

IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Technical Summary Comments

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Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

Batch AB (12 SEPTEMBER 2006) & Responses (Final)

#	B A T C H	PAGE	LINE	COMMENT	RESPONSE
TS-1	A	0:0	0:0	It would be welcome to highlight the difference in climate change beyond 2050 between various emission scenarios investigated in chapter TS.5 (e.g. likelihood of change in MOC, meltdown of ice sheets, extreme weather events, frequency and intensity of extreme weather events) [Govt. of Austria (Reviewer's comment ID #: 2002-28)]	Covered in text to the extent practical based on existing literature.
TS-2	A	0:0	0:0	It is proposed to include a list of abbreviations and definitions at the end of the TS [Govt. of Austria (Reviewer's comment ID #: 2002-29)]	A list of acronyms will be added as an annex
TS-3	A	0:0	0:0	The clarity and scientific rigor of the technical summary is very much appreciated and the absence of policy prescriptive language. Congratulation to the authors. [Govt. of Austria (Reviewer's comment ID #: 2002-30)]	Thanks
TS-4	A	0:0	0:0	Please do not split tables e.g. table TS-3 [Govt. of Austria (Reviewer's comment ID #: 2002-34)]	Fix in layout
TS-5	A	0:0	0:	There is excessive repetition in this Chapter. It could easily be reduced by half by judicious pruning [VINCENT GRAY (Reviewer's comment ID #: 88-2104)]	Rejected – no specific suggestions to consider
TS-6	A	0:0		The TS is a particularly useful document which provides excellent references to the body of the WG1 report. It is also more clearly and concisely presented than the SPM and includes many key findings that are not (and should be) included in the SPM. [Govt. of Australia (Reviewer's comment ID #: 2001-44)]	Thanks re first point. Any specifics given regarding what to add to SPM will be considered where they are offered.
TS-7	A	0:0		Water in atmosphere appears in several parts of the TS. It is a complex field for policymakers. It would be helpful to have an integrated picture of the water in atmosphere topic - perhaps a box that gives an integrated overview that leads into individual segments. [Govt. of Australia (Reviewer's comment ID #: 2001-45)]	Water is discussed where appropriate. Box would add length and would not be consistent with the structure of this TS (obs, attribution, projection, etc. kept separate) nor with the approach used for other boxes in this TS which are directed at specific tutorial issues rather than by topic or research approaches or needs.
TS-8	A	0:0		Throughout the TS acronyms have been used inconsistently this should be rectified and all acronyms need to be listed in an appendix. In addition, the term "industrial era" needs to be more clearly explained. [Govt. of Australia (Reviewer's comment ID #: 2001-46)]	Fix in copy edit
TS-9	A	0:0		The TS does not contain a description of the SRES. An explanation of the central components of the SRES, is necessary. In the TAR the SRES was discussed explicitly and	Box describing SRES to be added as in TAR.

				this should also be the case for the AR4. [Govt. of Australia (Reviewer's comment ID #: 2001-47)]	
TS-10	A	0:0		The TS is silent on the issue of abrupt climate change. There is emerging evidence (eg southern Europe, Antarctic Peninsula, SW Western Australia) for abrupt regime changes in the observed climate, rather than the smoothly varying changes portrayed in most "scenarios". This is discussed in chapter 10 and should be included in the TS [Govt. of Australia (Reviewer's comment ID #: 2001-48)]	Current text matches degree to which material is available in the report. Chapter 10 does not contain specific material on western Australia and abrupt change
TS-11	A	0:0		Through much of the text the convention for the use of less/greater for negative numbers is confused. For example on page 10, lines 41 "the sum of these effects has been reduced from more than -4 in TAR to -2.3". This should be rectified. [Govt. of Australia (Reviewer's comment ID #: 2001-49)]	Fix as we go and catch in copy edit
TS-12	A	0:0		The TS is an accurate and clear synthesis of the SOD. [Roxana Bojariu (Reviewer's comment ID #: 24-23)]	Thanks
TS-13	A	0:0		The Technical Summary is well written with clear findings. Only some minor corrections related to using of the standard uncertainty terms have to be made. [Aristita Busuioc (Reviewer's comment ID #: 35-1)]	Thanks
TS-14	A	0:0		The Technical Summary does not pay enough attention to a possible strong positive long term feedback coming from an additional increase in CO2 concentration due to increasing temperature. In my opinion, this could be a major source of uncertainty for long term projections. This is invoked in TS 5.4 (1.2 °C feedback is mentioned for 2100) but Chapter 6 (especially 6.4 and Box 6.2) shows that the variation of Green-House Gases during the last glacial-interglacial has followed Antarctica temperatures with a time lag of several centuries to a millennium. We know from that period that a decrease in temperature of about 5°C corresponded to a CO2 concentration less than 200 ppm. Even if there is still much uncertainty on the processes involved in these feedbacks, we cannot exclude that increasing global temperature up to 5°C (possible with A2 around 2100) will induce a symmetric process in a long term future which would dramatically increase the emission of CO2 from natural reservoirs. This could be a major catastrophic irreversible scenario for the long-term future. [Govt. of France (Reviewer's comment ID #: 2010-119)]	Only material for this would be in Ch06 and qualitative. See chapter 6. Believe current text is appropriate.
TS-15	A	0:0		General suggestion for ALL graphs: (1) if a graph is taken from a source in the literature, it must clearly be referenced; (2) for all graphs generated by IPCC, a standard format should be applied with the approximate dimensions like the one on SPM-3 or TS-2. This consistency would increase readability a lot. Suggestions to enlarge graphs are completed below with suggestions to delete text and graphs to keep the length of the document. [Govt. of Hungary (Reviewer's comment ID #: 2012-7)]	Graphics presentation to be improved in final drafts. Layout of TS-2 not always possible though.
TS-16	A	0:0		General suggestion for ALL graphs with a base period: this period should not be e.g. 1960-1990, because this period has no distinctive characteristics. Rather, the base period should be either the pre-industrial era, or 1850-1900 or any other period at the beginning of the	Have tried to be more consistent in choice of periods being compared where practical. Data does not always allow

				observations. This could then make it clear what has been changed since then (at least partly due to the human activities) since the beginning of the industrial era, or since we have data. [Govt. of Hungary (Reviewer’s comment ID #: 2012-8)]	this. 1960-1990 was chosen because adequate data are available to characterize it in many cases.
TS-17	A	0:0		Some parts of the document could be left out because they are irrelevant. These include Box TS.3.2 and Box TS.3.3. The space gained this way could be used to enlarge some graphs (see my comment on graph size above). [Govt. of Hungary (Reviewer’s comment ID #: 2012-9)]	Reject – other reviewers have indicated need for such material on ice sheets and sea level
TS-18	A	0:0		As in the SPM, there is not much coverage of the importance of the carbon cycle - either for influencing present day CO2 levels, or for amplifying future climate change. I agree with the text that is there, but would like to see it given more coverage. E.g. how about a figure of C4MIP results of future CO2 levels compared with "uncoupled" runs? [Chris Jones (Reviewer’s comment ID #: 120-6)]	Material on carbon cycle feedbacks and C4MIP has been clarified
TS-19	A	0:0		A general problem applying to this whole chapter is apparently the way it is formatted, as the italicized sentence at the start of many paragraphs seems to be a summary rather than an opening sentence. It would have helped to have the italicized sentence set apart as a heading rather than simply the lead of the paragraph. [Michael MacCracken (Reviewer’s comment ID #: 152-55)]	Editorial – here we have an approach allowing a headline statement to be made where appropriate, which we feel is helpful to most readers.
TS-20	A	0:0		The structure of the TS could be improved by modelling it on the SPM. At present it is difficult to find some of the TS material corresponding to that in the SPM. [David Parker (Reviewer’s comment ID #: 195-116)]	Reject – many reviewers supporting TS structure.
TS-21	A	0:0		This chapter gives all information to reader. Hence, it has been done excellent. [FATEMEH RAHIMZADEH (Reviewer’s comment ID #: 205-1)]	Thanks
TS-22	A	0:0		No comments [Michel Rixen (Reviewer’s comment ID #: 215-3)]	Thanks
TS-23	A	0:0		Due to the large number of acronyms that are used in the Technical Summary it could be useful to include them all in the Glossary jointly with the words that they substitute and expand them the first time they appears in the text. For example: Glossary, page 6, "Diurnal temperature range" could appear as "Diurnal temperature range (DTR)". Include also in the Glossary those acronyms that appears in a Box if they are used in the text. For example: pg 20, line 7 "PDO" is used in the text, it is defined in Box T.S.3.1 but there is no reference in the Glossary. [Govt. of Spain (Reviewer’s comment ID #: 2019-77)]	List of acronyms will be included as an Annex. Consistency with glossary will be checked.
TS-24	A	0:0		No major problems identified, it's in reasonably good shape, congratulations to the authors. Some comments made re the SPM are also relevant to the TS, and are repeated here for convenience. [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-50)]	Thanks
TS-25	A	0:0		Most paragraphs in this chapter are headed by bullet-like points in italics. Some of these use the phrase "...with a...level of scientific understanding." It would be good if use of this indication were done more consistently. [Govt. of United States of America (Reviewer’s comment ID #: 2023-875)]	Will check that all confidence levels given in the chapters are kept with material brought into the TS

TS-26	A	1:1	53:56	I find now the TS very well written, clear enough, well structured, reader friendly, and scientific sound. [Paolo Cherubini (Reviewer's comment ID #: 40-2)]	Thanks
TS-27	A	2:1	2:42	Editorial comment: In the table of contents, all the "Box TS....:" are at the same hierarchical level as the chapters, i.e. "TS.1", "TS.2" etc. Confusing. [Paolo Cherubini (Reviewer's comment ID #: 40-1)]	Agreed – will be fixed in final draft
TS-28	A	2:5	2:10	Ozone depletion, as well as greenhouse gases, should be highlighted as a driver of climate in these section headings (eg in 2.3). Several studies show ozone depletion has had a significant impact on atmospheric circulation in higher latitudes. This is particularly relevant in the Southern Hemisphere. [Govt. of Australia (Reviewer's comment ID #: 2001-50)]	Ozone is included in section 2.1. We do not agree that ozone depletion needs a separate section. The connection of ozone depletion to SH circulation has been noted in the TS.
TS-29	A	2:9	2:9	Change "VOLCANOES" to "VOLCANIC ERUPTIONS." My geologist friends insist that volcanoes do not cause climate change, but volcanic eruptions do. -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-6)]	Agreed.
TS-30	A	3:0		Box TS 1.1. There has always been the problem that the equation of "low confidence = 2 out of 10 chance of being correct" can lead to the potentially wrong interpretation that the opposite of such a statement therefore has a greater chance of being correct. (E.g. see Table SPM-1: if we have "low confidence" that the increase in tropical cyclones is "more likely than not due to human influence", this does NOT mean that we should have "high confidence" that it is "more likely than not NOT due to human influence".) It would be very helpful to make this clear by saying that even where confidence is low, the explanation given still represents the currently best possible, or most plausible or reasonable, explanation. This will also have to be harmonised across Working Groups. [Andy Reisinger (Reviewer's comment ID #: 210-65)]	Confidence levels have been clarified.
TS-31	A	3:1		DO NOT BEGIN THE TECHNICAL SUMMARY WITH AN INTRODUCTION. Readers are most interested in key findings, and they should be given these immediately. That is why each of the AR4 WG I chapters has an Executive Summary and why scientific papers begin with Abstracts. Start the Technical Summary with what is now Section TS.6, giving it the title "Executive Summary." Then go on to the Introduction and the later Sections. [Richard Soulen (Reviewer's comment ID #: 248-17)]	Rejected –in the final volume the SPM plays the role suggested here of an Executive Summary and does so more comprehensively than section 6.
TS-32	A	3:4	3:4	TS Comment: Replace the phrase "significant progress has been made in understanding past, recent and future climate change" with "studies of past, recent and future climate changes and their possible causes have continued". The phrase must be replaced because it is not true and the body of the report contains no mention of such "significant progress made in understanding past, recent and future climate change". The body of the report provides information on attribution studies for past and recent	Rejected. There are extensive new datasets described that improve understanding of the past, including e.g., ocean heat content, GRACE, and much more. There are new analyses such as those of the upper air temperature data. There are ensemble model simulations that much better characterize projections as compared to single realizations used

