be responsible for the observed regime occurrence deviations. It also shows that there is strong intra-seasonal variability in the 2003 regime occurrence anomalies related to different remote forcing mechanisms and regions. [Govt. of France (Reviewer's comment ID #: 2010-44)]	
3-935	A	71:37	71:41	The summer 2003 heat wave has been registered also more in the south of Europe, such as southern Italy (Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2006: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381.) [Michele Brunetti (Reviewer's comment ID #: 33-2)]	
3-936	A	71:38	71:40	I suggest including the following reference: Brunetti, M., Maugeri, M., Monti, F., Nanni	

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				T., 2006: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381. This paper gives evidence that the heat wave concerned central and southern Italy too. [Teresa Nanni (Reviewer's comment ID #: 186-2)]	
3-937	A	71:41	71:44	Also some more local studies highlight that summer 2003 was the warmest one over the past two centuries. In Italy, as an example, it was the warmest summer since data are available (Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2006: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381.), with maximum temperature being 4.8 K above the 1961-1990 average and minimum temperature being 4.0 K above the 1961-1990 average. [Michele Brunetti (Reviewer's comment ID #: 33-3)]	
3-938	A	71:41	71:44	Also for this point it should be useful to include the reference Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2006: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381. This paper highlights that in Italy the summer 2003 had an anomaly of 4.0 K for Tn and + 4.8 for Tx above the 1961-1990 normals. Moreover the paper allows to put the 2003 summer in a two-secular context, highlighting its extreme values with respect to any other value of the last 200 years. [Teresa Nanni (Reviewer's comment ID #: 186-3)]	
3-939	A	71:44		Another related result has been published in Nature : Chuine I., P. Yiou, N. Viovy, B. Seguin, V. Daux et E. Le Roy Ladurie. Grape Harvest Dates and Temperature Variations in France since 1370, Nature, 289-290 (2004) [Pascale DELECLUSE (Reviewer's comment ID #: 58-43)]	
3-940	A	71:45	71:45	add: The summer 2003 was a very extreme event. The probability for such an extreme event has increased by a factor of 20 in comparison with 1969/70. (Schönwiese, C.-D., Staeger, T., Trömel S., 2004: The hot summer 2003 in Germany, Meteor. Z,13, 323-327). Climate model simulations based on scenarios of human impact project a similar further enormous increase of this heat wave probability in the next decades (Schär et al. 2004: 3-101, 26) [Govt. of Germany (Reviewer's comment ID #: 2011-6)]	
3-941	A	71:45	71:45	Add: "Most abnormal were maximum temperatures in June and August (Rebetez et al. 2006)." [Rebetez M, Mayer H, Dupont O, Schindler D, Gartner K, Kroppe J, Menzel A, 2006: Heat and drought 2003 in Europe : a climate synthesis. Ann. For. Sc., Vol 63 Nr. 6, in press] [Martine Rebetez (Reviewer's comment ID #: 209-3)]	
3-942	A	71:45		"since 1500" should read "since at least 1500", unless, that is, there is evidence that there	

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				was a very warm European summer in 1500. [Adrian Simmons (Reviewer's comment ID #: 242-72)]	
3-943	A	71:47	71:48	'Already a record month…' – I'm not sure what this sentence is meant to mean. Is it intending to state that records were already being set by June? [Blair Trewin (Reviewer's comment ID #: 266-51)]	
3-944	A	72:7	72:7	should be 'have', not 'has'. [Blair Trewin (Reviewer's comment ID #: 266-84)]	
3-945	A	72:10	72:10	Delete" … and the most damaging storm on record (Katrina)." While the claim is accurate, it is misleading. Much of the damage attributed to Katrina was the resut of the failure of the New Orleans flood control system. It has now been documented that this failure had been predicted and could have been avoided had action been taken to correct the system's deficiencies. The discussion of this topic should be included in WG II's report, but WG I should limit itself to a discussion of the physical characteristics of storm, as is done earlier in this sentence. [Lenny Bernstein (Reviewer's comment ID #: 20-55)]	
3-946	A	72:10	72:10	Katrina was the most damaging storm on record, but this statement is incomplete. Most of the damage was the result of the failure of New Orlean's flood control system, a failure which could have been avoided had the system's documented shortcomings had been repaired. The discussion of Katrina's impacts and how they might have been avoided belongs in WG II's report. WG I should limit itself to a discussion of the storm's characteristics, and the degree to which they are related to recent changes in climate. [Jeff Kueter (Reviewer's comment ID #: 137-52)]	
3-947	A	72:10	72:11	Vince achieved category 1 (Saffir-Simpson) not far from Madeira during a few hours. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-9)]	
3-948	A	72:10	72:11	Vince achieved category 1 (Saffir-Simpson) not far from Madeira during a few hours. [Govt. of Spain (Reviewer's comment ID #: 2019-69)]	
3-949	A	72:10		Katrina was the most damaging storm on record, but this statement is incomplete. Most of the damage was the result of the failure of New Orlean's flood control system, a failure which could have been avoided had the system's documented shortcomings been repaired. The discussion of Katrina's impacts and how they might have been avoided belongs in WGII's report. WGI should limit itself to a discussion of the storm's characteristics, and the degree to which they are related to recent changes in climate. [Govt. of United States of America (Reviewer's comment ID #: 2023-258)]	
3-950	A	72:18	72:22	The last sentence likely needs modification in the light of my comments above at p 66. [Chris Folland (Reviewer's comment ID #: 71-50)]	
3-951	A	72:18	72:22	Could note that the vertical wind shear was not particularly favorable.	

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				[David Rind (Reviewer's comment ID #: 214-29)]	
3-952	A	72:18	:22	Could note that the vertical wind shear was not particularly favorable. [Govt. of United States of America (Reviewer's comment ID #: 2023-259)]	
3-953	A	72:33	72:33	"72%" should be "73%" as comment 2. [Lisa Alexander (Reviewer's comment ID #: 1-9)]	
3-954	A	72:34	72:34	"76%" should be "74%" as comment 1. [Lisa Alexander (Reviewer's comment ID #: 1-10)]	
3-955	A	72:52	72:53	Delete" … and the most damaging storm on record (Katrina)." While the claim is accurate, it is misleading. Much of the damage attributed to Katrina was the resut of the failure of the New Orleans flood control system. It has now been documented that this failure had been predicted and could have been avoided had action been taken to correct the system's deficiencies. The discussion of this topic should be included in WG II's report, but WG I should limit itself to a discussion of the physical characteristics of storm, as is done earlier in this sentence. [Lenny Bernstein (Reviewer's comment ID #: 20-56)]	
3-956	A	72:52	72:52	Katrina was the most damaging storm on record, but this statement is incomplete. Most of the damage was the result of the failure of New Orlean's flood control system, a failure which could have been avoided had the system's documented shortcomings had been repaired. The discussion of Katrina's impacts and how they might have been avoided belongs in WG II's report. WG I should limit itself to a discussion of the storm's characteristics, and the degree to which they are related to recent changes in climate. [Jeff Kueter (Reviewer's comment ID #: 137-53)]	
3-957	A	73:0		Table 3.7. Tropical cyclons, Definition, where it says '58 to 69 ms-1' it should say '59 to 69 ms-1' [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-10)]	
3-958	A	73:3	73:3	'may be' should be two words. [Blair Trewin (Reviewer's comment ID #: 266-85)]	
3-959	A	73:17	74:1	With the previous comment in mind, in Table 3.8, please replace the entry in column "Change" for "Small-scale severe weather phenomena" by a stronger and more indicative statement. For instance, instead of the present text "Insufficient studies for assessment", please insert "Insufficient data and studies for assessment; strong need for more and homogeneous databases worldwide." [Nikolai Dotzek (Reviewer's comment ID #: 59-5)]	
3-960	A	73:17	74:1	Also, in Table 3.8, please add the entry "3.8.4.2" in column "Section" for "Small-scale severe weather phenomena". [Nikolai Dotzek (Reviewer's comment ID #: 59-6)]	

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3-961	A	73:17		Table 3.8. The flood frequency results of Milly et al (2002) do not fit into any of the existing 'phenomena' because they are not necessarily a direct manifestation of heavy precipitation _events_ but rather depend on longer-term (~monthly to seasonal) precipitation amounts and also on other climate variables, such as temperature (in the case of the snowmelt-related flood events). In my opinion, on the basis of Milly et al (2002), an increase in the global rate of great floods (100-year floods on large river basins) did more likely than not occur in late 20th century (16 out of 21 events in second half of the record), and the trend more likely than not is due to human influence (through the climate system), which model simulations indicate can readily explain the observations. Such a global trend is also consistent with theory and changes in mean runoff in high latitudes and elsewhere. (Change in mean runoff need not be positive everywhere in order for global flood frequency to increase. The global frequency is mathematically more sensitive to regions of increased rate than regions of decreased rate.) [P.C.D. Milly (Reviewer's comment ID #: 179-13)]	
3-962	A	73:47	73:48	"Nonetheless, clear evidence…" This doesn't follow from the previous discussions, nor does it follow from a close examination of data quality issues. [Patrick Michaels (Reviewer's comment ID #: 176-19)]	
3-963	A	74:0	74:	In Table 3.8 "Tropical Cyclones" entry. Confidence given here is "likely" for positive trends in lifetimes and intensity since 1970s. In contrast, intensity increase since 1970 is termed "more likely than not" in tech summary. Also, "more confidence in frequency [no change] and intensity than track". Do you mean "duration" instead of "track"? This is the first mention of track change. This brings up a related question not addressed at all in the report: what confidence level to place on the reported duration trends? [Thomas Knutson (Reviewer's comment ID #: 132-14)]	
3-964	A	74:1	74:1	in Table 3.8, it asserts that drought has increased. This is not consistent with the evidence as discussed in the above comments. [Michael Roderick (Reviewer's comment ID #: 218-13)]	
3-965	A	74:3	76:32	This section (good that several observational chapters are integrated here) would improve from a diagrammatic summary of the evidence for warming, much as done in the TAR, Figs 2.39a and 2.39b. [Chris Folland (Reviewer's comment ID #: 71-51)]	
3-966	A	74:3		Seection 3.9 : The list of bullets in the synthesis section strikes me as redundant with the executive summary [Isaac Held (Reviewer's comment ID #: 105-32)]	
3-967	A	74:5	78:5	While a summary of consistency across observations is an important part of the overall assessment - its placement here precedes Chapters 4 and 5 - the results upon which it depends. This could be the subject of a special IPCC Technical Summary - or it could be	