				<p>climate changes and model projections of possible future climate changes that have been conducted since the TAR. But no “significant progress made in understanding past, recent and future climate change” is reported anywhere in the report.</p> <p>It should be noted that an ability to attribute a cause to an observed change merely demonstrates that the suggested cause is one possible explanation for the change. The ability to attribute the suggested cause is not evidence that the attributed cause is responsible for the change in part or in whole, and it does not prove that any other possible cause(s) did not cause the change. By no stretch of the imagination can the results of attribution studies be called “significant progress has been made in understanding past, recent and future climate change”.</p> <p>Furthermore, mystics with crystal balls claim to have “understanding” of the future but scientists do not. Indeed, scientists predict future outcomes (e.g. rate of acceleration of an object falling in a gravity field) using validated models that determine the inherent error magnitudes of the predictions. The TS and the body of the report model studies of possible future climate changes but they provide no evidence of the validity of the models to predict “future climate change”: indeed, the report states that the models only make “projections”, not predictions with estimated uncertainty. By no stretch of the imagination can production and comparison of such “projections” be said to be “significant progress has been made in understanding past, recent and future climate change”.</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-37)]</p>	<p>previously. See the report for many more examples.</p>
TS-33	A	3:4	3:4	<p>TS Comment: Replace the phrase “These advances have arisen from:” with “These studies include acquisition of:”.</p> <p>The phrase must be replaced because it is not true and the body of the report contains no mention of such “advances”.</p> <p>The body of the report provides information on attribution studies for past and recent climate changes and model projections of possible future climate changes that have been conducted since the TAR. But no “advances” are reported anywhere in the report.</p> <p>It should be noted that an ability to attribute a cause to an observed change merely demonstrates that the suggested cause is one possible explanation for the change. The ability to attribute the suggested cause is not evidence that the attributed cause is responsible for the change in part or in whole, and it does not prove that any other possible cause(s) did not cause the change. By no stretch of the imagination can the results of attribution studies be called “advances”.</p> <p>Furthermore, the TS and the body of the report model studies of possible future climate changes but they provide no evidence of the validity of the models to predict “future climate change”: indeed, the report states that the models only make “projections”, not predictions with estimated uncertainty. By no stretch of the imagination can production and comparison of such “projections” be said to be “advances”. Indeed, scientific “advances” in climate modelling are not possible until a climate model has been validated as a tool for</p>	<p>Rejected – see ch 1 regarding advances in modelling and projections for the past few decades, and the comparison of past IPCC projections with what has actually occurred. See ch 8 for extensive discussion of model evaluation, where e.g. it is shown that sea ice is modelled better than previously. See ch 9 for discussion of advances in attribution, which include explicit attribution studies not just of temperature but also of sea level pressure and ocean heat content.</p>

				<p>predicting climate and climate changes.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-38)]</p>	
TS-34	A	3:4		<p>Remove the colon</p> <p>[Richard Soulen (Reviewer's comment ID #: 248-18)]</p>	Agreed
TS-35	A	3:6	3:7	<p>TS Comment: Replace the phrase "The increased confidence in climate science provided by these developments" with "These developments".</p> <p>The phrase must be replaced because it is not true and the body of the report contains no mention of any "developments" that provide "increased confidence in climate science". The body of the report provides information on attribution studies for past and recent climate changes and model projections of possible future climate changes that have been conducted since the TAR. But no reason to have "increased confidence in climate science" is reported anywhere in the report.</p> <p>It should be noted that an ability to attribute a cause to an observed change merely demonstrates that the suggested cause is one possible explanation for the change. The ability to attribute the suggested cause is not evidence that the attributed cause is responsible for the change in part or in whole, and it does not prove that any other possible cause(s) did not cause the change. By no stretch of the imagination can the results of attribution studies be a reason for "increased confidence in climate science".</p> <p>Furthermore, the TS and the body of the report model studies of possible future climate changes but they provide no evidence of the validity of the models to predict "future climate change": indeed, the report states that the models only make "projections", not predictions with estimated uncertainty. By no stretch of the imagination can production and comparison of such "projections" be said to provide "increased confidence in climate science". Indeed, the continued failure during the "last 6 years" to obtain a model with validation and predictive ability provides reason for reduced confidence in climate science.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-39)]</p>	Rejected – see TS 33
TS-36	A	3:7		<p>Insert "by" after the second "as"?</p> <p>[Richard Soulen (Reviewer's comment ID #: 248-19)]</p>	Text edited
TS-37	A	3:8		<p>Replace "over-arching themes" with "evident" and replace "the" with "this"</p> <p>[Richard Soulen (Reviewer's comment ID #: 248-20)]</p>	Agreed
TS-38	A	3:10	3:10	<p>It would be helpful to have a second paragraph at line 10 which discusses what is known about climate and climate change. Presently, the first paragraph emphasises that we have learnt more and the second that we still have a lot to learn. This gives a quite biased picture that we are still quite limited in our knowledge and on a steep learning curve. In reality, much climate change science is quite non-controversial, and the science is now dealing (largely) with second order issues and feedback processes. It should also be made clear at</p>	Text of first paragraph deals with this in part. Disagree there is a need to repeat or extend that here.

				<p>this point that the 'predictions' in the AR4 are not forecasts but are based on probabilistic scenarios. [Govt. of Australia (Reviewer’s comment ID #: 2001-51)]</p>	
TS-39	A	3:13	3:13	<p>TS Comment: Replace the phrase “impose limitations on our ability to predict fully” with “prevent us from predicting” because the phrase is a (deliberately ?) misleading falsehood. The body of the report says that only “projections” – not “predictions” – are possible. [Richard Courtney (Reviewer’s comment ID #: 49-40)]</p>	<p>Text edited, using the word ‘understand fully’ now to emphasize the processes, which follow in this sentence.</p>
TS-40	A	3:28	3:29	<p>This parenthetic definition of the climate system excludes (unfrozen) soils and rivers of the continents. Why? [P.C.D. Milly (Reviewer’s comment ID #: 179-2)]</p>	<p>Not practical to list each of the many elements of the system here. That is why the word ‘including’ was used.</p>
TS-41	A	3:39		<p>The introduction of the report should include an overview of specific climate effects before discussion progresses to the more specific topic of radiative forcing. [Govt. of Australia (Reviewer’s comment ID #: 2001-52)]</p>	<p>Unclear what is meant by “specific” climate effects Radiative forcing is treated first because it is a driver for anthropogenic change.</p>
TS-42	A	3:41	4:42	<p>It needs to be made clear in the boxed discussion of uncertainty that 'likelihood' is based on authors' opinions and is not derived through an objective, quantitative process. [Govt. of Australia (Reviewer’s comment ID #: 2001-53)]</p>	<p>Covered in 4:4 to 4:6</p>
TS-43	A	3:41	4:44	<p>Box TS 1.1 is very helpful for policy readers. The authors should consider providing an abbreviated version of this box in the SPM. [Govt. of Australia (Reviewer’s comment ID #: 2001-54)]</p>	<p>Thanks. The key material is now in a footnote in the SPM</p>
TS-44	A	3:41	4:42	<p>Box TS.1.1: This is similar, but not identical to, Box 1.1 in Chapter 1. They should be made the same. I also find them quite confusing, especially the discussion of the distinction of "likelihood" and "confidence". This discussion should be expanded with more examples -- otherwise, I feel, many of the audience will be lost before the game begins. [John Hunter (Reviewer’s comment ID #: 112-29)]</p>	<p>Box 1.1. will be made consistent with Box TS-1. Examples could be problematic both in terms of choices and emphasis. The goal of this box is definitional. See rest of text for content.</p>
TS-45	A	3:41	4:42	<p>It is not obvious that a detailed discussion of confidence vs likelihood should be in the TS. Perhaps the last part of the box could be dropped. [Michael Manton (Reviewer’s comment ID #: 157-45)]</p>	<p>Rejected – both concepts used in the report and their evolution discussed in Chapter 1.</p>
TS-46	A	3:41		<p>Having a set of defined classifications for uncertainties for all parts of the assessment is a valuable addition to the AR4. [Franklin SCHWING (Reviewer’s comment ID #: 230-18)]</p>	<p>Thanks</p>
TS-47	A	3:43	3:43	<p>TS Comment: Replace the sentence “The importance of consistent and transparent treatment of uncertainties is clearly recognised by the IPCC in preparing its assessments of climate change.” with “The importance of consistent and transparent treatment of some uncertainties is clearly</p>	<p>Rejected – the report covers all uncertainties that can be identified through the assessed literature.</p>

				recognised by the IPCC in preparing its assessments of climate change.” because not all uncertainties are considered and some are not mentioned in any IPCC report. [Richard Courtney (Reviewer’s comment ID #: 49-41)]	
TS-48	A	3:51	4:14	This material would have more impact in the reader if the definitions and categories of uncertainties were placed in a bulleted list, or emphasized in some other way. [Govt. of Australia (Reviewer’s comment ID #: 2001-55)]	Rejected. This is descriptive material and not conducive to bullet form.
TS-49	A	3:53	33:53	Replace "external" by "miscellaneous climate" and "anthropogenic" by "human-induced" [VINCENT GRAY (Reviewer’s comment ID #: 88-1970)]	(NB page 33) Rejected – external vs internal factors in the climate system, “anthropogenic” used consistently throughout IPCC reports and supporting literature.
TS-50	A	4:0		It is clearly stated in TS that terms "likelihood" and "confidence" are distinct concepts, however it would be desirable a more accurate definition of the "confidence" concept. Although the different between both concepts is explained, some confusion may appear when assigning a similar range of values to both concepts. On the other hand, another meaning of "confidence" in connexion with model projections appears in page 31 which should require some additional explanation. [Govt. of Spain (Reviewer’s comment ID #: 2019-88)]	Accepted that the distinction between concepts is subtle. The descriptions and definitions here are and have to remain closely consistent with the IPCC Uncertainty Guidance Note used by all WGs. Confidence terminology on page 31 will be changed to match box TS-1.
TS-51	A	4:5	4:6	TS Comment: Replace the phrase “involves expert judgement about the state of that knowledge.” with “is subjective opinion.” because the phrase is inaccurate, misleading and untrue. Indeed, the definition of “structural uncertainties” earlier in the paragraph means that these uncertainties derive from ignorance and not knowledge. (It borders on deception to call opinion of unknown values “expert judgement about the state of that knowledge”.) [Richard Courtney (Reviewer’s comment ID #: 49-42)]	Rejected – “expert judgment” is recognized terminology and widely used and supported in the relevant literature on expert elicitation etc.
TS-52	A	4:8	4:9	Reverse order of clauses in sentence to make it clearer. [Govt. of Australia (Reviewer’s comment ID #: 2001-56)]	Rejected. The logic here requires going from the general to the specific.
TS-53	A	4:8	4:8	Change “science literature” in “scientific literature” [Aristita Busuioc (Reviewer’s comment ID #: 35-2)]	Agreed
TS-54	A	4:18	:19	Regarding the example used to show a very unlikely event (rolling a dice several times and getting a specific combination), as it is written, it not clear enough that it corresponds to an event very unlikely to happen, unless it is stated that in such an experiment to get three times the same number (6) in a row, the dice is rolled only 3 times. [Govt. of Chile (Reviewer’s comment ID #: 2005-14)]	Text clarified
TS-55	A	4:22	4:25	The definitions of “low” and “very low” confidence are decidedly curious. If you give a	Text clarified

				statement with “very low confidence”, you could equally say you have “very high confidence” the statement is incorrect. Reading the context for this in the Uncertainty Guidance Note, we find these last 2 terms (low, very low) should be reserved for special areas of high concern from a risk perspective. Are these terms actually used in this sense anywhere in the IPCC FAR? I realise there are space limitations, but some similar qualification or explanation of these definitions should be provided in the Technical Summary. [A. Brett Mullan (Reviewer’s comment ID #: 182-5)]	
TS-56	A	4:26	4:29	As one problem, I thought this sentence was quite confusing. On a more fundamental level, I think the authors of Chapter 2 really need to rethink using the lexicon for scientific understanding that is used, as in previous years, for radiative forcing. The problem that persists is that there does not seem to be any account taken about either the magnitude of the forcing or the magnitude of its uncertainty and the term used to describe it. This continues to lead to misinterpretations of scientific understanding that confuse the public. Looking at Figure SPM-2, for example, the uncertainty ranges for stratospheric water vapour and for contrail cirrus are a tenth of a W per square meter or less, yet the level of understanding is "Low" whereas the same level of understanding is given for direct aerosol effects when the uncertainty range spans about 0.8 W per square meter. There are other similar contradictions (though I am glad that the level of understanding on solar has been increased). What is really needed is an indication about whether the uncertainty is likely to be significant or not--for aerosols and perhaps land surface it is; for other species, the uncertainties are not really that important--or maybe they are, but it would really help to change that column to something indicating likely relative importance in calculating the overall radiative forcing and the response. At the very least, I do not think the brief referral to this column in the figure caption is adequate for the figure will frequently be shown without any explanation of what is meant--a new column/description needs to be created. [Michael MacCracken (Reviewer’s comment ID #: 152-56)]	Partially taken into account. We will retain LOSU as it accounts properly for a host of factors governing the quality of the method of estimation, and of the estimate itself. Also, consistent with the rest of the document, the 90% confidence interval is being adopted to convey the uncertainty of the estimate. It takes both these measures to do a rigorous assessment of the RF due to different agents. Will simplify the language as much as possible.
TS-57	A	4:27	4:27	TS Comment: Replace the word “Important” with “Some” because the importance – or otherwise – of the achieved progress cannot be known until feedbacks due to clouds are known. An increase to reflective cloud cover of less than 2% would provide a greater negative feedback than the effect of a doubling of carbon dioxide with the maximum possible positive radiative feedback from water vapour. [Richard Courtney (Reviewer’s comment ID #: 49-43)]	(Assumed to refer to page 5) Comment not understood. The significance of cloud feedbacks as noted by the reviewer justifies calling them important.
TS-58	A	4:32	4:35	These spurious "statistical" categories are just self-assessed guesses [VINCENT GRAY (Reviewer’s comment ID #: 88-1901)]	Rejected – the bases for probabilistic assessments of likelihoods are covered in the report.
TS-59	A	4:36	4:39	These two terms may be added to table. [FATEMEH RAHIMZADEH (Reviewer’s comment ID #: 205-2)]	Agreed

TS-60	A	4:38	4:39	The definition for "More likely than not," i.e., more than 50% likelihood, is too nebulous a definition to be useful. Since "likely" is defined as >66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means. [Lenny Bernstein (Reviewer's comment ID #: 20-12)]	Table changed to make it clear that more likely than not means >50% consistent with other ranges that are one sided.
TS-61	A	4:38	4:39	The definition of "More likely than not" i.e., more than 50% likelihood, is vague, since it gives no indication of an upper bound. WG I needs to either provide an upper bound for the term "more likely than not" or not use it. [Jeff Kueter (Reviewer's comment ID #: 137-12)]	See comment TS-60.
TS-62	A	4:38	:39	The definition for "More likely than not" (i.e., more than 50% likelihood) is too nebulous a definition to be useful. Since "likely" is defined as >66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means. [Govt. of United States of America (Reviewer's comment ID #: 2023-876)]	See comment TS-60.
TS-63	A	4:38	:39	The definition of "More likely than not" (i.e., more than 50% likelihood) is vague, since it gives no indication of an upper bound. WG I needs to either provide an upper bound for the term "more likely than not" or not use it. [Govt. of United States of America (Reviewer's comment ID #: 2023-877)]	See comment TS-60.
TS-64	A	4:41	4:42	Using the 95% confidence interval as an uncertainty band is standard scientific practice. There should be no exceptions. [Lenny Bernstein (Reviewer's comment ID #: 20-13)]	Rejected for many reasons. Many different definitions of uncertainty bands are used in the literature. This terminology goes beyond assumptions of normal distributions and covers many situations where 95% intervals can not be provided.
TS-65	A	4:41	4:42	It is good to hear that you sponsor 95% accuracy. But there should be NO exceptions. [VINCENT GRAY (Reviewer's comment ID #: 88-1900)]	See comment TS-64
TS-66	A	4:41	4:42	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. [Jeff Kueter (Reviewer's comment ID #: 137-13)]	See comment TS-64
TS-67	A	4:42	4:42	The general use of 2-sigma appears to be violated more often than not in this report, and in most cases is not reported at all. Care should be taken in better indicating the measure throughout the report. (i.e. exceptions are often not noted in text) It appears that for projections and models, 2-sigma is rarely used if at all. [Haroon Kheshgi (Reviewer's comment ID #: 125-24)]	See comment TS-64. Consistency in reporting uncertainty ranges has been improved.
TS-68	A	4:42		2 σ instead of 2- σ [Govt. of Spain (Reviewer's comment ID #: 2019-89)]	Text changed for other reasons
TS-69	A	5:1	5:9	The text should also point out that there are changes in solar insolation over the course of	Yes, there are variations, but this is not

				the 11 year solar cycle. [Jeff Kueter (Reviewer's comment ID #: 137-14)]	considered significant for the long-term change in solar irradiance. Rejected because this would be misleading in the context here.
TS-70	A	5:1		Section TS.2. The 3 intro paragraphs mention only radiative factors. Non-radiative factors are appended to Section TS.2.3 and as Section TS.2.6. It seems some mention of non-radiative forcing logically should appear in Section TS.2 Intro. [P.C.D. Milly (Reviewer's comment ID #: 179-3)]	Sentence added covering material in section TS2.6
TS-71	A	5:3	5:9	This sentence is inconsistent with the opening sentence of Section TS.3, which recognises the internal variability of the interacting ocean and atmosphere fluids as they mutually transport energy poleward in order to achieve global radiation balance. Modify the paragraph to read: "The Earth's global mean climate is determined by both the incoming energy from the Sun and the properties of the Earth. Thus, while changes in received solar energy (eg., caused by variations in the Earth's orbit around the Sun) inevitably affect the Earth's energy budget, the properties of the atmosphere, the oceans and the land surface, which themselves may be affected by climate feedbacks, are also important. The important roles of feedbacks are evident in the paleoclimatic record of changes in ice sheets and their association with pre-historic climate changes." (The purpose of this last sentence is obscure and could be omitted for brevity and conciseness). [William Kininmonth (Reviewer's comment ID #: 128-1)]	Rejected. Suggested paragraph is consistent with draft.
TS-72	A	5:4	5:4	The phrase 'absorption, and re-emission of energy' implies that only energy absorbed is then emitted by the greenhouse gases and aerosols. Suggest change 're-emission' to 'emission'. [Govt. of Australia (Reviewer's comment ID #: 2001-57)]	Accepted.
TS-73	A	5:5	5:6	This statement should also acknowledge that there are changes in received solar energy due to changes in the Sun's energy output over the 11-year solar cycle. [Lenny Bernstein (Reviewer's comment ID #: 20-14)]	See TS-69
TS-74	A	5:5		Replace "Thus while" with "Although" [Richard Soulen (Reviewer's comment ID #: 248-21)]	Agreed
TS-75	A	5:7		Delete "themselves"? [Richard Soulen (Reviewer's comment ID #: 248-22)]	Agreed
TS-76	A	5:13	5:13	Change sentence to read: "... To increase atmospheric absorption and emission of longwave radiation, ...". This is to give recognition to the fact that the atmosphere actually emits more longwave radiation than it absorbs [William Kininmonth (Reviewer's comment ID #: 128-2)]	Rejected, not relevant to the point being made.
TS-77	A	5:15	5:16	Another important way that the agents differ is in their time history (not just over the season, but over decades). [Michael MacCracken (Reviewer's comment ID #: 152-57)]	The word "seasonal" has been changed to "temporal"
TS-78	A	5:22	5:29	I do not agree with the importance given to "radiative forcing" in this paragraph. It is a measure of a climate change driver, but not the only thing that determines climate response. Basically i see the system as GHGs perturb the energy balance throughout the atmosphere	Rejected. The paragraph tries to distinguish between forcing, feedback and response. The word forcing seems

				and surface (RF measures this), this energy balance change then causes climate to change. This paragraph ignores the importance of surface forcing, for example. I would simply change the first two occurrences of "radiative forcing" to climate change drivers" or something - the third occurrence of RF is fine. [Piers Forster (Reviewer's comment ID #: 73-14)]	the appropriate one in this context.
TS-79	A	5:23	5:24	replace "with the result that each process has to be modelled accurately" by "and these must be taken into account" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-51)]	Text removed for other reasons
TS-80	A	5:24		Replace "While" with "Although" [Richard Soulen (Reviewer's comment ID #: 248-23)]	Agreed
TS-81	A	5:25	5:25	Phrase 'many of its changes' is clumsy and unclear. [Govt. of Australia (Reviewer's comment ID #: 2001-58)]	Text edited
TS-82	A	5:25	5:25	I would suggest changing "many of its" to "all of its important" to better indicate that we seem to have a good handle on this mechanism. [Michael MacCracken (Reviewer's comment ID #: 152-58)]	Text edited
TS-83	A	5:28	5:28	"global mean radiative forcing" is used here. Throughout the rest of the section "global mean is implicitly assumed. I suggest saying up front that unless otherwise stated "radiative forcing" refers to the global mean RF. [Piers Forster (Reviewer's comment ID #: 73-13)]	Agreed. Footnote definition and glossary definition both now note that RF values refer to global means
TS-84	A	5:29	5:29	Phrase 'model evaluations' is unclear and does not seem to link to rest of para. [Govt. of Australia (Reviewer's comment ID #: 2001-59)]	Text has been edited
TS-85	A	5:31		Is the role and impact of water vapor addressed adequately in this section or elsewhere? Since it is the ghg with the highest abundance, and is often cited by "climate skeptics", it should be. For example, should the radiative forcing of water vapor be included in Fig. TS-5 and Table TS-2? [Franklin SCHWING (Reviewer's comment ID #: 230-19)]	Rejected - Water vapor feedback is given quantitatively in section 2.5. Inclusion in Fig 5 or Table 2 would cause confusion with direct forcing agents.
TS-86	A	5:33	5:35	TS Comment: Replace the sentence "The dominant factor in the radiative forcing of climate in the industrial era is the increasing concentration of various greenhouse gases in the atmosphere." with "One factor in the radiative forcing of climate in the industrial era is the increasing concentration of various greenhouse gases in the atmosphere." because the sentence is an assertion which is not supported by any evidence of any kind. Attribution studies demonstrate that the assumption may be correct, but an ability to attribute a cause to an observed change merely demonstrates that the suggested cause is a possible explanation for the change. However, it is important to note that the ability to attribute a cause is not evidence that the attributed cause is responsible for the change in part or in whole. Not considered causes may be responsible. In this case one such not considered cause is the effect of solar eruptive effects on cloud formation (an increase to reflective cloud cover of less than 2% would provide a greater negative feedback than the	Rejected – the statement is not based on attribution but on observed atmospheric and solar changes and well understood mechanisms determining radiative forcing.

				effect of a doubling of carbon dioxide with the maximum possible positive radiative feedback from water vapour). Furthermore, the report itself says that increases in greenhouse gases may not be the dominant cause of positive radiative forcing. It reports that this forcing is $2.9 \pm 0.3 \text{ W m}^{-2}$ total but (as shown in Figure SPM-2) the sum of negative forcings is similar $-1.5 \pm 1.2 \text{ W m}^{-2}$ total with the uncertainties in both totals providing the possibility that the negative forcings may have the larger total magnitude. [2.3] [Richard Courtney (Reviewer's comment ID #: 49-44)]	
TS-87	A	5:33	5:34	Replace "The dominant " by "An important" Changes in the sun may also be important [VINCENT GRAY (Reviewer's comment ID #: 88-1902)]	See comment TS-86
TS-88	A	5:33	5:41	The reader needs better guidance as to where to find the information supporting this statement. Concentration data are in Table 2.1, but radiative efficiency data are in Table 2.14. Adding concentration data to Table TS-2 would solve the problem. [Jeff Kueter (Reviewer's comment ID #: 137-15)]	Table is too large to include further material here. Referencing to appropriate sections will be done.
TS-89	A	5:35	5:35	Insert after "can" "sometimes" [VINCENT GRAY (Reviewer's comment ID #: 88-1903)]	Rejected – Chapters 2 and 7 show that changes in all major GHGs are attributable to human activities
TS-90	A	5:38	5:40	This statement is correct, but finding the information in the underlying report to support it is difficult. Concentration data appears in Table 2.1, while radiative efficiency data appears in Table 2.14. Some guidance to the reader as to where the supporting information can be found would be very helpful. The information presented in Table 2-14 is repeated in Table TS-2. Adding concentrations to this table would provide a full set of information. [Lenny Bernstein (Reviewer's comment ID #: 20-15)]	See comment TS-88
TS-91	A	5:38	5:41	This sentence is confusing. Is this the time-varying changes of each greenhouse gas or the difference between the various greenhouse gases? [William Kininmonth (Reviewer's comment ID #: 128-3)]	Agreed – will clarify.
TS-92	A	5:38	5:38	Is the comparison really based on "ppb"? Or equal mass of emission? [Michael MacCracken (Reviewer's comment ID #: 152-59)]	Radiative efficiency is normalized to mixing ratios (ppb) not emissions so the text is correct as written.
TS-93	A	5:41	5:41	Add at end "Water vapour, which is by far the most important greenhouse gas, varies so greatly that it is very difficult to obtain a plausible average, or a reliable figure of its contribution to greenhouse forcing, either now or in the past. We do not know whether it is increasing or decreasing, or whether humans have an influence. As a stop gap measure we currently treat it as a "feedback" to the effects of carbon dioxide in our models" [VINCENT GRAY (Reviewer's comment ID #: 88-1904)]	Rejected. The relationship between temperature and water vapor is clear, establishing it as a feedback. The report documents the small contribution of direct input by humans. It is not a 'stop gap' but well established physics that is the basis.
TS-94	A	5:43	5:43	Make clear that 'concentrations' refers to present concentrations - not future concentrations. [Govt. of Australia (Reviewer's comment ID #: 2001-60)]	Agreed – will clarify
TS-95	A	5:44	5:44	TS Comment: Replace the phrase "is a function of the history of past emissions"	Rejected – the text is not restricted to