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				an appendix to AR4 WG1 Report. Where it currently sits it adds to the issue of spreading information on some climate change aspects across chapters - e.g. sea level rise p3-77 line 24-32 is not the subject of this chapter. Also, the current order of dot points does not flow well. [Govt. of Australia (Reviewer's comment ID #: 2001-250)]	
3-968	A	74:6	74:6	is it really confidence in the "realism of apparent observed changes" which you're looking for here? Surely such confidence comes from the quality control and uncertainty estimates applied to datasets before they merit reference in this chapter. Would it be true to say you're looking for enhanced confidence in our understanding of the climate system and how it will be affected by increases in greenhouse gases? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-19)]	
3-969	A	75:1	75:1	Insert after "happened" "This behaviour is not confirmed by radiosondes and satellites in the lower troposphere, which have found zero temperature rises for extended periods (1958-2002 for rsdiosondes, and 1978 to 1998 for satellites. The rise in land-and -sea surface temperatures are therefore largely attributable to proximity of most measuring equipment to local human activities" [VINCENT GRAY (Reviewer's comment ID #: 88-444)]	
3-970	A	75:5	75:5	Insert after "climate" "near cities" [VINCENT GRAY (Reviewer's comment ID #: 88-445)]	
3-971	A	75:10	75:13	This section is a near-duplicate of 3-76, 18-21. These two sections are near-duplicates and should be merged. [Blair Trewin (Reviewer's comment ID #: 266-52)]	
3-972	A	75:14	75:14	Insert after "increses" "(evident mainly to the surface record)" [VINCENT GRAY (Reviewer's comment ID #: 88-446)]	
3-973	A	75:14	75:14	Replace "are consistent" with "can be linked to" [VINCENT GRAY (Reviewer's comment ID #: 88-447)]	
3-974	A	75:15	75:15	Insert after "century "but" and continue the sentence [VINCENT GRAY (Reviewer's comment ID #: 88-448)]	
3-975	A	75:30	75:30	Would prefer '…Subarctic, with permafrost warming also observed…'. [Blair Trewin (Reviewer's comment ID #: 266-53)]	
3-976	A	75:52	75:54	Most of the evidence suggests the opposite—increased heating at the surface relative to the troposphere. There is some suggestion that the trends in the troposphere may be underestimated (Sherwood et al.) but the corrections have not been made and thus the ultimate outcome is unknown. [Patrick Michaels (Reviewer's comment ID #: 176-20)]	
3-977	A	76:18	76:19	This is exact repetition of P75, L10-11 and one of these should be removed.	

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				[Lisa Alexander (Reviewer's comment ID #: 1-11)]	
3-978	A	76:22	76:27	The evidence for increasing radiation since 1990 is conflicting, see p. 39, lines 18-22 and the above-noted comments. However, there is no conflict with regards to pan evaporation which has, in all published reports, continued to decrease "on average". [Michael Roderick (Reviewer's comment ID #: 218-14)]	
3-979	A	76:36	76:43	See the above comments about the Thornthwaite based calculations showing increasing droughts. This would reverse if pan evaporation measurements or Penman based calculations were used to define potential evaportranspiation in the PDSI calculations. [Michael Roderick (Reviewer's comment ID #: 218-15)]	
3-980	A	77:24	77:24	Replace "likely rose 18 ±3 cm" with 1.8±1.0mm per year" Chapter 5 page 3 line 42 gives 1.8±0.5 but I believe that this has only one standard deviation, so it must be doubled to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-449)]	
3-981	A	77:24	77:24	Insert after "the" "second half of" see Chapter 5 page 2 etc. [VINCENT GRAY (Reviewer's comment ID #: 88-450)]	
3-982	A	77:24	77:24	Replace "3.1±0.4" with "to 3.1±1.6" Again refer to Chapter 5 and double the uncertainty figure [VINCENT GRAY (Reviewer's comment ID #: 88-451)]	
3-983	A	77:35	77:35	add after SSTs "and air temperatures over the oceans". [Chris Folland (Reviewer's comment ID #: 71-52)]	
3-984	A	77:35	77:35	Insert after "1970's" But these increases are not evident in the lower troposphere (1958-2002 for radiosondes, 1978 to 1998 for satellies) so the land based figures must be contaminated by proximity to human activity." [VINCENT GRAY (Reviewer's comment ID #: 88-452)]	
3-985	A	77:41		It is stated (referring implicitly to the SAM) that there has been "cooling over the interior of Antarctica". See comment #64. Adding to that comment, just how clear is that there has been surface cooling over Antarctica? FIGURE 3.2.10 shows very few grid boxes over Antarctica at which there are sufficient data to produce reliable trends, and these seem to show more warming than cooling. Also, there is reference to warming, rather than cooling, in Chapter 4, page 4-25, lines 24 to 26. Later in the same paragraph on page 4-25, it is stated that overall trend analyses started between 1966 and 1982 show cooling, but longer-period trends show warming. Is any of this significant given the grey areas in FIGURE 3.2.10 (which refers to trends from 1979)? Of course, a SAM trend may have masked what would otherwise have been a much larger warming over the interior of Antarctica. A more circumspect conclusion might be appropriate here. [Adrian Simmons (Reviewer's comment ID #: 242-73)]	

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3-986	A	78:2	78:2	Delete "strongly" [VINCENT GRAY (Reviewer's comment ID #: 88-453)]	
3-987	A	79:0	109:	References for above text. John R. Christy. 2002: When Was The Hottest Summer? A State Climatologist Struggles for an Answer. Bulletin of the American Meteorological Society: Vol. 83, No. 5, pp. 723–734. John R. Christy, William B. Norris, Kelly Redmond and Kevin P. Gallo. 2006: Methodology and Results of Calculating Central California Surface Temperature Trends: Evidence of Human-Induced Climate Change? Journal of Climate (in press). Christopher A. Davey and Roger A. Pielke Sr.. 2005: Microclimate Exposures of Surface-Based Weather Stations: Implications For The Assessment of Long-Term Temperature Trends. Bulletin of the American Meteorological Society: Vol. 86, No. 4, pp. 497–504. Kevin P. Gallo. 2005: Evaluation of Temperature Differences for Paired Stations of the U.S. Climate Reference Network. Journal of Climate: Vol. 18, No. 10, pp. 1629–1636. Roger A. Pielke Sr. and Toshihisa Matsui: Should light wind and windy nights have the same temperature trends at individual levels even if the boundary layer averaged heat content change is the same? Geophysical Research Letters (in press). [Kevin Gallo (Reviewer's comment ID #: 79-2)]	
3-988	A	79:1	109:20	There needs to be consistency with other chapters over the use of et al in the reference list. Some other chapters have written their references in full. All chapters should adopt a common policy. [Chris Folland (Reviewer's comment ID #: 71-53)]	
3-989	A	79:21	79:22	Correct reference should be: Alexander, L.V., Tett, S.F.B and Jonsson, T, 2005 "Recent observed changes in severe storms over the United Kingdom and Iceland". Geophys. Res. Lett. 32, L13704, Doi:10.1029/2005GL022371 [Lisa Alexander (Reviewer's comment ID #: 1-16)]	
3-990	A	79:23	79:24	Correct reference should be: Alexander, L.V., et al., 2006: Global observed changes in daily climate extremes of temperature and precipitation. J. Geophys. Res., 111, D05109, doi:10.1029/2005JD006290 [Lisa Alexander (Reviewer's comment ID #: 1-17)]	
3-991	A	80:23	80:23	The initials from Baldwin are M.P. not only M. [ILEANA MARES (Reviewer's comment ID #: 161-11)]	
3-992	A	80:42	80:42	Please add for Barros the initials [ILEANA MARES (Reviewer's comment ID #: 161-12)]	
3-993	A	82:41		Insert before Cayan: Castro-Díez, Y.; Pozo-Vázquez, D.; Rodrigo, F.S. and Esteban-Parra, M.J., 2002. NAO and winter temperature variability in southern Europe. Geophys. Res. Lett., 29 (8), 1-4, doi: 10.1029/2001GL014042.	

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				[Govt. of Spain (Reviewer's comment ID #: 2019-31)]	
3-994	A	83:3		Papers cited for addition to the chapter 3 references - Insert the following reference: Chang, F.-L., and Z, Li, 2005a: A new method for detection of cirrus overlapping water clouds and determination of their optical properties, J. Atmos. Sci., 62, 3993–4009, 2005a. [Zhanqing Li (Reviewer's comment ID #: 147-10)]	
3-995	A	83:3		Papers cited for addition to the chapter 3 references - Insert the following reference: Chang, F.-L., and Z. Li, 2005b: A near-global climatology of single-layer and overlapped clouds and their optical properties retrieved from Terra/MODIS data using a new algorithm, J. Climate, 18, 4752-4771. [Zhanqing Li (Reviewer's comment ID #: 147-11)]	
3-996	A	83:5	83:5	I suppose that Changnon D. is same person with Changnon S.A. from lines 4 and 7. [ILEANA MARES (Reviewer's comment ID #: 161-13)]	
3-997	A	84:25	84:27	In my opinion is a little strange to appear b) before a) and I have also seen this situation for other references. My suggestion is to change these cases in References and of course in text. [ILEANA MARES (Reviewer's comment ID #: 161-14)]	
3-998	A	87:39	87:43	According with the rule of References order, line 39 must be moved after line 43 [ILEANA MARES (Reviewer's comment ID #: 161-15)]	
3-999	A	88:1	88:6	Is Gong D. same with Gong D.Y. and S. Wang with S.W. Wang ? [ILEANA MARES (Reviewer's comment ID #: 161-16)]	
3-1000	A	88:19	88:19	Insert " Gray, V.R. 2000: The Cause of Global Warming". Energy and Environment Vol 11, pages 613-628" [VINCENT GRAY (Reviewer's comment ID #: 88-455)]	
3-1001	A	90:39	90:45	Please move Inoue and Kimura before IPCC, 1999 [ILEANA MARES (Reviewer's comment ID #: 161-17)]	
3-1002	A	91:45	91:48	If Karoly D.J. is same with Karoly D. is not respected the rule of references orders [ILEANA MARES (Reviewer's comment ID #: 161-18)]	
3-1003	A	93:26	93:26	In association with comment # 2, insert a new reference "Lander, M., and C. P. Guard, 2006: The urgent need for a re-analysis of western North Pacific tropical cyclones. 27th Conference on Hurricanes and Tropical Meteorology, American Meteorological Society, Monterey, California, April 2006.". The reference can be assessed online at http://ams.confex.com/ams/27Hurricanes/techprogram/paper_107845.htm. [Chiu-Ying LAM (Reviewer's comment ID #: 139-4)]	
3-1004	A	95:1	95:7	Both Mantua and Marengo have not same initials. [ILEANA MARES (Reviewer's comment ID #: 161-19)]	