				<p>with “may be influenced by the history of past emissions” While the phrase may be correct for some greenhouse gases (e.g. the CFCs) it is certainly not true for others, notably carbon dioxide (CO₂). Since the TAR, peer reviewed reconsideration of the evidence has shown that human activities are not making a substantial or significant contribution to increasing CO₂ in the atmosphere. The annual pulse of anthropogenic CO₂ into the atmosphere should relate to the annual increase of CO₂ in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)). Also, the annual increase to CO₂ in the atmosphere is the residual of the seasonal changes to CO₂ in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)). Rorsch et al. conclude; “This paper has considered the flows of CO₂ in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO₂ emission to investigate the cause(s) of alteration to atmospheric CO₂ concentration. The considerations of this paper start from the suggestion that the relatively large increase of CO₂ concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased.”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-45)]</p>	<p>anthropogenic emissions so is correct as stands.</p>
TS-96	A	5:47	5:50	<p>Make explicit that the long-lived gases are well mixed in the atmosphere and therefore relatively uniform in their concentrations around the globe. [Govt. of Australia (Reviewer’s comment ID #: 2001-61)]</p>	<p>Accepted. Text added</p>
TS-97	A	5:47	5:49	<p>Change the sentence to read: "... Are chemically stable and the elevated concentrations persist in the atmosphere over time scales ...". Some greenhouse gases, such as CO₂, have a relatively short residence time in the atmosphere (a few years) but the half-life of a change due to anthropogenic emissions will be decades to centuries. [William Kininmonth (Reviewer’s comment ID #: 128-4)]</p>	<p>Text will clarify removal time scales for CO₂.</p>
TS-98	A	5:48	5:49	<p>TS Comment: Delete “carbon dioxide,” because it is certainly not true. Since the TAR, peer reviewed reconsideration of the evidence has shown that human activities are not making a substantial or significant contribution to increasing carbon dioxide (CO₂) in the atmosphere. The annual pulse of anthropogenic CO₂ into the atmosphere should relate to the annual increase of CO₂ in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).</p>	<p>Rejected. There are many lines of observational evidence that anthropogenic emissions are causing CO₂ increase as summarized in FAQ 7.1. The arguments made in Rorsch et al are not accepted as they are inconsistent</p>

				<p>Also, the annual increase to CO₂ in the atmosphere is the residual of the seasonal changes to CO₂ in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)). Rorsch et al. conclude; "This paper has considered the flows of CO₂ in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO₂ emission to investigate the cause(s) of alteration to atmospheric CO₂ concentration. The considerations of this paper start from the suggestion that the relatively large increase of CO₂ concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased. "</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-46)]</p>	with well known carbon cycle processes.
TS-99	A	5:48	5:48	<p>Insert before "carbon" "water vapour" [VINCENT GRAY (Reviewer's comment ID #: 88-1905)]</p>	Rejected – the sentence is about long lived species that are emitted and as covered in Chapter 2 anthropogenic water vapor emissions are not significant.
TS-100	A	5:49	5:49	<p>Replace "long-lived" by "well mixed". It is the latter property which is important for the discussion. [William Kininmonth (Reviewer's comment ID #: 128-5)]</p>	The connection between long lived and well mixed is now explained in the text.
TS-101	A	5:49	5:50	<p>The accurate estimations of global concentrations of LLGHGs are not easy. This sentence should be removed. Although they have long lives, uneven distributions of sources and sinks affect observation in any station. For example, global CO₂ has large latitudinal (and longitudinal) gradients that vary in season. To overcome this situation, many programmes (e.g. Global Atmosphere Watch in WMO) distribute many stations all over the world, and they are not enough even now. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-1)]</p>	The text here is intended to give a general overview and we believe it is well balanced in that respect. No change made.
TS-102	A	5:50	5:50	<p>Insert after "concentrations" "apart from water vapour". Delete "accurately" [VINCENT GRAY (Reviewer's comment ID #: 88-1906)]</p>	Water vapor is not a long lived species so is not relevant here.
TS-103	A	5:50	5:50	<p>Insert after "concentrations"... "tend to be well-mixed and so rather uniform, and" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-52)]</p>	The connection between long lived and well mixed is now explained in the text.
TS-104	A	5:52	5:53	<p>Last sentence is unclear and vague. [Govt. of Australia (Reviewer's comment ID #: 2001-62)]</p>	Text has been replaced with specific statement about CO ₂ persistence in the atmosphere over various timescales based on literature assessed in Ch07.
TS-105	A	5:52	5:53	<p>It is proposed to include a linkage to the underlying mathematical funktion. [Govt. of Austria (Reviewer's comment ID #: 2002-31)]</p>	Rejected – too detailed for this level of text. See also TS-104.

TS-106	A	5:52	5:53	<p>TS Comment: Delete the sentence saying “However, a portion of the anthropogenic enhancement to CO2 concentrations is expected to persist in the atmosphere for thousands of years” because it is certainly not true that those who have considered the matter “expect” any such thing. Indeed, the available evidence does not support the contention that there has been any “anthropogenic enhancement to CO2 concentrations”.</p> <p>Since the TAR, peer reviewed reconsideration of the evidence has shown that human activities are not making a substantial or significant contribution to increasing carbon dioxide (CO2) in the atmosphere. The annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).</p> <p>Also, the annual increase to CO2 in the atmosphere is the residual of the seasonal changes to CO2 in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)).</p> <p>Rorsch et al. conclude; “This paper has considered the flows of CO2 in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO2 emission to investigate the cause(s) of alteration to atmospheric CO2 concentration.</p> <p>The considerations of this paper start from the suggestion that the relatively large increase of CO2 concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased. ”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-47)]</p>	See TS-98.
TS-107	A	5:52	5:52	<p>Do you need to say "small portion"? As it stands it sounds over alarmist - quantifying the portion would be even better.</p> <p>[Piers Forster (Reviewer’s comment ID #: 73-15)]</p>	See comment TS-104
TS-108	A	5:52	5:52	<p>Replace "anthropogenic" with "human-induced"</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-1907)]</p>	Rejected – “anthropogenic” is a standard term in this context
TS-109	A	5:52	5:53	<p>This sentence is qualitative and emotive. The point is better made by the suggested change in comment 4, above. "However, because of the long decay time of any anthropogenic enhancement, elevated concentrations are expected to persist in the atmosphere for thousands of years".</p> <p>[William Kininmonth (Reviewer’s comment ID #: 128-6)]</p>	See TS-104
TS-110	A	6:0	6:	<p>"Figure TS-2 and Fig TS-1 are mixed up. Should be the other way around."</p> <p>[Govt. of Canada (Reviewer’s comment ID #: 2004-114)]</p>	Agreed. Fixed

TS-111	A	6:0		Either Figures TS-1 and TS-2, or the text references to them, appear to have been reversed, making this whole page decidedly confusing.... If so this may sort out some of the following comments... [Govt. of United Kingdom (Reviewer's comment ID #: 2022-53)]	Agreed. Fixed
TS-112	A	6:3	6:3	Replace "atmosphere or by" with "atmosphere, by removal at the surface or by" [William Collins (Reviewer's comment ID #: 45-37)]	text edited to cover this
TS-113	A	6:3	6:3	Replace "sulphate" by "oxides of sulphur". Sulphates are generally aerosols, not gases. [William Kininmonth (Reviewer's comment ID #: 128-7)]	Text corrected
TS-114	A	6:3	6:3	I think carbon monoxide is not removed by washout [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-2)]	Text has been clarified
TS-115	A	6:4		Replace "through" with "by" [Richard Soulen (Reviewer's comment ID #: 248-24)]	Agreed
TS-116	A	6:6		Remove semicolon and change "due to" to "caused by" [Richard Soulen (Reviewer's comment ID #: 248-25)]	Agreed but no semicolon to remove
TS-117	A	6:9	6:50	In this section it would be useful to compare the different radiative forcings using %'s or fractions - eg the radiative forcing due to methane is currently ~1/3 that of CO ₂ , and the forcing of nitrous oxide is about 1/3 that of methane. [Govt. of Australia (Reviewer's comment ID #: 2001-63)]	It is problematic to compare individual terms to the total RF including aerosol because of the large uncertainty in the total. Comparisons to just the better known LLGHG total have been considered but are also confusing when the aerosol terms are considered. Thus there is no simple way of providing this sort of normalization.
TS-118	A	6:11	6:12	TS Comment: Delete the sentence saying "Analyses of modern air samples together with air samples extracted from polar ice cores now provide detailed time series data over 650,000 years for CO ₂ , methane and nitrous oxide see Figure TS-1) [2.3, 6.3, 6.4, 6.5]" because it is an exaggeration that is tantamount to a lie. Air samples extracted from ice cores are inherently incapable of providing "detailed time series" for concentrations of gases they contain. It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firm in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are 'biogenic artefacts'. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO ₂ and the recent rates of change to atmospheric concentration of CO ₂ have repeatedly occurred in recent millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO ₂ concentration and this permits the	Stomatal data have been considered in ch. 6, section 6.3 for the pre-Quaternary periods. Stomatal index proxies for past atmospheric concentration are subject to greater uncertainty than ice core data as they are subject to plant responses to environmental factors other than CO ₂ , and calibration is difficult. Stomatal reconstructions are much more noisy and show more disagreement between records than occurs for ice-core data - again suggesting larger inherent uncertainties. The limitations on temporal resolution due to diffusion in ice cores are well recognized and reflected in this assessment. In contrast to issues with stomatal proxies,

				<p>determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberget al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO₂ changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389</p> <p>This Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric CO₂ concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO₂) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data</p> <p>This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO₂ concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C₃ plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO₂ concentration (16–18). Identification of a CO₂-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO₂ changes at different time scales (14, 17–25).”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-48)]</p>	<p>diffusional effects in ice cores can be corrected for with high confidence and checked through measurements of multiple species in air bubbles. The dating control of ice-cores is also generally more reliable than for recovered leaf material. Although stomatal proxies may provide some additional information, the literature suggests that they are not inconsistent with ice-core data and does not support the interpretation taken by this reviewer.</p>
TS-119	A	6:11	6:35	<p>Looks like the the ref. to Fig TS1 should be TS2 (line 11) and TS2 should be TS1 (line 35) [Reto Knutti (Reviewer’s comment ID #: 133-19)]</p>	Agreed.
TS-120	A	6:12	6:12	<p>Add at end. "However, all these measurements are based on unrepresentative samples. Modern measurements are mostly over oceans, with very few over land. Paleoclimate</p>	Rejected – Chapters support these gases being well mixed and measurements

				results are from a small number of ice cores. As the gas is not "well-mixed" (in contrast to previous claims) the increases claimed are subject to considerable uncertainty" [VINCENT GRAY (Reviewer's comment ID #: 88-1915)]	being globally representative.
TS-121	A	6:12	6:19	Figure TS-1 only goes back 8000 years; you need to refer to Figure TS-2 also. [David Parker (Reviewer's comment ID #: 195-64)]	Figure and text will be adjusted to be consistent
TS-122	A	6:15	6:15	There is a problem with figure numbering, as the reference to figure TS-1 really seems to refer to what is included as figure TS-2. [Michael MacCracken (Reviewer's comment ID #: 152-60)]	Agreed
TS-123	A	6:17	6:26	<p>TS Comment: Delete this entire paragraph because it is not true. Figure TS-1 does seem to show as stated, but it is a misrepresentation based on selective use of data that is tantamount to lies.</p> <p>Figure TS-1 is grossly misleading for two reasons. It stitches together different data sets derived from different sources using different methods, and thus wrongly implies the data sets are directly comparable. And, importantly, it ignores the severe limitations of the ice core data.</p> <p>Air samples extracted from ice cores are inherently incapable of revealing high and low atmospheric concentrations of the gases they contain. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are 'biogenic artefacts'. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO₂ and the recent rates of change to atmospheric concentration of CO₂ have repeatedly occurred in recent millennia.</p> <p>The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO₂ concentration and this permits the determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011-12014, Rapid atmospheric CO₂ changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent</p>	See TS-118

				<p>Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389 This Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric CO₂ concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO₂) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO₂ concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C₃ plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO₂ concentration (16–18). Identification of a CO₂-sensitive gene involved in stomatal development in <i>Arabidopsis thaliana</i> demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO₂ changes at different time scales (14, 17–25).”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-50)]</p>	
TS-124	A	6:17	6:18	<p>Replace "unprecedented in at least' with "thought to be grreater than that of" [VINCENT GRAY (Reviewer’s comment ID #: 88-1908)]</p>	Rejected, no basis for change given.
TS-125	A	6:19	6:19	<p>"in at least the last 20,000 years" as shown in Figure TS-1" is not correct, because in the figure covers only 8000 years proposal: delete: "as shown in Figure TS-1" or write "8000" instead of "20,000" [Govt. of Germany (Reviewer’s comment ID #: 2011-84)]</p>	Agreed – see comment TS-121
TS-126	A	6:19	6:19	<p>Delete "It is very likely that" [VINCENT GRAY (Reviewer’s comment ID #: 88-2115)]</p>	Reject, confidence level is correct
TS-127	A	6:19	6:19	<p>The figure does not show the last 20,000 years as the text indicates, but only the last 8000 years. [Michael MacCracken (Reviewer’s comment ID #: 152-61)]</p>	Agreed – See comment TS-121
TS-128	A	6:19	6:19	<p>Fig TS-1 does not support this statement (Fig TS-2 does in principle but not very convincingly: say that "detailed analysis of the data illustrated in Fig TS-2 shows..." ???) [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-54)]</p>	Figures are transposed in draft – see comment TS-121 etc
TS-129	A	6:19		<p>Fig. TS-1 only goes back ca. 8000 years, which cannot show what happened over the past 20,000 years. [Franklin SCHWING (Reviewer’s comment ID #: 230-20)]</p>	Agreed – See comment TS-121
TS-130	A	6:19		<p>Text says 20,000 years but TS-1 shows only an 8,000 year span</p>	Agreed – See comment TS-121

				[Richard Soulen (Reviewer's comment ID #: 248-26)]	
TS-131	A	6:20	6:20	Delete "well-mixed" They are NOT well-mixed" [VINCENT GRAY (Reviewer's comment ID #: 88-2116)]	Rejected – see comment TS-120
TS-132	A	6:21	6:21	Delete "at present" as you have already said "over the last decade". [David Parker (Reviewer's comment ID #: 195-65)]	Agreed – text changed
TS-133	A	6:24	6:25	To improve the clarity of this statement, it may be modified to “The radiative forcing by LLGHGs of the global mean earth climate system has the highest level of confidence of any forcing agent”. [Govt. of India (Reviewer's comment ID #: 2013-2)]	Agreed
TS-134	A	6:24	6:24	In saying "any similar period", how far back does this go? Perhaps the only faster change might be for a large asteroid impact--or are there other causes? [Michael MacCracken (Reviewer's comment ID #: 152-62)]	Accepted. Will re-word.
TS-135	A	6:25	6:25	Change “the highest level of confidence” in “very high confidence” [Aristita Busuioc (Reviewer's comment ID #: 35-3)]	The text is intended to compare confidence levels rather than use a standard confidence range
TS-136	A	6:25	6:25	Is "of the global mean Earth's climate system" needed here. Radaitve forcing is adquatlyexplained in the preamble? [Piers Forster (Reviewer's comment ID #: 73-12)]	Agreed
TS-137	A	6:25	6:25	Add at end "which is not very impressive" [VINCENT GRAY (Reviewer's comment ID #: 88-1909)]	Rejected – no rationale for comment provided.
TS-138	A	6:29	6:29	Add at end "except water vapour" [VINCENT GRAY (Reviewer's comment ID #: 88-1910)]	Rejected – as explained elsewhere changes in water vapor are almost all due to feedback effects so not covered here.

TS-139	A	6:31	6:34	<p>TS Comment: Delete the sentences saying “Atmospheric CO2 concentration increased by only 20 ppm over 8,000 years prior to industrialisation, and multi-decadal- to-centennial scale variations were less than 10 ppm.” because they are not true. They are a set of misrepresentations based on selective use of data. Simply, they are lies that utilise ice core data without consideration of the limitations of that data and ignore other data that demonstrates the statements are untrue. Air samples extracted from ice cores are inherently incapable of revealing high and low atmospheric concentrations of the gases they contain. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO2 and the recent rates of change to atmospheric concentration of CO2 have repeatedly occurred in recent millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO2 concentration and this permits the determination of past atmospheric CO2 concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO2 concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO2 concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)). The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO2 changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389 THIS FIGURE FLATLY CONTRADICTS THE UNTRUE ASSERTIONS IN LINES 31-34 ON PAGE 6. It compares stomatal data with ice core data from the Taylor Dome for atmospheric CO2 concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO2) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and</p>	See TS-118
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				<p>the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data. This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO2 concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C3 plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO2 concentration (16–18). Identification of a CO2-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO2 changes at different time scales (14, 17–25).”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-51)]</p>	
TS-140	A	6:31	6:31	<p>Change the opening to singular--there is only one atmospheric CO2 concentration (at least on a global average). [Michael MacCracken (Reviewer’s comment ID #: 152-63)]</p>	Accepted
TS-141	A	6:34	6:35	<p>Final sentence (referring to 'since the TAR') is unclear. [Govt. of Australia (Reviewer’s comment ID #: 2001-64)]</p>	Text has been changed to clarify
TS-142	A	6:35	6:35	<p>Reference should be to Figure TS-1 rather than TS-2. [David Parker (Reviewer’s comment ID #: 195-66)]</p>	Accepted. Fixed.
TS-143	A	6:35	6:35	<p>Neither Fig TS-1 nor TS-2 actually supports the final statement re the TAR !! Put the reference to the correct figure on line 33, perhaps ? [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-55)]</p>	See TS-121
TS-144	A	6:37	6:37	<p>This is referring to the wrong figure. [Michael MacCracken (Reviewer’s comment ID #: 152-64)]</p>	Accepted. Fixed

TS-145	A	6:39	6:49	<p>Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the text.</p> <p>[Lenny Bernstein (Reviewer’s comment ID #: 20-16)]</p>	<p>Uncertainties will be revised to be 90% confidence intervals as far as possible.</p>
TS-146	A	6:39	6:49	<p>All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range.</p> <p>[Jeff Kueter (Reviewer’s comment ID #: 137-16)]</p>	<p>See comment TS-145</p>
TS-147	A	6:40	6:43	<p>TS Comment: Delete the sentences and word saying “Atmospheric methane concentrations varied slowly between 750 and 550ppb over the last 11,500 years, but increased by about 1000 ppb in just the last two centuries, representing the fastest changes in at least the last 8,000 years. However,” because they are assertions that cannot be justified from any existing data. The assertions utilise ice core data without consideration of the limitations of that data and ignore other data that demonstrates the ice core data cannot demonstrate whether or not the assertions are true. Air samples extracted from ice cores are inherently incapable of revealing high and low atmospheric concentrations of the gases they contain. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firm in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Importantly, air bubbles travel through the ice with time, and liquid water exists throughout the ice, so high concentrations of methane in the bubbles will dissolve with time. The apparent increase of 1,000 ppb in the last two centuries may be a result of this (the youngest bubbles hhave yet to travel sufficiently to lose their high methane concentration). Indeed, this is the most probable reason for the apparent increase in atmospheric methane concentration since 1750 because there is no known physical reason for such an apparently large change in atmospheric methane concentration throughout the last two centuries. Stomata data indicate the limitations of ice core data for determining past atmospheric concentrations of CO2 but do not suffer from the problems of dissolution, smoothing due to diffusion and removal of “high” values (on the assumption that “high” values are biogenic artefacts). These problems would have similar effects on ice core measurement of CO2 and methane. The stomatal data indicate that the present atmospheric concentration of CO2 and the recent rates of change to atmospheric concentration of CO2 have repeatedly occurred in recent</p>	<p>Rejected. The limitations of ice core data for atmospheric concentrations due to diffusion etc are noted explicitly in this report. No literature reference or other evidence is provided to support the hypothesis advanced by the reviewer (apparently for the first time) that air bubbles travel through ice and change their trace gas concentrations. On the contrary there is an extensive literature documenting the integrity of ice core data on past atmospheric concentrations (see Chapter 6). The reference here to stomatal data is out of context as to the best of our knowledge no one has suggested that stomata respond to methane concentrations in the atmosphere.</p>

				<p>millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO₂ concentration and this permits the determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)). The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO₂ changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389 This Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric CO₂ concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO₂) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data. This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO₂ concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C₃ plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO₂ concentration (16–18). Identification of a CO₂-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO₂ changes at different time scales (14, 17–25).” [Richard Courtney (Reviewer’s comment ID #: 49-53)]</p>	
TS-148	A	6:42	6:42	Refer to Figure TS-2. [David Parker (Reviewer’s comment ID #: 195-67)]	Accepted. Fixed.

TS-149	A	6:44		Change line to: averaged 0.2 ppb yr ⁻¹ (0.01% per yr) for the 6-year period from 1999 to 2005. If the value is really so sensitive to the inclusion of 2005, then some mention of this sensitivity is appropriate. Otherwise, it looks like the report is cherry-picking. [Govt. of United States of America (Reviewer's comment ID #: 2023-878)]	Growth rates over different periods now given for balance.
TS-150	A	6:45	6:45	TS Comment: To avoid being grossly misleading, at the end of this paragraph add the following sentence. "The recent fluctuations in rate of change to atmospheric methane concentration suggest that the atmospheric methane concentration is a function of natural processes." [Richard Courtney (Reviewer's comment ID #: 49-54)]	Rejected. The CH ₄ concentration is much higher than could be supported by natural emissions as explained in chapter 7. The possible reasons recent variability are covered in the text.
TS-151	A	6:45	6:45	Add at end "Its concentration has now been constant for five years, and seems likely to fall" [VINCENT GRAY (Reviewer's comment ID #: 88-1911)]	Rejected – no basis given for expecting a fall.
TS-152	A	6:48	6:49	TS Comment: To avoid being grossly misleading, delete the sentence saying. "Ice core data show that the atmospheric N ₂ O was almost stable for the last 11,500 years, before the onset of the industrial period." The ice core do not "show" that because they are incapable of showing it. However, the ice core data do imply it but only if the limitations of that data are ignored. Air samples extracted from ice cores are inherently incapable of revealing high and low atmospheric concentrations of the gases they contain. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are 'biogenic artefacts'. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data indicate the limitations of ice core data for determining past atmospheric concentrations of CO ₂ but do not suffer from the problems of dissolution, smoothing due to diffusion and removal of "high" values (on the assumption that "high" values are biogenic artefacts). These problems would have similar effects on ice core measurement of CO ₂ and N ₂ O. The stomatal data indicate that the present atmospheric concentration of CO ₂ and the recent rates of change to atmospheric concentration of CO ₂ have repeatedly occurred in recent millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO ₂ concentration and this permits the determination of past atmospheric CO ₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO ₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO ₂ concentration is indicated by	Rejected. See TS-118, but also there are no stomatal proxies for N ₂ O !

				<p>the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagoner F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO2 changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389</p> <p>This Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric CO2 concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO2) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data.</p> <p>This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO2 concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C3 plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO2 concentration (16–18). Identification of a CO2-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO2 changes at different time scales (14, 17–25).”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-55)]</p>	
TS-153	A	6:48	6:49	<p>Change "was almost" to "had been essentially" and change ", before" to "prior to"</p> <p>[Michael MacCracken (Reviewer’s comment ID #: 152-65)]</p>	Text edited
TS-154	A	6:53	7:55	<p>TS Comment: Delete this paragraph because it is untrue. Ice core data are inherently incapable of “demonstrating” that the atmospheric CO2, CH4 and N2O concentrations are higher than in the past 650,000 years. An assertion that they demonstrate what they cannot demonstrate is a lie. And a claim that the assertion “supports understanding” of anything is another lie.</p> <p>Air samples extracted from ice cores are inherently incapable of revealing high and low atmospheric concentrations of the gases they contain. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high</p>	See response to comments TS-118, TS-147 and TS-152 and similar comments by this reviewer

			<p>values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data.</p> <p>Stomata data indicate the limitations of ice core data for determining past atmospheric concentrations of CO₂ but do not suffer from the problems of dissolution, smoothing due to diffusion and removal of “high” values (on the assumption that “high” values are biogenic artefacts). These problems would have similar effects on ice core measurement of CO₂, CH₄ and N₂O.</p> <p>The stomatal data indicate that the present atmospheric concentration of CO₂ and the recent rates of change to atmospheric concentration of CO₂ have repeatedly occurred in recent millennia.</p> <p>The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO₂ concentration and this permits the determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenbergh et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>The importance of this is clearly shown by Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO₂ changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389</p> <p>This Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric CO₂ concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO₂) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data.</p> <p>This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO₂ concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C₃ plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in</p>	
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				<p>ambient CO2 concentration (16–18). Identification of a CO2-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO2 changes at different time scales (14, 17–25).”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-56)]</p>	
TS-155	A	6:55	6:55	<p>Replace "understanding" by "conclusion" (or "inference" if you're deliberately trying to be vague...)</p> <p>[Govt. of United Kingdom (Reviewer’s comment ID #: 2022-56)]</p>	<p>Wording changed to be more specific and reference made to Question 7.1 in main text.</p>
TS-156	A	7:0	7:	<p>Figure TS-3 Comment: Figure TS-3 is grossly misleading. It should be consistent by showing the annual (i.e. not the 5-year average) of the anthropogenic CO2 emissions. It shows</p> <ol style="list-style-type: none"> (1) the ANNUAL increase to atmospheric CO2 concentration, (2) the assumed ANNUAL increase to atmospheric CO2 concentration using the assumption that 100% of the anthropogenic CO anthropogenic CO2 emissions were to stay in the atmosphere, and (3) the 5-YEAR AVERAGE of the anthropogenic CO2 emissions. <p>It is hard to conclude other than this inconsistency is intended to mislead. The annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-62)]</p>	<p>Rejected. The figure does show annual emissions. 5-year averages are an attempt only to show relative constancy of the airborne fraction when year to year variation is averaged out. Both are still there in the figure. See also TS-98.</p>
TS-157	A	7:1	7:1	<p>...fossil fuel combustion'. Clarify that it is change in stocks (not size of reservoirs) that matters.</p> <p>[Govt. of Australia (Reviewer’s comment ID #: 2001-65)]</p>	<p>Rejected. In this context “emissions” is more easily understood than changes of stocks which is normally only used in relation to LUC.</p>
TS-158	A	7:1	7:2	<p>TS Comment: Delete the words “increases with the magnitude of fossil fuel and other emissions such as those from land use change, but also” because they are untrue. There is no evidence to support this suggestion and significant evidence denies it.</p> <p>Since the TAR, peer reviewed reconsideration of the evidence has shown that human activities are not making a substantial or significant contribution to increasing CO2 in the atmosphere. The annual pulse of anthropogenic CO2 into the atmosphere should relate to the annual increase of CO2 in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).</p> <p>Also, the annual increase to CO2 in the atmosphere is the residual of the seasonal changes to CO2 in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and</p>	<p>See responses to TS-156 and TS-98</p>