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3-1005	A	95:36	95:36	Insert " McKitrick, R. and P. J. Michaels, 2004 : A test of corrections for extraneous signals in gridded surface temperature data. Climate Research Vol 26 pages 150-173" [VINCENT GRAY (Reviewer's comment ID #: 88-454)]	
3-1006	A	98:52		Insert before Power: Pozo Vázquez, D.; Esteban-Parra, M.J.; Rodrigo, F.S. and Castro-Díez, Y., 2000. An analysis of the variability of the North Atlantic Oscillation in the time and the frequency domains. Int. J. Climatol., 20, 1675-1692. [Govt. of Spain (Reviewer's comment ID #: 2019-32)]	
3-1007	A	98:52		Insert the following refernece before Power: Pozo-Vázquez, D.; Esteban-Parra, M.J.; Rodrigo, F.S. and Castro-Díez, Y., 2001. The association between ENSO and winter atmospheric circulation and temperature in the North Atlantic region. J. Climate, 16, 3408-3420. [Govt. of Spain (Reviewer's comment ID #: 2019-33)]	
3-1008	A	98:52		Insert the following refernece before Power: Pozo-Vázquez, D.; Tovar-Pescador, J.; Gámiz-Fortis, S.R; Esteban-Parra, M.J. and Castro-Díez, Y., 2004. NAO and Solar radiation variability in the European North Atlantic region. Geophys. Res. Lett., 31, 5, L05201, doi: 10.1029/2003GL018502. [Govt. of Spain (Reviewer's comment ID #: 2019-34)]	
3-1009	A	98:52		Insert the following refernece before Power: Pozo-Vázquez, D.; Gámiz-Fortis, S.R.; Tovar-Pescador, J.; Esteban-Parra, M.J. and Castro-Díez, Y., 2005. North Atlantic winter SLP anomalies based on the autumn ENSO state. J. Climate, 18, 97-103. [Govt. of Spain (Reviewer's comment ID #: 2019-35)]	
3-1010	A	100:13	100:15	Robertson has different initials for 2001a and 2001b [ILEANA MARES (Reviewer's comment ID #: 161-20)]	
3-1011	A	102:18	102:19	Insert between line 18 and 19: Shabbar, A., and B. Bonsal, 2004: Associations between low frequency variability modes and winter temperature extremes in Canada. Atmos.-Ocean. 42, 127-140. See Comment# 2 above. [Xiaolan L. WANG (Reviewer's comment ID #: 282-19)]	
3-1012	A	102:18	102:19	Insert between line 18 and 19: Shabbar, A., and B. Bonsal, 2003: An assessment of changes in winter cold and warm spells over Canada. Natural Hazards, 29, 173-188. See Comment#18 above. [Xiaolan L. WANG (Reviewer's comment ID #: 282-20)]	
3-1013	A	102:44	102:44	The last four digits of the DOI must be 5306 instead of 6306. [Andreas Sterl (Reviewer's comment ID #: 253-2)]	
3-1014	A	105:11		Insert the following reference before Trigo: Trigo, R.M.; Pozo-Vázquez, D.; Osborn, T.J.; Castro-Díez, Y.; Gámiz-Fortis, S.R. and Esteban-Parra, M.J., 2004. North Atlantic Oscillation influence on precipitation, river flow and water resources in the Iberian	

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				Peninsula. Int. J. Climatol., 24, 925-944, doi: 10.1002/joc.1048. [Govt. of Spain (Reviewer's comment ID #: 2019-36)]	
3-1015	A	105:56	105:57	This reference, Vinnikov et al. [2004], is not used in the text of the chapter. Rather than remove it, text needs to be added referring to it (see following comments). - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-10)]	
3-1016	A	106:26	106:27	This paper has now been published. The dull details are: Wang, B., and Q. Ding, 2006: Changes in global monsoon precipitation over the past 56 years. Geophys. Res. Lett., 33, L06711, doi:10.1029/2005GL025347. [Ian Simmonds (Reviewer's comment ID #: 241-13)]	
3-1017	A	106:30	106:32	The initials are not same for Wang 2002a and 2002b [ILEANA MARES (Reviewer's comment ID #: 161-21)]	
3-1018	A	106:42	106:43	Add the following reference between line 42 and 43: "Wang, X. L., and V. R. Swail, 2006: Historical and possible future changes of wave heights in northern hemisphere oceans. In: Atmosphere-Ocean Interactions - Vol. 2 [Perrie, W. (ed.)]. Advances in Fluid Mechanics Series Vol 39. Wessex Institute of Technology Press, Southampton, UK. ISBN: 1-85312-929-1, 240pp." See Comment #9 above. [Xiaolan L. WANG (Reviewer's comment ID #: 282-10)]	
3-1019	A	106:45	106:45	Replace "2006" with "2006a" because of the suggested citation to "Wang et al., 2006b" (see Comments #1-5 above) [Xiaolan L. WANG (Reviewer's comment ID #: 282-16)]	
3-1020	A	106:47	106:48	Add the following reference between line 47 and 48: "Wang, X. L., H. Wan, and V. R. Swail, 2006b: Observed Changes in Cyclone Activity in Canada and Their Relationships to Major Circulation Regimes. J. Climate, 19, 896-915." See Comments #1-4 above). [Xiaolan L. WANG (Reviewer's comment ID #: 282-5)]	
3-1021	A	107:41	107:46	Please insert Wiedenmann (line 46) before Wielicki (line 41) [ILEANA MARES (Reviewer's comment ID #: 161-22)]	
3-1022	A	108:43	108:44	The paper should be "Yu, R.,B. Wang, and T. Zhou, 2004b……", add another paper as "Yu Rucong, Bin Wang, and Tianjun Zhou, 2004a, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270" [Govt. of China (Reviewer's comment ID #: 2006-41)]	
3-1023	A	109:1	109:1	Please change the initial Y with X for Zhang [ILEANA MARES (Reviewer's comment ID #: 161-23)]	
3-1024	A	109:9	109:9	Insert "Zhao, Z., Y.Ding,, Y. Luo., and S. Wang., 2005: "Recent studies on attributions of climate change in China. "Acta Meteorologica Sinica" Vol 19, Pages 389-398	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-456)]	
3-1025	A	110:1	111:10	This whol;e "Question" is garbled, and a feeble attempt to summarise the previous text. It is completely unnecessary and should be deleted [VINCENT GRAY (Reviewer's comment ID #: 88-493)]	
3-1026	A	110:3	110:3	Insert after "risen", "but the absence of any temperature rise in the lower troposphere (1958 to 2002 for radiosondes and 1978 to 1998 for satellites) shows that land-based measurements are upwardly biased by proximity of the measurement equipment to human activities" [VINCENT GRAY (Reviewer's comment ID #: 88-457)]	
3-1027	A	110:3	110:3	Replace "but with":by "There are" [VINCENT GRAY (Reviewer's comment ID #: 88-458)]	
3-1028	A	110:3		I suggest the first sentence needs to include a statement clarifying the period for which "Generally temperatures have risen" and it could be worth emphasizing the facts listed in this sentence come from observations, ie: OBSERVATIONS OVER THE PAST 150 YEARS SHOW THAT generally temperatures have risen ... [David Wratt & David Fahey (Reviewer's comment ID #: 67-101)]	
3-1029	A	110:4	110:4	Replace "For the global average warming" by "Surface temperature changes" [VINCENT GRAY (Reviewer's comment ID #: 88-459)]	
3-1030	A	110:4	110:4	Replace "has" by "have" [VINCENT GRAY (Reviewer's comment ID #: 88-460)]	
3-1031	A	110:4	110:4	Replace "in the last century" by "since 1865", [VINCENT GRAY (Reviewer's comment ID #: 88-461)]	
3-1032	A	110:4	110:4	Replace "two" by "four" [VINCENT GRAY (Reviewer's comment ID #: 88-462)]	
3-1033	A	110:4	110:4	Insert after "phases" "from 1855 to 1910 a fall of 0.02°C" [VINCENT GRAY (Reviewer's comment ID #: 88-463)]	
3-1034	A	110:4	110:4	Replace "the 1910s-1940s" with "1910 to 1942" [VINCENT GRAY (Reviewer's comment ID #: 88-464)]	
3-1035	A	110:4	110:5	FAQ 3.1 states an increase in global warming from 1910s - 1940s of 0.35 deg C. Is this consistent with the SPM (page 6, line 41), which states an increase of 0.14 deg C per decade for 1910 - 1945. When I convert the value in the SPM into total temperature increase, I get a value of 0.49 deg C, which is quite larger than the value given in FAQ 3.1. Is the time period different for these two values (FAQ vs. SPM)? [Melinda Marquis (Reviewer's comment ID #: 162-47)]	
3-1036	A	110:4	110:5	FAQ 3.1 states an increase in global warming 0.55 deg C from the 1970s to the present. Is this consistent with the SPM (page 6, line 41), which states an increase of 0.17 deg C	

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				per decade for 1979 - 2005. When I convert the value in the SPM into total temperature increase, I get a value of 0.46 deg C, which is smaller than the value given in FAQ 3.1. Is the time period different for these two values (FAQ vs. SPM)? [Melinda Marquis (Reviewer's comment ID #: 162-48)]	
3-1037	A	110:5	110:5	Replace "(0.35°C)" with "an increase of 0.4°C" [VINCENT GRAY (Reviewer's comment ID #: 88-465)]	
3-1038	A	110:5	110:5	Insert before "and" "a fall in temperature of 0.5°C between 1942 and 1978" [VINCENT GRAY (Reviewer's comment ID #: 88-466)]	
3-1039	A	110:5	110:5	Replace "more strongly" with"an increase" [VINCENT GRAY (Reviewer's comment ID #: 88-467)]	
3-1040	A	110:5	110:5	Replace "(0.55°C)" with "0.45°C" [VINCENT GRAY (Reviewer's comment ID #: 88-468)]	
3-1041	A	110:5	110:5	Replace "but with 0.1°C cooling between" with. "None of these sequences could have been influenced by increases in greenhouse gases; the first two because of the low amount, the second because a cooling is unliely to be caused by a greenhouse gas increase, and the fiurth because the observed warming at the surface cannot be detected for most of the sequence in the lower troposphere, where greenhouse warming is supposed to happen." [VINCENT GRAY (Reviewer's comment ID #: 88-469)]	
3-1042	A	110:6	110:9	Replace from "slightly greater" on line 6 to "resolved" on line 9 with "zero temperature change from 1958 to the year 2002, with a slight rise since then. Satellites found no temperature change between 1978 and 1998, but a rise since then started by the unusually strong El Niño event of 1999"" [VINCENT GRAY (Reviewer's comment ID #: 88-470)]	
3-1043	A	110:6		Suggest for clarity to replace 'estimates' with 'measurements' [David Wratt & David Fahey (Reviewer's comment ID #: 67-20)]	
3-1044	A	110:7	110:7	"… warming evolves differently." What does this mean? [Melinda Marquis (Reviewer's comment ID #: 162-65)]	
3-1045	A	110:8		Suggest for clarity to rewrite as '…balloons differ somewhat, ranging from less to …' [David Wratt & David Fahey (Reviewer's comment ID #: 67-21)]	
3-1046	A	110:9	110:9	"… uncertainties in the observing system are not yet fully understood." May not need this in an FAQ. [Melinda Marquis (Reviewer's comment ID #: 162-66)]	
3-1047	A	110:12	110:12	Insert after "century", "But, as has been explained, this cannot be attribuited to a rise in greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-471)]	