				<p>increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)). Rorsch et al. conclude; "This paper has considered the flows of CO₂ in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO₂ emission to investigate the cause(s) of alteration to atmospheric CO₂ concentration. The considerations of this paper start from the suggestion that the relatively large increase of CO₂ concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased."</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-57)]</p>	
TS-159	A	7:2	7:2	<p>Insert after "but" "not at the same rate, as it" [VINCENT GRAY (Reviewer's comment ID #: 88-1912)]</p>	Text now removed for other reasons
TS-160	A	7:3	7:3	<p>global fossil fuel emissions' [Govt. of Australia (Reviewer's comment ID #: 2001-66)]</p>	Agreed
TS-161	A	7:3	7:3	<p>Change to: "...global emission due to fossil fuel use, cement production and gas flaring increase from 6.5 to 7.2 GtC/yr." to be consistent with the underlying text (Pg. 2-3, lines 42-43). [Lenny Bernstein (Reviewer's comment ID #: 20-17)]</p>	Fossil carbon dioxide sources now explained in a footnote
TS-162	A	7:3	7:3	<p>I strongly suggest that the emission rates either be reported for years (e.g. 2004) based on data from that year (including estimate of uncertainty) or deleted. Examination of the underlying text shows that this number is (surprisingly) simply an extrapolation of the past trend of CO₂ emissions, not based on data from 2004. If one uses energy statistics and an emission estimation methodology (as was done in the TAR) to estimate recent emissions based on for example BP energy statistics, then one would arrive at a higher estimate (~7.6) for 2004. Additionally, these numbers do not report emissions from cement as has been past practice in IPCC, or an uncertainty. Additionally, past assessments have only highlighted averages (e.g. decade) of emissions which would diminish errors or variability found in a single year's data. Highlighting an extrapolation of emissions in the SPM and TS and presenting it as fact could degrade the quality of this assessment, and I strongly recommend that this be addressed. [Haroon Kheshgi (Reviewer's comment ID #: 125-23)]</p>	Accepted. Will note extrapolation of emissions data to years 2004 and 2005. This is the same procedure as used in the TAR and assumes that the small cement emissions scale with fuel use.
TS-163	A	7:3	7:3	<p>The underlying chapter (Pg. 2-3, lines 42-43) indicates that the emissions cited here are from fossil fuel use, cement production, and gas flaring. Change the text to indicate that these emissions are from all three sources, not just fossil fuel use. [Jeff Kueter (Reviewer's comment ID #: 137-17)]</p>	See comment TS-161
TS-164	A	7:3		<p>Change to: "...global emission due to fossil fuel use, cement production and gas flaring increase from 6.5 to 7.2 GtC/yr" to be consistent with the underlying text (Pg. 2-3, lines 42-</p>	See TS-161

				43). The underlying chapter (Pg. 2-3, lines 42-43) indicates that the emissions cited here are from fossil fuel use, cement production, and gas flaring. Change the text to indicate that these emissions are from all three sources, not just fossil fuel use. [Govt. of United States of America (Reviewer's comment ID #: 2023-879)]	
TS-165	A	7:8	7:8	Leaving' is a loose term - suggest 'removed by ocean/land processes' [Govt. of Australia (Reviewer's comment ID #: 2001-67)]	Text clarified
TS-166	A	7:8	7:12	<p>Table TS-1 Comment: Delete Table TS-1 and its title or replace it with a graph that shows annual data.</p> <p>The Table is grossly misleading because its use of decadal and 5-year totals hides the fact that the annual emissions do not relate to the annual increases of the gases in the air. This concealment amounts to a deception in that it misleads a reader to assume the anthropogenic emissions are causal of the atmospheric increase.</p> <p>The annual pulse of anthropogenic CO₂ into the atmosphere should relate to the annual increase of CO₂ in the atmosphere if one is causal of the other, but their variations greatly differ from year to year. (ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005)).</p> <p>Also, the annual increase to CO₂ in the atmosphere is the residual of the seasonal changes to CO₂ in the atmosphere, and the Northern Hemisphere seasonal changes (decrease and increase) each year are approximately an order of magnitude greater than both the total annual increase and the total annual anthropogenic emission. (Rorsch et al. (2005)).</p> <p>Rorsch et al. conclude; "This paper has considered the flows of CO₂ in and out of the atmosphere. It used the disturbance of the natural cycle by current anthropogenic CO₂ emission to investigate the cause(s) of alteration to atmospheric CO₂ concentration.</p> <p>The considerations of this paper start from the suggestion that the relatively large increase of CO₂ concentration in the atmosphere in the twentieth century (some 30%) is likely to have been caused by the increased mean temperature that preceded it. The main cause is possibly desorption from the oceans with an observed time lag of half a century. However, it cannot be excluded that the production rate from other sources, such as microbiological activity, among others, could have increased. "</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-58)]</p>	Rejected – annually resolved data for the partitioning of the carbon cycle budget is not available. Figure TS-3 already shows the annually resolved net effect in the atmosphere.
TS-167	A	7:8	7:8	This Table makes it impossible to detect trends. We therefore do not know how the carbon cycle is likely to develop in the future [VINCENT GRAY (Reviewer's comment ID #: 88-1913)]	The table shows the data that is available. Subsequent text covers projections of the carbon cycle.
TS-168	A	7:8	7:10	The uncertainties given in this table are 1-sigma, not 2-sigma as recommended only a few pages earlier...and this is not indicated in the caption. Fix. [Haroon Kheshgi (Reviewer's comment ID #: 125-25)]	The uncertainty ranges are now described in the caption.
TS-169	A	7:8	7:11	While gas flaring is technically a fossil fuel use, Table TS-1 should be changed to indicate that the emissions are from fossil fuel use, cement production and gas flaring to avoid any	See TS-161

				misinterpretation and to be consistent with the underlying chapter. [Jeff Kueter (Reviewer's comment ID #: 137-18)]	
TS-170	A	7:8	:11	While gas flaring is technically a fossil fuel use, Table TS-1 should be changed to indicate that the emissions are from fossil fuel use, cement production and gas flaring to avoid any misinterpretation and to be consistent with the underlying chapter. [Govt. of United States of America (Reviewer's comment ID #: 2023-880)]	See TS-161
TS-171	A	7:12	7:12	Point that annual accumulation of CO ₂ in atmosphere seems to have accelerated (see Table TS-1) in recent years (4.1 GtC 2000-2005; 3.2 in 1990s) seems significant. Should receive comment in text (& perhaps in SPM). [Govt. of Australia (Reviewer's comment ID #: 2001-68)]	Accepted, text will mention this.
TS-172	A	7:14	7:14	This sentence is too weak and does not accurately represent the findings in the body of the report. The authors should review this statement to ensure that the level of likelihood is made consistent with the chapter, as it currently appears to be too low. [Govt. of Australia (Reviewer's comment ID #: 2001-69)]	This specific text has been removed for other reasons. The reviewer's concern is dealt with through more specific statements added elsewhere, and added reference to Question 7.1.
TS-173	A	7:14	7:15	TS Comment: Delete the sentence saying "Measurements of carbon isotopes and oxygen provide evidence showing that human activities are responsible for observed CO ₂ increase." because it is a falsehood. The measurements demonstrate that the human emissions mix with the air (which is not surprising) and do not "provide evidence" of anything else except that they can be used (with poor resolution) to partition carbon fluxes. [Richard Courtney (Reviewer's comment ID #: 49-59)]	Text removed for other reasons but the comment is rejected in principle and other text will make it clear that increases in CO ₂ are anthropogenic.
TS-174	A	7:14	7:24	Do you need to say at the start of this bullet that ~30% of our atmospheric emissions are taken up by the biosphere? You see it in the table, but may not be obvious here.. [Piers Forster (Reviewer's comment ID #: 73-16)]	Text is now substantially revised and this point is included.
TS-175	A	7:15		"biosphere and the oceans": does "biosphere" or "oceans" include marine phytoplankton ? "terrestrial and marine ecosystems, soils and oceans" or "land and ocean carbon uptake" (cf. TS-45, line 8) might be better terms to distinguish between biotic/abiotic or between terrestrial/oceanic sinks. This applies also to the rest of the chapter. [Govt. of Germany (Reviewer's comment ID #: 2011-221)]	Marine biosphere has insignificant effect in drawing CO ₂ from the atmosphere as explained in chapter 7 where this level of detail is available. No change made.
TS-176	A	7:15		It is not correct to say that uptake rates to the biosphere are only dependent on CO ₂ concentration and on climate (see also the text itself after line 26 on the same page). The capacity of the biosphere to sequester carbon also depends a lot on the amount and type of vegetation present, which is also very much affected by human activity. [Govt. of Hungary (Reviewer's comment ID #: 2012-10)]	As reviewer points out the issue is dealt with to some extent in the next paragraph and further details have to be left of the chapter.
TS-177	A	7:16	7:18	The numbers in this sentence are not consistent with Table TS-1. For 2000-2005 the ocean uptake was between 1.8 and 2.6 of a total uptake of 2.9 Gt/year; ie, between 62 and 90	The text has been clarified and changed slightly. Consistency with tabulated

				percent of the uptake, not 50-70%. [William Kininmonth (Reviewer’s comment ID #: 128-8)]	values is explained specifically.
TS-178	A	7:19	7:19	Insert after "determined" , "approximately" [VINCENT GRAY (Reviewer’s comment ID #: 88-1914)]	Text has been removed for other reasons.
TS-179	A	7:21	:22	The statement “...CO2 uptake by the ocean is not linked to a corresponding O2 flux” confuses me. This statement seems appropriate for CO2 going into solution in the ocean, but some of the flux of CO2 into the oceans will be associated with photosynthesis, and so will have a compensating O2 flux. A quick look at section 5.4, referenced here, seems to support my understanding, but I remain somewhat confused. [Govt. of United States of America (Reviewer’s comment ID #: 2023-881)]	Text has been removed for other reasons
TS-180	A	7:24	7:24	TS Comment: To avoid being misleading, append the following sentence to the end of the paragraph. “However, this determination is affected by the assumptions it uses and, therefore, the estimates of the different carbon fluxes have very high uncertainty.” [Richard Courtney (Reviewer’s comment ID #: 49-60)]	Rejected - uncertainties are given explicitly.
TS-181	A	7:26	7:26	'Vegetation growth' [Govt. of Australia (Reviewer’s comment ID #: 2001-70)]	Agreed
TS-182	A	7:28	7:28	Move second sentence into a new para. Future releases is a different topic to remainder of para. [Govt. of Australia (Reviewer’s comment ID #: 2001-71)]	Rejected. The reference to future is only part of one sentence the paragraph is intended to summarize the main factors in terrestrial uptake of CO ₂ .
TS-183	A	7:29	7:29	To increase clarity, consider changing, "Thus changes in atmospheric O2 and CO2 can be used ..." to "Thus changes in *the ratio of* atmospheric O2 [and] *to* CO2 can be used ..." [Melinda Marquis (Reviewer’s comment ID #: 162-104)]	Text removed for other reasons
TS-184	A	7:39	7:39	Clarify '.....increasing concentration of CO2 in the atmosphere....' [Govt. of Australia (Reviewer’s comment ID #: 2001-72)]	See TS-187
TS-185	A	7:39	7:39	TS Comment: Delete the word “reliably” because it is misleading (ref. Rorsch A, Thoenes D and Courtney RS, E&E v10 no2 (2005)). [Richard Courtney (Reviewer’s comment ID #: 49-61)]	Rejected – See TS-98
TS-186	A	7:39	7:39	For clarity, consider adding "atmospheric" before "CO2 on terrestrial ..." [Melinda Marquis (Reviewer’s comment ID #: 162-105)]	Agreed
TS-187	A	7:39	7:44	This paragraph understates current knowledge. The sentence in italics remains true, but the following stronger statements are also supported by evidence. (1) Land plant photosynthesis and growth generally increase in response to CO2 levels above ambient. This effect levels off at high concentrations. (2) The CO2 fertilization effect could play a role in maintaining the terrestrial carbon sink so long as it produces a continuous increase in plant growth. The global magnitude of the effect, and the extent to which it is and will be constrained by	Noted, but LAs feel that the CO2 fertilization effect still cannot be quantified reliably – e.g. see the literature assessed in chapter 7. The role of increasing temperature is dealt with in

				nutrient availability, remain controversial. However it is agreed that the effect would not persist into a phase of CO2 stabilization, because soil organic matter decomposition would "catch up", leading to a new equilibrium carbon storage on land. (3) Warming acts against CO2 fertilization because soil organic decays faster at higher temperatures, although the temperature sensitivity of the slowly decaying components is not established experimentally. (4) Models suggest that the combination of these processes will lead to the terrestrial biosphere sink declining and eventually turning into a source. [Iain Colin Prentice (Reviewer's comment ID #: 201-38)]	section 5 consistently with the points made by the reviewer and a cross reference to that section has been added.
TS-188	A	8:5		IAV due to biomass burning is redundant with the previous sentence which already mention fires. [Pierre Friedlingstein (Reviewer's comment ID #: 77-35)]	Text rewritten and addresses this point.
TS-189	A	8:9	8:	Remove these 2 sentences. The first one gives only ocean numbers (why not land, fossil,...) and these numbers are not consistent with table TS-1 anyway. The second sentence is redundant to the beginning of the paragraph which already deals with interannual variability. Furthermore the sentence is wrong as it gives the impression that CO2 IAV is largely driven by oceanic processes. [Pierre Friedlingstein (Reviewer's comment ID #: 77-36)]	The text is about interannual variability and needs to cover both ocean and terrestrial biosphere – but has been clarified to say that IAV is largely due to the latter.
TS-190	A	8:9	8:12	El Niño events result in high temperature anomalies in many areas globally, particularly in the Tropics, which increase CO2 emission from the terrestrial biosphere by the enhanced respiration of plants and decomposition of organic matter in the soil (Keeling et al., 1995). This factor is fallen out. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-3)]	This is now covered through reference to respiration – details have to be left to the chapter.
TS-191	A	8:11	8:11	Expand the acronym "SST" [Govt. of Spain (Reviewer's comment ID #: 2019-78)]	Acronyms will be fixed during copy editing
TS-192	A	8:14	8:14	This figure does not show ENSO variations, per the claim in the text. [Michael MacCracken (Reviewer's comment ID #: 152-66)]	Figure caption edited to cover ENSO years.
TS-193	A	8:16	8:38	Authors should review this paragraph to ensure that it includes the most recent research and analysis as cited in chapter 2. [Govt. of Australia (Reviewer's comment ID #: 2001-73)]	Text has been taken from chapter 2 and 7. No specific suggestions given for change.
TS-194	A	8:16	8:16	Insert after "decreased" "but has been constant for the past four years, and is likely to decline" [VINCENT GRAY (Reviewer's comment ID #: 88-1916)]	First part unnecessary and no basis for projecting a decline given.
TS-195	A	8:16	8:16	It is unclear what the meaning of "average" is in this sentence. Explanation required. [Govt. of Japan (Reviewer's comment ID #: 2014-25)]	Accepted – text no longer uses average.
TS-196	A	8:21	8:21	TS Comment: To be accurate, and to avoid being very misleading, delete the word "well". [Richard Courtney (Reviewer's comment ID #: 49-63)]	Rejected – likely explanations for the growth rate decline and variability are covered in Chapter 7
TS-197	A	8:24	8:24	Change "ppm" to "ppb". [David Parker (Reviewer's comment ID #: 195-68)]	Agreed

TS-198	A	8:25	8:27	<p>TS Comment: To be accurate, and to avoid falsehood, delete the words “, observations and modelling studies show that current methane levels are due to continuing anthropogenic emissions of methane which are greater in magnitude than its natural sources.”.</p> <p>These statements are fabrication that must be deleted.</p> <p>Several paragraphs in Chapter 2 say that reasons for the present variability of atmospheric methane concentration are not known and are mostly – probably entirely – natural (i.e. not anthropogenic). The Chapter admits that the sources and sinks of methane are not known, are not understood, and are varying for reasons that are completely not understood. Hence, the model of Wang et al. (2004) is – and can only be – pure conjecture. Its results are science fiction and not science. And t model of Lassey et al. (2005) is pointless because it assumes “the methane sink remains stable”, but Chapter 2 says these sinks are not known, are not understood, and are varying for reasons that are completely not understood. Hence, the work of Lassey et al. (2005) is not science and it is not even worthy of being described as science fantasy.</p> <p>Considering these facts, it can only be a falsehood for the Technical Summary to allude to them by claiming that “, observations and modelling studies show that current methane levels are due to continuing anthropogenic emissions of methane which are greater in magnitude than its natural sources.”.</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-64)]</p>	<p>Rejected. Comment appears to be based on a misunderstanding of many facts that are already explained in the Technical Summary and in the underlying chapters. Chapters 2 and 7 do not say or imply that sources and sinke are unknown. Independent evidence for stability in sinks is mentioned explicitly. The reasons for interannual variability are not, and do not have to be, the same as the reasons for the much higher concentration now compared to pre-industrial, and the text now makes that point more clearly.</p>
TS-199	A	8:25	8:25	<p>Insert after "declined" , "to zero"</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-1917)]</p>	<p>Rejected – growth rate values are cited in the text and are not zero.</p>
TS-200	A	8:25	8:27	<p>For precision and clarity, consider adding "in large part" before "due to continuing anthropogenic ...". I suggest this because the "current methane levels" are due to both continuing anthropogenic emissions and to nature sources.</p> <p>[Melinda Marquis (Reviewer’s comment ID #: 162-106)]</p>	<p>Rejected. The sentence already mentions both anthropogenic and natural emissions and draws a comparison between them.</p>
TS-201	A	8:26		<p>"anthropogenic emissions of methane which are greater in magnitude than its natural sources". Recent findings of aerobic methane production by land plants should nevertheless be mentioned, if only to show that our understanding of the components of the carbon cycle is increasing. Cite Keppler et al., nature 439: 187 (12.01.2006) Applies also to chapter 2, page 13, line 11ff.</p> <p>[Govt. of Germany (Reviewer’s comment ID #: 2011-222)]</p>	<p>Rejected – inclusion of just some specific parts of the CH₄ budget would be unbalanced and mention of this particular source, relying on one controversial study, is too speculative to justify special mention here.</p>
TS-202	A	8:27	8:28	<p>TS Comment: To avoid a falsehood, replace the words “Individual sources of methane are not as well quantified as total emissions but are mostly biogenic and” with “Anthropogenic emissions of methane”</p> <p>The total sources of methane are estimated by summing the individual methane emissions and, also, by subtracting the estimates of total methane sinks from the change to methane in the air. But Chapter 2 says the methane sources and sinks are not known, are not</p>	<p>Rejected – See TS-198</p>

				understood, and are varying for reasons that are completely not understood. Hence, there is no way to determine whether or not “Individual sources of methane are not as well quantified as total emissions but are mostly biogenic”. This emphasized by the sentence of the TS (p TS-8 lines 31-32 that admits the uncertainties concerning natural sources. [Richard Courtney (Reviewer’s comment ID #: 49-65)]	
TS-203	A	8:27	8:28	how can we know the sum of the emissions better than we know the individual components? Is there any reason to believe the uncertainties are off-setting? [David Rind (Reviewer’s comment ID #: 214-1)]	Sentence added for clarification
TS-204	A	8:28	8:28	Insert after "agriculture", "forests" [VINCENT GRAY (Reviewer’s comment ID #: 88-1918)]	Rejected – no basis given and would be inconsistent with chapter 7
TS-205	A	8:29	8:31	There is inadequate evidence on increase of CH4 emissions from lower latitudes; hence this may not require a specific mention in this summary. It is suggested that part of the line referring to lower latitude emissions may be dropped. [Govt. of India (Reviewer’s comment ID #: 2013-3)]	Accepted this text has been removed.
TS-206	A	8:31	8:31	"Suggest replacing beginning of sentence that reads ""Recent Observations...."" with ""The observed decreased growth rate of methane underscores...."" (that is, avoid vague language when being specific helps the reader make the link.)" [Govt. of Canada (Reviewer’s comment ID #: 2004-115)]	Agreed
TS-207	A	8:32	8:32	I don't understand the meaning of "suggest uncertainties of the impact of global change on natural sources." How does this follow from the previous statement(s)? At the very least, please add "of methane" to the end of the sentence. [Melinda Marquis (Reviewer’s comment ID #: 162-107)]	Accepted – text moved to the next paragraphh for a more appropriate context
TS-208	A	8:34	8:35	is it remarkably large or small interannual variability? [David Rind (Reviewer’s comment ID #: 214-2)]	Text clarified
TS-209	A	8:34	:35	Is it remarkably large or small interannual variability? [Govt. of United States of America (Reviewer’s comment ID #: 2023-882)]	Text clarified
TS-210	A	8:35	8:35	Change “remarkable in “ high” [Aristita Busuioc (Reviewer’s comment ID #: 35-4)]	Accepted
TS-211	A	8:35	8:35	I would suggest deleting "remarkable" [Piers Forster (Reviewer’s comment ID #: 73-17)]	Accepted
TS-212	A	8:41	8:43	"The two sentences here are not really contradictory but they appear so upon a first reading: that the industrial era increase in N2O is PRIMARILY anthropogenic (sentence 1) and that about 47% of total emissions are anthropogenic (sentence 2). Suggest clarifying by noting in sentence 1 that you refer to ATMOSPHERIC N2O and in sentence 2 that the 47% refers to annual (yes/no?) N2O emissions." [Govt. of Canada (Reviewer’s comment ID #: 2004-116)]	Rejected – the sentence makes it clear that it is the increase that is being attributed to anthropogenic emisisions. Not the total atm N ₂ O.

TS-213	A	8:43	8:43	Can analogous percentages of the other main GHG emissions (e.g., for carbon dioxide and for methane) that are anthropogenic be stated in the TS? Here you say that 47% of total N ₂ O emissions are anthropogenic. I don't suppose that merely dividing the ppm of CO ₂ in the year 1750 (~ 260 ppm) by its 2005 value (~380 ppm), to give a figure of 32% is meaningful, eh? [Melinda Marquis (Reviewer's comment ID #: 162-108)]	Unfortunately this can not be done because of the different lifetimes involved.
TS-214	A	9:1	9:2	Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the text. [Lenny Bernstein (Reviewer's comment ID #: 20-18)]	See comment TS-145
TS-215	A	9:1	9:8	This para is still based on our FOD Exec summary bullet. Because of review comments a couple of things changed. A) we no longer discuss the future ozone hole. B) mid lat ozone loss is not referred to as a "depletion" (ozone is more moved around than chemically destroyed). I suggest updating to something based on our latest bullet "Stratospheric ozone is near its minimum level in the satellite observations era, and the magnitude of its RF is expected to decrease in the future. The RF is evaluated to be $-0.03 \pm 0.07 \text{ W m}^{-2}$, weaker than quoted in TAR, with a medium level of scientific understanding. The total concentration of ozone depleting substances has already peaked in the atmosphere and global stratospheric ozone may be beginning to show signs of recovery but is still ~4% below pre-1980 levels. The Antarctic ozone hole still forms every spring and at certain altitudes ozone is completely destroyed. In addition to the chemical destruction of ozone, dynamical changes may have contributed to Northern Hemisphere midlatitude ozone reduction. [Piers Forster (Reviewer's comment ID #: 73-18)]	Accepted
TS-216	A	9:1	9:1	clarify, in regard to what is "the stratospheric ozone is near its minimum level" to the projections? to the observations? [Govt. of Germany (Reviewer's comment ID #: 2011-244)]	Clarified see TS-215
TS-217	A	9:1	9:8	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer's comment ID #: 137-19)]	See comment TS-145
TS-218	A	9:1		Define "satellite observations era" [Richard Soulen (Reviewer's comment ID #: 248-27)]	Clarified see TS-215
TS-219	A	9:2	9:2	Is it the radiative forcing of stratospheric ozone that is estimated to 0.03 W/m ² or the change of radiative forcing relative to pre-80s values ? 0.03 W/m ² seems negligible whereas the ozone layer contributes very significantly to the temperature profile of the	Definition of RF has been clarified as has this text see TS-215