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3-1048	A	110:16	110:16	Replace "over the 20th Century" with "from 1855" [VINCENT GRAY (Reviewer's comment ID #: 88-472)]	
3-1049	A	110:16	110:16	Insert after "was" " a fall of 0.02°C between 1855 and 1910, [VINCENT GRAY (Reviewer's comment ID #: 88-473)]	
3-1050	A	110:16	110:16	I don't like the use of 'assumption' here. Suggest 'A linear trend over the 20th century is a very poor approximation of the temperature changes which have occurred.' [Blair Trewin (Reviewer's comment ID #: 266-54)]	
3-1051	A	110:16		Suggest for clarity to rewrite as 'A linear temperature trend …' A further suggestion is to delete this sentence to reduce complexity. [David Wratt & David Fahey (Reviewer's comment ID #: 67-22)]	
3-1052	A	110:17	110:17	Replace "(0.35°C)" by "(0.42°C)" [VINCENT GRAY (Reviewer's comment ID #: 88-474)]	
3-1053	A	110:17	110:17	Replace "the 1910s to the 1940s" wu\ith "1910 to 1942" [VINCENT GRAY (Reviewer's comment ID #: 88-475)]	
3-1054	A	110:17	110:17	Delete "slight" [VINCENT GRAY (Reviewer's comment ID #: 88-476)]	
3-1055	A	110:17	110:17	Replce "(0.1°C)" with "(0.5°C)" [VINCENT GRAY (Reviewer's comment ID #: 88-477)]	
3-1056	A	110:17	110:17	Replace "then" with "1942" [VINCENT GRAY (Reviewer's comment ID #: 88-478)]	
3-1057	A	110:17	110:18	Replace "the 1970s" with "1978" [VINCENT GRAY (Reviewer's comment ID #: 88-479)]	
3-1058	A	110:18	110:18	Delete "rapid" [VINCENT GRAY (Reviewer's comment ID #: 88-480)]	
3-1059	A	110:18	110:18	Replace "(0./55°C)" with "(0.45°C)" [VINCENT GRAY (Reviewer's comment ID #: 88-481)]	
3-1060	A	110:18	110:18	Insert after "2005" "None of these sequences could have been influenced by increases in greenhouse gases; the first two because of the low amount, the second because a cooling is unliely to be caused by a greenhouse gas increase, and the fiurth because the observed warming at the surface cannot be detected for most of the sequence in the lower troposphere, where greenhouse warming is supposed to happen." [VINCENT GRAY (Reviewer's comment ID #: 88-482)]	
3-1061	A	110:18	110:20	The statement that 1998 was unambiguously the warmest year is not consistent with other material in the chapter . Please make this consistent throughout. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-17)]	
3-1062	A	110:18	110:20	The warmest year of the series was recorded in 1998 and 10 of fthe 11 warmest years	

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				have occurred in the last eleven complete years (1995 - 2005). However, the SPM (page6, lines 34-35) states, "2005 and 1998 were the warmest two years on record. Five of the six warmest years have occurred in the last five years (2001-2005)." And Chapter 3 (Ex. Sum., Page 3, lines 15-18) states, "2005 is one of the warmest two years on record. The warmest years in the instrumental record are 1998 and 2005, with 1998 ranking first in CRU/UKMO estimate, but with 2005 slightly ahead in the NCDC and GISS estimates. 2002 to 2004 are the 3rd, 4th and 5th warmest years in the series since 1850 and 10 of the last 11 years (1995 to 2005) – the exception being 1996 – are among the 11 warmest years." The body of Ch. 3 (Page 13, Section 3.2.2.4, lines 10-14) present this information in a still slightly different manner. This information should be presented consistently in all places. [WG1 TSU (Reviewer's comment ID #: 285-4)]	
3-1063	A	110:19	110:18	Insert after "1998" "because of the El Niño event of that year, also evident in the lower troposphere records" [VINCENT GRAY (Reviewer's comment ID #: 88-483)]	
3-1064	A	110:19	110:19	Delete "and" [VINCENT GRAY (Reviewer's comment ID #: 88-484)]	
3-1065	A	110:19		"The warmest year of the series was recorded in 1998 …". See comment #60. [Adrian Simmons (Reviewer's comment ID #: 242-74)]	
3-1066	A	110:20	110:20	Insert after "2005)' "but this was not detected in the lower troposphere, so it could not be influenced by greenhouse gas increases" [VINCENT GRAY (Reviewer's comment ID #: 88-485)]	
3-1067	A	110:20	110:20	Instead of " … in the most recent phase," can you please provide the specific years, e.g., … since xxxx (year). [Melinda Marquis (Reviewer's comment ID #: 162-67)]	
3-1068	A	110:24	110:24	Add at end "and the Antarctic continent since measurements began." [VINCENT GRAY (Reviewer's comment ID #: 88-486)]	
3-1069	A	110:24	110:24	A few areas have cooled … Greenland. Haven't parts of Antarctica also cooled? [Melinda Marquis (Reviewer's comment ID #: 162-68)]	
3-1070	A	110:26	110:26	The 'significant' is presumably defined in statistical terms. Suggest rewording 'the most significant warming, in statistical terms, has occurred…'. [Blair Trewin (Reviewer's comment ID #: 266-55)]	
3-1071	A	110:27	110:29	This statement is not supported by anything in the main body of Chapter 3. It appears to refer to the findings in: Horton, E.B., Folland, C.K. and Parker, D.E. 2001. The changing incidence of extremes in worldwide and central England temperatures to the end of the twentieth century. Clim. Change, 50, 267-295.	

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				[Govt. of Australia (Reviewer's comment ID #: 2001-251)]	
3-1072	A	110:27	110:29	This sentence tells readers something about behaviour up to 1990, but leaves them in suspense over what has happened since then. State whether this behaviour changed (or not) after 1990 ? [David Wratt & David Fahey (Reviewer's comment ID #: 67-102)]	
3-1073	A	110:27	110:29	I am not sure where this statement comes from, if its a global result. Check or omit. The statement and its reference should appear in the main text. [Chris Folland (Reviewer's comment ID #: 71-54)]	
3-1074	A	110:27	110:27	"Up to about 1990, ..." Why stop at 1990? [Melinda Marquis (Reviewer's comment ID #: 162-69)]	
3-1075	A	110:32	110:34	Replace "negligible" to "standards" on line 34 by "significant, and largely explain the difference between the surface record and the lower troposphere, where warming has not been observed between 1958 and 1998" [VINCENT GRAY (Reviewer's comment ID #: 88-487)]	
3-1076	A	110:33	110:33	insert comma after 'accounted for'. [Blair Trewin (Reviewer's comment ID #: 266-86)]	
3-1077	A	110:34		Suggestion: … Increasing evidence suggests that LOCAL urban effects extend to … (Reason: Clarification - since the preceding sentence talks about both global and local effects). [David Wratt & David Fahey (Reviewer's comment ID #: 67-103)]	
3-1078	A	110:35		Suggest for completeness to change to '… cloud amounts, ….' [David Wratt & David Fahey (Reviewer's comment ID #: 67-23)]	
3-1079	A	110:36		Suggest changing to 'pollution during weekends.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-24)]	
3-1080	A	110:45	110:46	Let the end of this paragraph read as follows: ... not all regions. These trends are consistent with LLGHGs causing global warming. The daytime temperatures are influenced by both the solar radiation which is "on" only during the day and the long-wavelength back radiation due to the LLGHGs which is "on" all the time. The nightime temperatures are influenced only by the back radiation due to the LLGHGs. Thus one expects the day temperatures to be fractionally influenced less by an increase in the the greenhouse effect than the night temperatures in either the winter or the summer. Since 1950, the length of the frost free season has increased in most mid to high latitude regions of both hemispheres. In the NH this is mostly manifest as as earlier start to spring rather than later frosts in the autumn. This is also consistent with LLGHGs causing global warming. The carbon dioxide concentration in the atmosphere has seasonal variations being highest in the spring and lowest in the late autumn after the growing season. The	

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				amplitueds of these variations are increasing with time. The leads one to expect that the last frost in the spring occurs earlier, but that the first frost in the autumn is not shifted too to later times by as much as the shift in the last frost in spring. [Wilmer Anderson (Reviewer's comment ID #: 5-57)]	
3-1081	A	110:50	110:51	Delete from "and the data " on line 50 to "practices" on line 51. This is grossly exaggerated [VINCENT GRAY (Reviewer's comment ID #: 88-488)]	
3-1082	A	110:51		Suggest for clarity to rewrite as 'from satellite instruments (MSU)…' and later in the text. [David Wratt & David Fahey (Reviewer's comment ID #: 67-25)]	
3-1083	A	110:52	110:54	Delete from "but" on line 52 to "Although" on line 54. Sagain, grossly exaggerated [VINCENT GRAY (Reviewer's comment ID #: 88-489)]	
3-1084	A	110:55	110:55	Delete from "they" to trends" This makes no sense [VINCENT GRAY (Reviewer's comment ID #: 88-490)]	
3-1085	A	110:57	111:1	Replace from "somewhat" on 110 line 57 to "system" on page 111 line 1 with "show no temperture change from 1958 to 2002 followed by a slight rise by radiosondes, and no temperature change from 1978 to 1999 followed by a large El Niño peak in 1999 and a warm period from 2001" [VINCENT GRAY (Reviewer's comment ID #: 88-491)]	
3-1086	A	111:1	111:3	The sentence beginnning "Balance of evidence…" can be interpreted to be conflicting with the statement in Executive Summary (page 4, line 6 and 7) "It is likely that …" [Govt. of Finland (Reviewer's comment ID #: 2009-52)]	
3-1087	A	111:1	111:3	The statement that "The balance of evidence suggests that the tropical lower atmosphere has warmed slightly less than the surface since 1979" may not be accurate by considering the recognized problems in the radiosonde data and UAH MSU data. [Qiang Fu (Reviewer's comment ID #: 78-11)]	
3-1088	A	111:1	111:3	Delete from "The balance of" on line 1 to "warming" on line 3. The sentence is untrue [VINCENT GRAY (Reviewer's comment ID #: 88-492)]	
3-1089	A	111:6	111:6	FAQ 3.1 Can you please clarify what is meant by "but the warming evolves differently"? Lines 1-2 (same page, 111) state that "the balance of evidence suggests that the *tropical* lower atmopshere has warmed slightly less than the surface since 1979, though some estimates show equal warming." Lines 3-4 state "balloon data show warming in the *tropical* upper atmosphere (~10km), and ... data indicate cooling in the tropical and global *stratosphere.* The last sentence (lines 5-6) states that "estimates for the lower atmosphere since 1958 show slightly greater overall *global* warming rates than the surface, but the warming evolves differently." What does the phrase "the warming evolves differently" mean? Is the warming of troposphere less than the warming of the	

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				stratosphere? Is the warming in the troposphere (at the same altitude) different at different latitudes? Is the warming of the stratosphere (at the same altitude) different at different latitudes? How does the warming at the Earth's surface compare with these other two variables (e.g., latitude and altitude [stratosphere or troposphere])? What is the main message of this paragraph? [Melinda Marquis (Reviewer's comment ID #: 162-52)]	
3-1090	A	111:6	111:6	"… but the warming evolves differently." Unless you can state simply what this means, it may be better to delete it from the FAQ. [Melinda Marquis (Reviewer's comment ID #: 162-70)]	
3-1091	A	112:1	113:17	This "Question" is also an oversimplied misleadingh summary of the previous text. How many"summaries" do you need? You do not need this one. Delete it. [VINCENT GRAY (Reviewer's comment ID #: 88-494)]	
3-1092	A	112:1		Since the questions will be published separately and will be read by large numbers of people who are not experts on climate change it would be helpful if the terms El Nino, North Atlantic Oscillation (NAO),and Southern oscillation were explained very briefly in this answer to the question. [Wilmer Anderson (Reviewer's comment ID #: 5-32)]	
3-1093	A	112:8	112:8	Insert 'events' after 'heavy precipitation'. [Blair Trewin (Reviewer's comment ID #: 266-56)]	
3-1094	A	112:8		Suggest for clarity changing to '…heavy precipitation events have…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-28)]	
3-1095	A	112:18	112:18	Add 'or sublimed' after 'condensed'. [JAVIER MARTIN-VIDE (Reviewer's comment ID #: 165-11)]	
3-1096	A	112:18	112:18	Add 'or sublimed' after 'condensed'. [Govt. of Spain (Reviewer's comment ID #: 2019-71)]	
3-1097	A	112:22	112:22	What is the antecedent of "this"? PDSI? If so, I'd suggest revising the sentence to read, "… , which factors in crude estimates of changes in evaporation." If not, I'd suggest clarifying to what "this" refers. [Melinda Marquis (Reviewer's comment ID #: 162-49)]	
3-1098	A	112:33	112:35	Does the increase of 7% K-1 refer to the saturation mixing ratio? If this is correct, then it would be specified by adding this magnitude or the corresponding physical magnitude after the water holding capacity of the atmosphere. [Govt. of Spain (Reviewer's comment ID #: 2019-30)]	
3-1099	A	112:35	112:36	The statement "Observations suggest that relative humidity remains about the same overall…" is a typical example of an inacceptable inbalance of a statement on a climate element with a really poor database compared to e.g. temperature (with a long-term and	

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				good quality database of global coverage). In fact we cannot say today if relative humidity has changed or not at global scale, let's say for example during the 20th century. A statement like this one - still being unchanged in the reviewed version - is kind of cheating and constrains future research ("why doing something in fields which are already solved!?" [Reinhard Böhm (Reviewer's comment ID #: 23-2)]	
3-1100	A	112:36	112:37	It will be puzzling to the general reader why "increased water vapour" should result "in part from increased drying at the surface". This sentence needs some expansion and explanation. [David Wratt & David Fahey (Reviewer's comment ID #: 67-106)]	
3-1101	A	112:48	112:48	El Niño related droughts and floods have a much wider influence than the text suggests. Change to 'much of the mid-latitudes of the Pacific-rim countries'. [Govt. of Australia (Reviewer's comment ID #: 2001-252)]	
3-1102	A	112:54		Suggest for clarity definining NAO. [David Wratt & David Fahey (Reviewer's comment ID #: 67-29)]	
3-1103	A	112:56	112:57	For instance in the European sector ...and north African regions. Please add the following reference to that statement: Xoplaki et al. 2004.Xoplaki, E., Gonzalez-Rouco, J. F., Luterbacher, J., and H. Wanner, 2004: Wet season Mediterranean precipitation variability: influence of large-scale dynamics and trends, Climate Dynamics, 23, 63-78 [Jürg Luterbacher (Reviewer's comment ID #: 151-5)]	
3-1104	A	113:13	113:13	Replace 'El Niño' with 'ENSO' (to make it clear that, depending on the region, heavy rainfall events might be associated with La Niña instead of El Niño). [Blair Trewin (Reviewer's comment ID #: 266-57)]	
3-1105	A	114:1	115:16	Another unnecessary "summary. Delete it [VINCENT GRAY (Reviewer's comment ID #: 88-495)]	
3-1106	A	114:4	112:6	Rather than saying "heat waves have increased" we suggest it would be better to say "the number of heat waves have increased" - as otherwise there is some ambiguity as to whether the text is referring to intensity and / or number. Likewise with warm nights etc. If the intention for this sentence is to indicate the NUMBERS of various events have increased we suggest the following rewording: Since 1950 THE NUMBER OF heat waves HAS increased and widespread increases have occurred in THE NUMBER OF warm nights. Drought FREQUENCY also HAS increased ... warmer conditions. Generally, NUMBERS OF heavy daily precipitation events have increased ... [David Wratt & David Fahey (Reviewer's comment ID #: 67-107)]	
3-1107	A	114:4	114:4	Insert 'the frequency of' before 'warm nights'. [Blair Trewin (Reviewer's comment ID #: 266-58)]	

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3-1108	A	114:4		Suggest for clarity that the first paragraph of the answer follow the order in the question: heat waves, floods, droughts, hurricanes. Further suggest that the word 'flood' be used somewhere as in 'heavy precipitation events that lead to flooding'. [David Wratt & David Fahey (Reviewer's comment ID #: 67-31)]	
3-1109	A	114:5		Suggest that the word 'evapotranspiration' not be used in the first paragraph of the answer because the word will not be widely recognized by the non-expert reader. Perhaps 'evaporation' could be used here without loss of meaning. Another reason to remove 'evapotranspiration' is that it does not appear to be used elsewhere in the answer. [David Wratt & David Fahey (Reviewer's comment ID #: 67-32)]	
3-1110	A	114:6	114:7	Suggest rewording: 'Heavy daily precipitation events have become more frequent in most, but not all, locations'. [Blair Trewin (Reviewer's comment ID #: 266-59)]	
3-1111	A	114:12	114:21	The use of the example of a bell curve, and discussion of the 1st, 5th and 10th percentiles, are not appropriate for daily precipitation. "Daily precipitation amount' should be deleted from line 13, and a sentence added at line 17 (after 'constitutes the mean'): 'For variables which are bounded below by zero, such as daily precipitation amount, only the high percentile extremes (e.g. the 90%, 95%, 99% values) are considered'. [Govt. of Australia (Reviewer's comment ID #: 2001-253)]	
3-1112	A	114:12		This introductory paragraph is unfortunately very complex and will confuse many of the non-experts reading this answer. For example, many readers will stumble badly when trying to understand the thought 'exhibit some kind of bell curve when frequency of values in narrow intervals is plotted.' It isn't even clear to me exactly what is meant. I suggest strongly that either one of two changes be made. The first option is to illlustrate in a cartoon figure the concept of bell curve, distribution, percentiles, tails, etc. and more fully explain what is meant by these terms/concepts in the figure caption. The other option is to write this introductory material without using these terms and include parenthetical remarks, if any, that tie the concepts to the rigorous definitions. I think that this latter option is simpler and will be more effective for this format. [David Wratt & David Fahey (Reviewer's comment ID #: 67-33)]	
3-1113	A	114:14	114:14	I am not sure what "some kind of bell curve means". Be more explicit. [Chris Folland (Reviewer's comment ID #: 71-55)]	
3-1114	A	114:23	114:29	FAQ 3.3 and the figure for it: Given the greater increase in warm nights than in warm days from 1980 to present (shown in Q3.3, Fig. 1), why hasn't the DTR for 1979-2004 decreased? I'm sorry, but I still find this confusing. [Melinda Marquis (Reviewer's comment ID #: 162-50)]	
3-1115	A	114:23		Add for clarity at start of sentence 'In the last 50 yrs, over 70%…' What is meant by "significant" in this sentence? Perhaps change the wording to : …. Has shown a	

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				STATISTICALLY SIGNIFICANT decrease …" (if that is what is meant) and explain meaning of statistical significance in a footnote ? [David Wratt & David Fahey (Reviewer's comment ID #: 67-108)]	
3-1116	A	114:29	114:30	"Despite … heat waves." Is this needed in an FAQ? "Cold-tail distribution" may lose a few readers in our FAQ-audience. [Melinda Marquis (Reviewer's comment ID #: 162-71)]	
3-1117	A	114:29		Insert the following as indicated: …Figure 1). These trends are consistent with LLGHGs causing global wrming. The daytime temperatures are influenced by both the solar radiation which is "on" only during the day and the long-wavelength back radiation due to the LLGHGs which is "on" all the time. The nightime temperatures are influenced only by the back radiation due to the LLGHGs. Thus one expects the day temperatures to be fractionally influenced less by an increase in the the greenhouse effect than the night temperatures in either the winter or the summer. Despite the greater increase ... [Wilmer Anderson (Reviewer's comment ID #: 5-62)]	
3-1118	A	114:35		What is meant by the phrase "For more rare precipitation events" ? Expand or explain. [David Wratt & David Fahey (Reviewer's comment ID #: 67-109)]	
3-1119	A	114:42	114:42	Please note that Figure 1(Question 3.2) has not a) and b) but only Top and Bottom [ILEANA MARES (Reviewer's comment ID #: 161-24)]	
3-1120	A	114:54	114:54	Replace 'El Niño' with 'ENSO', to include La Niña effects as well. [Blair Trewin (Reviewer's comment ID #: 266-60)]	
3-1121	A	115:7	115:14	As in comments above the poleward shift of the storm tracks related to the NAO may have reversed. Its more a strong fluctuation than a shift on the latest evidence. [Chris Folland (Reviewer's comment ID #: 71-56)]	
3-1122	A	115:8		Expand "NH" to Northern Hemisphere. The non-technical reader might not understand NH. [David Wratt & David Fahey (Reviewer's comment ID #: 67-110)]	
3-1123	A	115:10	115:10	Explain what DJF stands for [Wilmer Anderson (Reviewer's comment ID #: 5-33)]	
3-1124	A	115:10		Expand "DJF" to December / January / February, for the benefit of the non-technical reader. [David Wratt & David Fahey (Reviewer's comment ID #: 67-111)]	
3-1125	A	115:12	115:14	The statement that "increases in many areas simply arise because there are more people to observe these phenomena" is not supported by the literature. King (1997) [King, P. S. W., 1997: On the absence of population bias in the tornado climatology of southwestern Ontario. Wea. Forecastng, 12, 939–946.] has shown that there appears to be a relatively low threshold of population density necessary to get a high fraction of the true events	

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				reported. Instead, the increase in reports is more likely due to increased public awareness and efforts to collect information. For instance, informal networks of storm chasers in Europe now collect and collate reports of events that, historically, would have made it into local newspapers, but no farther in the media. In the US, increased emphasis on soliciting and collecting reports to verify forecasts of severe weather has led to the increase in reports. I'd change the final clause in the sentence to "increases in many areas simply arise because of increased public awareness and improved efforts to collect reports of phenomena." [Harold Brooks (Reviewer's comment ID #: 31-6)]	
3-1126	A	116:1	116:56	This appendix gives a spurious impression of accuracy which cannot be justified when the data are not randomly distributed. It contains no attention to the problem of bias. The "surface temperature record" is the most important example.Attempts to coorect for this bias are inadequate (see McKitrick and Micheals 2004 "A test of corrections for extraneous signals in gridded surface temperature data'. Climate Research Vol 26 pages 159-173 [VINCENT GRAY (Reviewer's comment ID #: 88-496)]	
3-1127	A	116:1	123:23	These appendices add unnecessary length to the report and are not appropriate to be included in an assessment of climate change. For background information contained in Appendix 3A the reader should be referred to a suitable text book, while information similar to that in 3B is available from WMO. [Govt. of Australia (Reviewer's comment ID #: 2001-254)]	
3-1128	A	116:14	116:16	I am very concerned about the plotting of filtered data right upto the end-points of the data. The description of the filter is described well in appendix 3A and so from a technical point of view the plots using the filters are doing what is expected of them. However I believe that the use of filters near end-points is misleading, it depends on subjective choices being made of what to pad the filter with. The trend near the end points will almost certainly not be the same as will be subsequently found when more data is eventually added. continued in next row [Gareth S. Jones (Reviewer's comment ID #: 121-58)]	
3-1129	A	116:14	116:16	... Continued from previous row The text states that "If there is a trend, then this will be conservative in the sense that it will underestimate the anomalies at the end." However the text does not state the obvious inverse of this, that if there is no trend, then the method could overestimate the anomalies at the end. So a plot could suggest that there is a trend near the end of data when there might be none. More importantly underestimating a real trend could be more misleading. As Mann et al 2004 themselves point out, mis-interpretating trends at ends of data has been done before. Mischievous people might claim from the graphs that global	

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				temperatures have decelerated increases, something which is not supported by the currently available data. They could alternatively claim subjective processing of the data. ... Continued on next row [Gareth S. Jones (Reviewer's comment ID #: 121-59)]	
3-1130	A	116:14	116:16	... Continued from previous row I know that there has been some discussion about how to apply the filters, and that an agreement has been made between the authors. That, however, does not mean it is the best way of showing the data. I see no problem in not showing smoothed data at the ends of data, we must accept that many different people will see these graphs, not just technical people with sophisticated knowledge of filters. So what is shown must be as accurate as can be possible and not have artificial uncertainty added to them. Smoothed data should only be shown when data, that goes into the filter window, is available. [Gareth S. Jones (Reviewer's comment ID #: 121-60)]	
3-1131	A	116:40	116:51	Clarify whether you allow for uncertainties in the individual values when these are available. This increases the trend uncertainty, though does not not usually affect the trend. [Chris Folland (Reviewer's comment ID #: 71-57)]	
3-1132	A	116:55	116:56	The sentence beginning, "Nevertheless, the results depend…" is vague, disputatious and incorrect. It applies more to the REML results, which are presented without such caveat in the chapter. No citation to any literature is given to defend the implication that fractionally-integrated estimators are less physically-realistic than the linear regression models used elsewhere. Persistency models were developed in hydrology precisely to improve physical realism, so as to provide a better match between the stochastic model and the geophysical phenomena. As for transparency, the lack of transparency of GCM's or other numerical models is never regarded as a deficiency in IPCC documents. And there is no sense in which fractional-integration models lack transparency--the methods are well-known and code is published. They are not trivial, but that doesn't mean they are not transparent. The sentence is wrong, unnecesary and should be removed. [Ross McKitrick (Reviewer's comment ID #: 174-13)]	
3-1133	A	117:16	117:16	Add at end "These procedures do not tackle thebias resulting from the fact that the measuring equipment is not randomly distributed, either within a grid box, or by grid boxes themselves" [VINCENT GRAY (Reviewer's comment ID #: 88-497)]	
3-1134	A	117:44	117:44	Replace "some adjustments are quite uncertain" with "most adjustments are useless"	

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				[VINCENT GRAY (Reviewer's comment ID #: 88-498)]	
3-1135	A	117:47	117:47	Insert "sometimes" after "is" [VINCENT GRAY (Reviewer's comment ID #: 88-499)]	
3-1136	A	117:52	117:52	Delete "almost" [VINCENT GRAY (Reviewer's comment ID #: 88-500)]	
3-1137	A	117:57		Insert following after period: "However, using a "normal" 30-year period while the climate is changing is an inadequate method for examining temperature changes. Vinnikov et al. (2002, 2004) have presented a powerful new technique for analysis and display of the diurnal and seasonal cycles of mean climate and climate change, which is insensitive to missing data and makes no requirement of the definition of a normal period. Examples show the detailed patterns of the seasonal cycle of diurnal cycle changes as well as changes of variance." ref: Vinnikov, Konstantin Y., Alan Robock, and Alan Basist, 2002: Diurnal and seasonal cycles of trends of surface air temperature. J. Geophys. Res., 107 (D22), 4641, doi:10.1029/2001JD002007. Vinnikov, Konstantin Y., Alan Robock, Norman C. Grody, and Alan Basist, 2004: Analysis of diurnal and seasonal cycles and trends in climatic records with arbitrary observation times. Geophys. Res. Lett., 31, L06205, doi:10.1029/2003GL019196. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-16)]	
3-1138	A	118:1	118:1	Suggest making the intent of this clearer by adding to state '..systematic biases across a substantial proportion of a network'. [Blair Trewin (Reviewer's comment ID #: 266-61)]	
3-1139	A	118:8	118:8	"However often the change is not documented, and its date must be determined by iterative tests". This is true for e.g. Alexandersson's method, but Caussinus and Mestre's (2004) method (not quoted in the actual bibliography) allows to detect and correct an unknown number of homogeneity breaks in climatological data series. Caussinus, H. and O. Mestre (2004) : Detection and correction of artificial shifts in climate series. Appl. Statist., part 3, 405-425. [Govt. of France (Reviewer's comment ID #: 2010-45)]	
3-1140	A	118:12	118:12	Add at end "In practice, these procedures cannot be applied rigorously unless there are many reliable stations. This situatio n applies to the USA and to China, but probably cannot be applied elsewhere" [VINCENT GRAY (Reviewer's comment ID #: 88-501)]	
3-1141	A	118:14	118:18	There are some evidences that also, on large averaging areas, trends (in particular as far as temperature is concerned) are affected by systematic biases that give a spurious signal which is sometimes comparable to the trend we want to study (Boehm et al., 2001 [this is already in the references]; Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2006:	

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				Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381.) [Michele Brunetti (Reviewer's comment ID #: 33-4)]	
3-1142	A	118:15	118:15	Please give also reference to Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2006: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., 26, 345-381. [Teresa Nanni (Reviewer's comment ID #: 186-4)]	
3-1143	A	118:33	118:33	Add at end "Systematic upwards bias, even in supposedly "corrected" surface temperature series, has been demonstrated by McKitrick and Michaels 2004 "A test ofcorrections for extraneous signals in gridded temperature data" Climate Research Vol 26 pages 158-173. They identify a statistically significant influence of a range of socioeconomic factors such as increases in population, coal usage prosperity, and defective data. Their "corrected" temperature trend from 1979 to 2000 fell from 0.270°C per decade to 0.011°C per decade" [VINCENT GRAY (Reviewer's comment ID #: 88-502)]	
3-1144	A	118:34	118:36	Recommend insertion of following text. General homogeneity adjustments routinely applied to land temperature observations should be used with caution in the analysis of individual stations. Site-specific land cover, microclimate, and instrument placement have been demonstrated (Christy, 2002; Christy et al., 2005; Davey and Pielke Sr, 2005; Gallo, 2005; Pielke Sr, 2005) to confound and override the general assumptions often used in homogeneity adjustments. [Kevin Gallo (Reviewer's comment ID #: 79-1)]	
3-1145	A	118:35	118:46	I would like to see a reference to emerging studies of homogenizing daily data which explicitly adjust the higher order moments. E.g. P. M. Della-Marta and H. Wanner. A method for homogenising the extremes and mean of daily temperature measurements. Journal of Climate, In Press, 2006. This work also gives a good summary of other existing methods of daily data homogenization, including Trewin, B. C. and A. C. F. Trevitt (1996) The development of composite temperature records. Int. J. Climatol.,16, 1227-1242. [John Caesar (Reviewer's comment ID #: 36-5)]	
3-1146	A	118:36	116:36	Replace "only a" by "very few" [VINCENT GRAY (Reviewer's comment ID #: 88-503)]	
3-1147	A	118:37	116:37	Insert after "homogeneous" "so far, successfully applied only to the continental USA and China" [VINCENT GRAY (Reviewer's comment ID #: 88-504)]	
3-1148	A	118:47	118:47	No mention is made of the problem of incomplete data, which are regularly included in	

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				averages. The poroblem is particularly acute with data from Russia from 1989 to 2001, as documented by McKitrick and Michaels 2004 "A test of corrections for extraneous signals in gridded temperature data" Climate Research Vol 26 pages 158-173., Figure 3. [VINCENT GRAY (Reviewer's comment ID #: 88-505)]	
3-1149	A	121:13	121:41	Don't see the need for this; most readers don't need to know this much detail about LKS and HadAT, and why omit other methods here? What's the point of going over this ground twice? [Melissa Free (Reviewer's comment ID #: 76-9)]	
3-1150	A	121:18	121:20	In the sentence that spans these lines, it should also be pointed out that there has been a tendency for many stations to change from launching/reporting twice a day to launching/reporting once a day (illustrated, for example, in Fig.2 of Uppala et al., 2005). This is significant from the trend viewpoint because of the day-night bias differences for many (especially older) measurements. [Adrian Simmons (Reviewer's comment ID #: 242-75)]	
3-1151	A	121:25	121:25	Unless it's intended to be a standard across the whole document (which I doubt), I don't think the acronym CDRs is necessary or useful and it would be better spelt out. [Blair Trewin (Reviewer's comment ID #: 266-62)]	
3-1152	A	121:39	121:39	"Likely time- varying" is better than "possibly enhanced". [Chris Folland (Reviewer's comment ID #: 71-58)]	
3-1153	A	121:39		I'm not sure "possibly enhanced" is quite the right way to put it. Should "possibly enhanced" be replaced by "time-varying radiation", or is something else being referred to? [Adrian Simmons (Reviewer's comment ID #: 242-76)]	
3-1154	A	122:14		Simmons et al.(2004) must be referred to as well as Bengtsson et al.(2004) here, if only to avoid infringement of copyright. The text in this paragraph is lifted directly from the opening paragraph of the paper. [Adrian Simmons (Reviewer's comment ID #: 242-77)]	
3-1155	A	122:31	123:23	Given this good discussion, is it possible to reduce some of the broadly similar discussion in the main text on this topic? [Chris Folland (Reviewer's comment ID #: 71-59)]	
3-1156	A	122:31	123:23	Would it not make more sense to place this section before the preceding reanalysis section from the point of view of continuity? [Peter Thorne (Reviewer's comment ID #: 264-15)]	
3-1157	A	125:1	125:10	Fig 3.2.1. It would be nice to add error bars to the CRUTEM3 bars. [Chris Folland (Reviewer's comment ID #: 71-60)]	
3-1158	A	125:6	125:10	Figure 3.2.1 It is not obvious which of the two filters described in Appendix 3.A are used. Please clarify.	

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				[Gareth S. Jones (Reviewer's comment ID #: 121-25)]	
3-1159	A	125:6	125:10	Figure 3.2.1 This is a very important plot and is mostly clear and understandable but... Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. In this case the method used to deal with filtering endpoints ("minimum slope") causes the smoothed curves to suggest a deceleration in the increase of temperatures at the start of the 21st Century. This could be mischievously taken adavantage of by some when such an assertion really cannot be made based on the available data. The method of coping with end points may also exagerate the difference between the CRUTEM3 and NCDC plots. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-26)]	
3-1160	A	126:0	126:	Figure 3.2.2 – Define DTR [Govt. of United Kingdom (Reviewer's comment ID #: 2022-7)]	
3-1161	A	126:5	126:8	Figure 3.2.2 It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-27)]	
3-1162	A	126:5	126:8	Figure 3.2.2 Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. In this case the method used to deal with filtering endpoints, "minimum slope", causes the smoothed curves to suggest a deceleration in the increase of temperatures around the turn of the century. Such a conclusion really cannot be made based on the available data. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-28)]	
3-1163	A	127:0	127:	Figure 3.2.3 USHCN? – figure captions should be understandable on their own [Govt. of United Kingdom (Reviewer's comment ID #: 2022-8)]	
3-1164	A	127:5	127:10	The time period covered by data is missing. [Govt. of Finland (Reviewer's comment ID #: 2009-53)]	
3-1165	A	128:1	128:11	Fig 3.2.4. It would be nice to add error bars to the HadSST2 bars. The green line could be	

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				darker and possibly continuous. [Chris Folland (Reviewer's comment ID #: 71-61)]	
3-1166	A	128:4	128:11	Figure 3.2.4 It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-29)]	
3-1167	A	128:4	128:11	Figure 3.2.4 Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-30)]	
3-1168	A	130:1	130:7	It would instructive to add a hemispheric difference plot, with error bars. This diagnostic has been used in climate change detection e.g. by Mike Schlesinger. [Chris Folland (Reviewer's comment ID #: 71-62)]	
3-1169	A	130:5	130:7	The time period covered by data is missing. [Govt. of Finland (Reviewer's comment ID #: 2009-54)]	
3-1170	A	130:5	130:7	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-31)]	
3-1171	A	130:5	130:7	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. In this case the method used to deal with filtering endpoints, "minimum slope", causes the smoothed curves to suggest a deceleration in the increase of temperatures around the turn of the century. This could be mischievously taken adavantage of by some when such an assertion really cannot be made based on the available data. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-32)]	
3-1172	A	131:1	131:7	Fig 3.2.1. It would be nice to add error bars to the HadCRIUT3 bars. [Chris Folland (Reviewer's comment ID #: 71-63)]	
3-1173	A	131:5	131:7	The time period covered by data is missing.	

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				[Govt. of Finland (Reviewer's comment ID #: 2009-55)]	
3-1174	A	131:5	131:7	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-33)]	
3-1175	A	131:5	131:7	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-34)]	
3-1176	A	132:0		Figure 3.3.1: Please replace "VASClim" by "VASClimO" as this is the name of the data set resulting from the research project VASClimO (Variability Analysis of Surface Climate Observations). [Christoph Beck (Reviewer's comment ID #: 17-7)]	
3-1177	A	132:5	132:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-35)]	
3-1178	A	132:5	132:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-36)]	
3-1179	A	132:7	132:7	Rudolf et al. 1994 is the correct reference for the GPCC v.3 data but not for the VASClimO data set. The correct VASClimO reference is: Beck et al. 2005 - Beck, C., J. Grieser and B. Rudolf (2005): A new monthly Precipitation Climatology for the global land areas for the period 1951 to 2000. Climate Status Report, 2004: 181-190, German Meteorological Service – available via http://www.dwd.de/de/FundE/Klima/KLIS/prod/KSB/ksb04/28_precipitation.pdf [Christoph Beck (Reviewer's comment ID #: 17-8)]	
3-1180	A	133:0		Figure 3.2.9 (also figure 3.3.2, page 3-137). In these figures, the upper and lower maps should use the same units and scales to allow ready comparisons of the rate of change between the two periods. In Figure 3.2.9 as it currently stands, the rate of warming	

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				appears to be greater over the full century than in the post-1979 period, when in fact the reverse is true. [Govt. of Australia (Reviewer's comment ID #: 2001-255)]	
3-1181	A	133:1		Figure 3.2.9. Can you provide uncertainties (e.g. a range or a typical value) for the temperature trends shown at a grid cell level – or at some other regional scale. The figure implies a fairly high degree of confidence because of the spatial consistency of the trend patterns, but to back this up with a statement about the uncertainty in the trend at the grid cell level would provide useful complementary information. [Martin Manning (Reviewer's comment ID #: 155-5)]	
3-1182	A	133:10	133:10	Add at end. "The upper diagram is misleading since the record ffrom 1901-2005 is highly irregular, and cannot be regarded as "linear" [VINCENT GRAY (Reviewer's comment ID #: 88-506)]	
3-1183	A	136:0		Figure 3.3.1. Why is 1981-2000 used as the base period here, rather than a longer averaging period (e.g. 1961-1990)? [Govt. of Australia (Reviewer's comment ID #: 2001-256)]	
3-1184	A	136:2	:2	replace "VasClim" by "VASClimO" which is the correct name of the project and means Variability Analysis of Surface Climate Observations [Jürgen Grieser (Reviewer's comment ID #: 89-3)]	
3-1185	A	136:5	136:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-37)]	
3-1186	A	136:5	136:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-38)]	
3-1187	A	137:0	138:	Please, consider to be redone Figure 3.3.3 as it is totally unreadable: neither the legend of the central figure nor the 19 graphs in the panels associatted to different regions on the globe. [Constanta Emilia Boroneant (Reviewer's comment ID #: 26-2)]	
3-1188	A	137:0		Figure 3.3.3. The region names are too small to be readable. [Galina Churkina (Reviewer's comment ID #: 42-3)]	
3-1189	A	137:0		Figure is impossibly small to read	

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-260)]	
3-1190	A	137:1	137:1	Different scales have been used on each of the figures (deg/dec the other deg/century, colour scale is different too) - either use different colour scheme for each or use the same scale. Using the same colours is misleading. [Govt. of Australia (Reviewer's comment ID #: 2001-257)]	
3-1191	A	139:1	139:7	Fig 3.3.4 is of poorer quality than most of the others. [Chris Folland (Reviewer's comment ID #: 71-64)]	
3-1192	A	139:5	139:5	It's probably better to put the info about the filter (1/12 [1-3-4-3-1]) into the text where this figure is discussed (page 18, line 9). [Melinda Marquis (Reviewer's comment ID #: 162-46)]	
3-1193	A	140:0	143:	All these diagrams need to be redrawn and transferred to after page 135 to follow on after the "Surface Temperature" part of the text [VINCENT GRAY (Reviewer's comment ID #: 88-507)]	
3-1194	A	140:0		Fig 3.4.1 The negative slice in T(Trop) above 100 hPa is a bit overdone. Q. Fu will also complain about this since his new set of coefficients do not have quite so much negative weight above 100 hPa. T*G in the current CCSP figure is a bit better. [John Christy (Reviewer's comment ID #: 41-25)]	
3-1195	A	141:0	141:	This diagram is confusing. The temperature records for the radiosonde and for the MSU (satellites) should be on separate sheets, uncontaminated with each other and the surface record. The lower troposphere temperature records definitely do not agree with the surface record. The radiosonde record shows no temperature change from 1958 to 2004, whatever is ckclaimed for a "linear" trend, and the MSU record shows no temperature change from 1978 to 1998. It is unacceptable to draw a "linear trend" through records that are dominated by the 1999 El Niño event [VINCENT GRAY (Reviewer's comment ID #: 88-508)]	
3-1196	A	141:3	141:3	Correct spelling of El Chichón to include accent on "o." -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-8)]	
3-1197	A	142:0	142:	It is not acceptable to give "linear trends" which include the 1999 El Niño peak. Since the surface, lower troposphere radiosonde and satellite records are all approximately linear from 1978 to 1998 this graph should be redrawn with linear trends for this region to display the differences which exist between the surface and lower troposphere reccords [VINCENT GRAY (Reviewer's comment ID #: 88-509)]	
3-1198	A	142:0		Fig. 3.4.3 It looks like you have 10 time series to identify. That makes it tough in chosing colors, but I found it difficult to separate the reddish colors. [John Christy (Reviewer's comment ID #: 41-28)]	

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3-1199	A	142:0		Figure 3.4.3. To paraphrase Wallace,these are "the wrong errors gromett". The uncertainty in the linear trend implies that datasets are consistent whereas taking the difference would yield many significant differences. This could be got around by denoting with a star each that is significant difference series to a "reference for that level" timeseries. Even better would be to ditch the totally meaningless error bars. They are not errors unique to each dataset, rather they are an expression of the common high frequency variability. The lowermost panel also needs a full key as I'm extremely confused as to what these 4 unlabelled trends are (I suspect there should be only 2) and the upper-most panels do not help in this regard. Suggest surface to lower trop is defined by a solid rather than a dashed line to help in this interpretation. [Peter Thorne (Reviewer's comment ID #: 264-10)]	
3-1200	A	143:0		Fig. 3.4.4. I noted last time that this figure is the outlier (i.e. the one showing maximum warming) based on Fig. 3.4.3. This is another case where the authors are leaving themselves open to bias by selecting the most extreme case of tropospheric warming among the extant datasets. At the minimum, the caption can be honest and state that this is the most extreme case of warming of the datasets. [John Christy (Reviewer's comment ID #: 41-26)]	
3-1201	A	143:0		Figure 3.4.4 is not both RSS and UAH as implied by the text (It may therefore have slipped through the net). It is also not sensible to stretch the Fu et al. technique this far. It was developed for large-scale diagnostics. Here it is being applied to the gridbox scale. This isn't particularly sensible. Suggest use of Figure 4.3 from CCSP report here which i suspect may have been decided but not implemented. [Peter Thorne (Reviewer's comment ID #: 264-11)]	
3-1202	A	143:5	143:5	Add at end "This figure is spurious, because the "linear trend" is dominated by the very large El Niño peak in 1999" [VINCENT GRAY (Reviewer's comment ID #: 88-510)]	
3-1203	A	145:0		Fig. 3.4.6. This is of interest perhaps. On a lark, I calculated the global UAH LT v5.2 trend for the period shown in the figure based on water vapor and RSS temperature. The result? UAH LT was +0.17 K/decade, exactly what is shown from this independent methodology. Should this not be taken as a sort of verification of the UAH methodology? [John Christy (Reviewer's comment ID #: 41-27)]	
3-1204	A	148:1	148:7	The weak trend pattern in the top left may have nearly disappeared. Can PMSL be used in the NH diagrams, to extend the record to now e.g using the Allan and Ansell (2006) data set?. [Chris Folland (Reviewer's comment ID #: 71-65)]	
3-1205	A	152:0	152:	Please, enlarge the space between the bottom figures so as to make readable the y-label for Darwin Southern Oscillation Index	

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				[Constanta Emilia Boroneant (Reviewer's comment ID #: 26-3)]	
3-1206	A	152:0		Figure 3.6.2. The caption is very difficult to understand. [Galina Churkina (Reviewer's comment ID #: 42-4)]	
3-1207	A	152:1	152:11	The SOI index can be extended back to 1850 e.g as in Allan and Ansell (2006), or some variation of the SOI based on that data set.. [Chris Folland (Reviewer's comment ID #: 71-66)]	
3-1208	A	152:5	152:10	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-39)]	
3-1209	A	152:5	152:10	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-40)]	
3-1210	A	152:8	152:8	Please insert point and comma after Konnen at al [ILEANA MARES (Reviewer's comment ID #: 161-25)]	
3-1211	A	153:7	153:10	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-41)]	
3-1212	A	153:7	153:10	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-42)]	
3-1213	A	154:1	154:5	My suggestion is to delete Aleutian SLP and Indian SST on the ordinates, because there are these specifications insight of these pictures. [ILEANA MARES (Reviewer's comment ID #: 161-26)]	
3-1214	A	154:13	154:14	.. To facilitate comparison with (a), What means (a) ? Might be Top Figure as is in text, namely Figure 3.6.4a and b instead of top and lower? [ILEANA MARES (Reviewer's comment ID #: 161-27)]	

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3-1215	A	156:1	156:13	The top two diagrams at least can be extended back to (near) 1850 using the Allan and Ansell (2006) data set. [Chris Folland (Reviewer's comment ID #: 71-67)]	
3-1216	A	156:1	156:2	My suggestion is to delete the explanation on the ordinates, because there are these specifications insight of the pictures. [ILEANA MARES (Reviewer's comment ID #: 161-28)]	
3-1217	A	156:4	156:13	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-43)]	
3-1218	A	156:4	156:13	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-44)]	
3-1219	A	157:6	157:12	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-45)]	
3-1220	A	157:6	157:12	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-46)]	
3-1221	A	157:6	157:12	Figure caption should be ordered top, middle, bottom, not bottom, middle, top. [Adrian Simmons (Reviewer's comment ID #: 242-78)]	
3-1222	A	158:5	158:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-47)]	
3-1223	A	158:5	158:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to	

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				introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-48)]	
3-1224	A	159:0		Figure 3.7.1 What is "epoch (??) mean annual range of precipitation"? [Galina Churkina (Reviewer's comment ID #: 42-5)]	
3-1225	A	160:5	160:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-49)]	
3-1226	A	160:5	160:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-50)]	
3-1227	A	161:6	161:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-51)]	
3-1228	A	161:6	161:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end points. [Gareth S. Jones (Reviewer's comment ID #: 121-52)]	
3-1229	A	162:6	162:8	It is not obvious which of the two filters described in Appendix 3.A are used. Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-53)]	
3-1230	A	162:6	162:8	Use of smoothed curve right to ends of plot is misleading. It gives false impression of confidence of trend near the ends. An accurate deduction of the trend at the end of the data cannot be made with the available data when used with the filtering method. Mann etal 2004 showed that any one method of endpoint filtering does not fit all data situations and that such smoothed plots can be misused. I really don't understand the need to introduce artifical uncertainty into these important plots. I strongly recommend not showing smoothed plot to within filter window length of end	

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				points. [Gareth S. Jones (Reviewer's comment ID #: 121-54)]	
3-1231	A	163:0		Fig. 3.8.1. These are probability distribution functions rather than density functions. [Lisa Alexander (Reviewer's comment ID #: 1-12)]	
3-1232	A	164:2	164:18	In order to be in concordance with the text please change Upper, Middle and Lower with a), b) and c). [ILEANA MARES (Reviewer's comment ID #: 161-29)]	
3-1233	A	165:0	165:	The six plots of ACE index are not visible to read. Nither the x nor the y axis . Please, redo these figure in a readable form. [Constanta Emilia Boroneant (Reviewer's comment ID #: 26-4)]	
3-1234	A	165:0		Figure 3.8.3. The regions' names are too small to be readable [Galina Churkina (Reviewer's comment ID #: 42-6)]	
3-1235	A	165:1	165:11	Fig 3.8.3 would benefit from low frequency curves in each sub-diagram. [Chris Folland (Reviewer's comment ID #: 71-68)]	
3-1236	A	167:0	167:	The resolution of this picture is unappropriate. Please, redo it. [Constanta Emilia Boroneant (Reviewer's comment ID #: 26-5)]	
3-1237	A	167:0		Fig. 3.8.5. Cannot see these graphs properly – needs to be redone. [Lisa Alexander (Reviewer's comment ID #: 1-13)]	
3-1238	A	168:0		Figure 3.8.6. The distributions in the lower panel look suspiciously idealised (are they simply Gaussians fitted to the observed mean/standard deviation?). If my assumption is correct, I think it would be more useful to see the actual 2003 distribution, not a fit to it, to make it easier to assess whether the warmth in 2003 was driven by a shift in the full distribution or a change in its shape. (I couldn't get Basel data easily, but some Zurich data I have suggests that the 2003 distribution, in fact, was rather negatively skewed). [Blair Trewin (Reviewer's comment ID #: 266-13)]	
3-1239	A	168:5	168:9	It is not stated how the data is filtered in the top plot? Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-55)]	
3-1240	A	168:5	168:9	As it is not explained what filter is used the following comment may be spurious. However if the "minimum slope" method is used, as described in Appendix 3.A, then the data upto the endpoint will have been reflected in time. This could mean that the 2003 extreme temperature is included twice in the filter at the end points! Surely this is wrong! Clarify. [Gareth S. Jones (Reviewer's comment ID #: 121-56)]	
3-1241	A	169:0		Comment on the caption for Question 3.1 Figure 1 It might be a good idea to comment on the fact that the temperature changes locally in the NH can be much larger than the global mean temperature changes.	

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				[Wilmer Anderson (Reviewer's comment ID #: 5-59)]	
3-1242	A	169:0		Replace the bottom RHS figure with a T2LT trend plot from either RSS or UAH and denote which within the figure caption. Use of channel recombination at these scales is hard to justify and just leaves the chapter open to attack whereas use of T2LT will be less controversial. [Peter Thorne (Reviewer's comment ID #: 264-14)]	
3-1243	A	169:1	169:2	Top diagram. Would look better if the dots were circular and somewhat smaller. [Chris Folland (Reviewer's comment ID #: 71-69)]	
3-1244	A	169:1		Suggest for clarity and utility to change vertical axis to absolute temperature or to make left hand axis absolute temperature and right hand axis temperature change. [David Wratt & David Fahey (Reviewer's comment ID #: 67-26)]	
3-1245	A	169:9	169:19	How is the plot in the upper figure smoothed? Whatever way the filter is applied it is misleading to show the smooth plot right upto the end points. It will give a false impression of confidence in the trend, which is not covered by the uncertainties also displayed. In this case the smooth plot suggests a deceleration of the upward trend at the start of the 21st century. This could be misused by some mischievous person, when such a conclusion cannot be currently confirmed with the available data. [Gareth S. Jones (Reviewer's comment ID #: 121-57)]	
3-1246	A	169:9		Make it clear in the caption that the temperatures are from measurements, e.g. …Annual global mean temperatures FROM MEASUREMENTS (black dots …)… [David Wratt & David Fahey (Reviewer's comment ID #: 67-104)]	
3-1247	A	169:12	169:13	Suggested wording changes here for clarity: CLIMATE model results DRIVEN BY ESTIMATED RADIATIVE FORCINGS FOR THE 20th CENTURY (Chapter 9) SUGGEST there is little change prior to about 1920,and THAT a substantial fraction … [David Wratt & David Fahey (Reviewer's comment ID #: 67-105)]	
3-1248	A	169:14	169:14	"Solar radiation" is used here to mean changes in the Sun's irradiance. This is different meaning to that used throughtout the rest of the chapter of surface downward shortwave from the Sun. Suggest changing the phrase to "solar irradiance" or something similar but distinct from "solar radiation" [Gareth S. Jones (Reviewer's comment ID #: 121-24)]	
3-1249	A	169:15		Suggest for clarity to break sentence as …variability. From about…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-27)]	
3-1250	A	169:19	169:19	Add at end "This whole diagram is spurious There is no justification to draw a "linear trend" through such an ireegular record, and there is no reason to suppose that a model based on greenhouse gases could possibly simulate it. The fact that the model does not fit it shows that such an assumption is wrong The maps are equally spurious as they are	

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				dominated by the large El Niño event of 1999". They would look very different if they plotted trends from 1978 to 1998 and this should be done. [VINCENT GRAY (Reviewer's comment ID #: 88-511)]	
3-1251	A	170:1	170:10	Please replace top and lower panel with a) and b) as is specified in text. [ILEANA MARES (Reviewer's comment ID #: 161-30)]	
3-1252	A	170:6		The figure caption does not adequately explain the figure to non-experts. Suggest defining the PSI for a non-expert and to explain with more detail what the figures represent. For example, in the global plot, what period of time is represented? [David Wratt & David Fahey (Reviewer's comment ID #: 67-30)]	
3-1253	A	171:0		Q3.3., Fig. 1. Graphs need to be redone since colour bar range is not wide enough [Lisa Alexander (Reviewer's comment ID #: 1-14)]	
3-1254	A	171:2	171:2	Should the TS Table TS-4 (TS, page 31, line 22) note the exception that although much of mid- to high-latitudes showed an increase in warm days and warm nights in the late 20th century, Greenland, southern S. America, and the southeast U.S. showed a decrease in the number of warm days (shown in Question 3.3, Figure 1)? [Melinda Marquis (Reviewer's comment ID #: 162-51)]	
3-1255	A	171:5		Suggest for clarity adding a useful definition of 'percentile temperature indices' that ties in well with text discussion. Also suggest labeling the vertical axes and moving numbers and labels to left hand side of graphs. [David Wratt & David Fahey (Reviewer's comment ID #: 67-34)]	
3-1256	A	171:10	171:10	Add at end. "This behaviour is consistent with the dominant influence of human habitation on the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-512)]	