				stratosphere. [Philippe Tulkens (Reviewer's comment ID #: 271-4)]	
TS-220	A	9:6	9:7	replace "dynamical changes" by "changes in stratospheric transport patterns" [Rolf Müller (Reviewer's comment ID #: 181-10)]	Rejected – authors feel original wording more appropriate
TS-221	A	9:7	9:7	replace "ozone depletion" by "ozone change" or "reduction of .. O3 levels"; depletion sounds like implying chemistry as a cause to me. [Rolf Müller (Reviewer's comment ID #: 181-11)]	Accepted see TS-215
TS-222	A	9:10	9:10	An explanation for the asymmetrical uncertainty band needs to be provided. [Lenny Bernstein (Reviewer's comment ID #: 20-19)]	Generic text added to Box TS.1. However, it would be unbalanced to explain the exact origin of asymmetric uncertainties in each case.
TS-223	A	9:10	9:16	Again this is based on our FOD ES, which has been updated in light of review comments. I suggest "Tropospheric ozone RF is estimated to be 0.35 (+0.15/-0.1) W m ⁻² with a medium level of scientific understanding. Observations show that trends in tropospheric ozone during the last few decades vary both in terms of sign and magnitude. There are indications of significant upward trends at low latitudes. Several new chemical transport model studies of the RF due to the increase in tropospheric ozone since preindustrial time exist and have increased complexity compared to models used in TAR." [Piers Forster (Reviewer's comment ID #: 73-19)]	No significant differences to original text – so text unchanged.
TS-224	A	9:10	9:16	Most readers will not be familiar with asymmetrical uncertainty bands. Either explain in the TS why this occurs or provide a reference to the explanation in the underlying report. [Jeff Kueter (Reviewer's comment ID #: 137-20)]	See comment TS-222.
TS-225	A	9:10	9:10	I don't understand why the "+" and "-" values are unequal. [Melinda Marquis (Reviewer's comment ID #: 162-109)]	see TS-222
TS-226	A	9:18	9:18	Delete "Direct emission of water vapour by human activities makes a negligible contribution to radiative forcing". There is no evidence for this statement. [VINCENT GRAY (Reviewer's comment ID #: 88-1919)]	Rejected – the evidence and literature is covered in chapter 2 as referenced at the end of the paragraph
TS-227	A	9:19	9:20	Replace "increase. This" with "may increase, but there is no evidence that it" in line19. Delete "in contrast" on line 20 [VINCENT GRAY (Reviewer's comment ID #: 88-1920)]	Rejected – there is ample justification for setting this context to the following statement
TS-228	A	9:20	9:20	I would change "use" to "emission". Is aviation emission of water vapour really a "use"? [Piers Forster (Reviewer's comment ID #: 73-20)]	Text clarified
TS-229	A	9:20	9:21	Replace "corresponds" with "may possibly correspond" [VINCENT GRAY (Reviewer's comment ID #: 88-1921)]	Rejected – no basis for change given
TS-230	A	9:24	9:24	"Simulations suggest". What about actual measurements? [VINCENT GRAY (Reviewer's comment ID #: 88-1922)]	Measurements give the total water vapor content – and as pointed out the anthropogenic contribution here is a small component whose effect can only

					be gauged via simulations.
TS-231	A	9:25	9:25	"...up to 1%, ..." [delete "a"] [David Parker (Reviewer's comment ID #: 195-69)]	Accepted
TS-232	A	9:26		"...factor of three uncertainty..." as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty consistent across chapters or at least contain a prefacing discussion in the TS. [Govt. of United States of America (Reviewer's comment ID #: 2023-883)]	Text changed for other reasons
TS-233	A	9:29	9:29	Add at end "Changes in water vapour from urban water supply or use may also be important, and there may also be natural changes" [VINCENT GRAY (Reviewer's comment ID #: 88-1923)]	Rejected – no basis for change given
TS-234	A	9:31	9:31	Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the text. [Lenny Bernstein (Reviewer's comment ID #: 20-20)]	See TS-145
TS-235	A	9:31	9:35	This is again our FOD ES bullet, the last sentence should probably be changed to what we now say "The level of scientific understanding is low because the vertical profile of water vapour change is not well known – and the RF is very dependent on this" [Piers Forster (Reviewer's comment ID #: 73-21)]	Para now rewritten based on revised section 2.5.6
TS-236	A	9:31	9:35	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer's comment ID #: 137-21)]	See TS-145
TS-237	A	9:42	9:42	Consider adding a line at the end of this paragraph about removal of CFCs - how, and at what rate (slowly). [Govt. of Canada (Reviewer's comment ID #: 2004-117)]	Text added
TS-238	A	9:46	9:46	"Consider adding a sentence to begin the non-italicized part that explains that ozone is a secondary pollutant formed from chemical precursors. Then the next sentence is clearer to the reader." [Govt. of Canada (Reviewer's comment ID #: 2004-118)]	Text added
TS-239	A	9:49	9:49	Change "much less confidence" in "very low confidence" or "low confidence" (see uncertainty guidance) [Aristita Busuioc (Reviewer's comment ID #: 35-5)]	Accepted
TS-240	A	9:50	9:50	"Add word ""precursor"" before ""emissions"""	Rejected – this has to cover emissions of

				[Govt. of Canada (Reviewer’s comment ID #: 2004-119)]	all species including major greenhouse gases.
TS-241	A	9:53	10:2	"This paragraph about the linkages between trop ozone and air quality and climate change does not even mention climate change. What is the critical link here? Future changes in daytime max temps? Heat waves?" [Govt. of Canada (Reviewer’s comment ID #: 2004-121)]	Original text mentioned climate change in the first sentence?? However para has been revised for clarity
TS-242	A	9:57	9:57	"Replace phrase ""regional stagnation"" with ""stagnation of regional air masses""." [Govt. of Canada (Reviewer’s comment ID #: 2004-120)]	Text changed
TS-243	A	10:7	10:17	"In this introductory paragraph about aerosols - maybe after the 4th sentence - one sentence is recommended to be added about how aerosol properties vary substantially which affects whether they are scattering, absorbing or partially absorbing aerosols." [Govt. of Canada (Reviewer’s comment ID #: 2004-123)]	Accepted
TS-244	A	10:7	10:48	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer’s comment ID #: 137-22)]	See TS-145
TS-245	A	10:7	10:8	how can direct aerosol radiative forcing be described as considerably better understood than in the TAR if it is still given a low level of scientific understanding (the same as it had in TAR)? [David Rind (Reviewer’s comment ID #: 214-3)]	The point is that in the TAR several significant direct terms had a VERY low LOSU. This now clarified.
TS-246	A	10:7	10:8	These two sentences sit unhappily together. Direct aerosol radiative forcing is stated now to be "considerably better understood than in TAR" in the first sentence, but in the second its value is said to be "with a low level of scientific understanding". [Adrian Simmons (Reviewer’s comment ID #: 242-5)]	See TS-245
TS-247	A	10:7	:8	"...now considerably better understood..." and "...low level of understanding" seem contradictory. How would you characterize the level of understanding at the time of TAR? [Govt. of United States of America (Reviewer’s comment ID #: 2023-884)]	See TS-245
TS-248	A	10:7	:8	How can direct aerosol radiative forcing be described as considerably better understood than in the TAR if it is still given a low level of scientific understanding (the same as it had in TAR)? [Govt. of United States of America (Reviewer’s comment ID #: 2023-885)]	See TS-245
TS-249	A	10:8	10:30	Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the	See TS-145

				text. [Lenny Bernstein (Reviewer's comment ID #: 20-21)]	
TS-250	A	10:10	10:19	Is 'black carbon' defined somewhere? [Franklin SCHWING (Reviewer's comment ID #: 230-21)]	No – readers should consult the cited literature for some terms.
TS-251	A	10:13	10:13	Contained' not well understood by policymaker. Suggest 'delivered'. [Govt. of Australia (Reviewer's comment ID #: 2001-74)]	Rejected – several terms used in the report are subject to constraints either observational or theoretic – word needs to be used.
TS-252	A	10:15	10:15	"It would be nice to know what aerosols are formed from biomass burning - even one line to say that a mix of aerosols is formed, some with absorbing properties (or something to that effect). It would also be nice to make a clear point whether the only 'absorbing aerosols' are black carbon aerosols, the rest being reflective." [Govt. of Canada (Reviewer's comment ID #: 2004-122)]	Rejected – any brief explanation as suggested would be potentially misleading given the complexity and variability of biomass burning emissions.
TS-253	A	10:16	10:18	The data referred to on sulfate aerosols from the arctic are indicative of decreases in SO _x emissions from higher latitudes, but not of changes in emissions from South and SE Asia which now dominates the increase in coal use. Suggest that this statement be better defended in the underlying text, or removed. [Haroon Kheshgi (Reviewer's comment ID #: 125-27)]	Statement now more specific and matched to chapter.
TS-254	A	10:23	10:25	The sentence refers to a revision from “strongly negative” but does not say - to what? This requires to be explicitly stated. [Govt. of India (Reviewer's comment ID #: 2013-4)]	Clarified
TS-255	A	10:24	10:24	""...revised from being strongly negative TO SLIGHTLY POSITIVE (add these words) owing to..."" [Govt. of Canada (Reviewer's comment ID #: 2004-124)]	Text edited
TS-256	A	10:24	10:24	Consider clarifying " ... is now revised from being strongly negative owing to better ..." Now revised to what? [Melinda Marquis (Reviewer's comment ID #: 162-110)]	See TS-254
TS-257	A	10:29	10:30	In the SPM this RF is simply referred to as the aerosol indirect effect", maybe we need consistent terminology. Could we call it the cloud albedo effect in the SPM? [Piers Forster (Reviewer's comment ID #: 73-22)]	Accepted SPM text edited
TS-258	A	10:30	10:30	Delete "(" in front of "... low (level..". [Michael Danilin (Reviewer's comment ID #: 55-7)]	fixed
TS-259	A	10:30	10:30	"...very low level..." [delete the odd parenthesis] [David Parker (Reviewer's comment ID #: 195-70)]	fixed
TS-260	A	10:30	10:30	Delete "(" between "low" and "level" [Govt. of Spain (Reviewer's comment ID #: 2019-79)]	fixed
TS-261	A	10:39	10:44	"It is unclear how the RF value in the header of -0.9 ± 0.5 relates to the values given in lines	Text clarified

				39-40 of between -0.2 and -2.0. Does the phrase "overall indirect aerosol effect on clouds" mean something in addition to the cloud-albedo effect is included. Clarify. Also, it's not clear what the numbers in the rest of the paragraph are - and since they don't show up in Figure TS-5, there is no help there." [Govt. of Canada (Reviewer's comment ID #: 2004-125)]	
TS-262	A	10:50	10:50	Additional text as the last par. of "TS2.2 Aerosols": The above changes of the radiation balance were not monotonous in the 20th Century. Surface observations indicated rapid decrease of the global radiation from the 1960s until ca. 1990, often referred as "global dimming", with gradual increase since that time ("global brightening"). These fluctuations should also be considered in relation with real sensitivity of the climate system as well as to the climate change attribution. [also: 2.4, 7.5] [Govt. of Hungary (Reviewer's comment ID #: 2012-11)]	This is dealt with elsewhere – changes in sulfate emissions are explicitly mentioned. Global dimming is covered in section 3.
TS-263	A	10:52	10:54	It would be useful to give the sign of the net radiative forcing from spreading contrails and their effects on nearby cirrus, and the effect of persistent linear contrail cover. [Govt. of Australia (Reviewer's comment ID #: 2001-75)]	Available information insufficient for this, but accepted that this needs to be stated – text edited.
TS-264	A	10:52	10:57	This is an example of where a problem arises due to the special lexicon used in Chapter 2--namely, it is said that this effect is not quantified, implying large uncertainty. Perhaps so, but in the context of the overall issue under discussion in this assessment, the effect is quite small. It would really help to have a lexicon that really can be used to compare relative significance--not just the state of scientific understanding. [Michael MacCracken (Reviewer's comment ID #: 152-67)]	Rejected - Relative significance is given directly by quoting absolute numbers and indirectly in the ordering from more to less significant factors in the text.
TS-265	A	10:52	11:5	Reverse the order of these two paragraphs about cirrus & contrails (much easier to understand the other way around), and then insert "also" after "Aviation may" to clarify the story-line. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-57)]	Rejected – the original ordering from general to specific makes more sense
TS-266	A	10:52		For completeness and to avoid misleading the reader, I suggest stating that other RF effects from aviation emissions are accounted for separately from aerosol and cloud effects. [David Fahey (Reviewer's comment ID #: 66-4)]	This would be too sweeping a statement – no change
TS-267	A	11:2	11:2	I disagree that contrail direct RF is known now with a factor of two uncertainty. This factor of two uncertainty is in fact the spread between the values of 0.006 W/m ² (Marquart et al., 2003) and 0.015 W/m ² (Myhre and Stordal, 2001) reported in Chapter 2. Both these calculations made many assumptions and may severely suffer from our poor knowledge of the relative humidity in the upper troposphere (which is a key parameter in contrail calculations). I think it is better to re-word this sentence as follows: "Persistent linear contrails from global aviation contribute a small radiative forcing of 0.010 W/m ² , with AT LEAST a factor of two uncertainty and a low level of scientific understanding." [Michael Danilin (Reviewer's comment ID #: 55-9)]	Taken into account through mention of low level of scientific understanding.
TS-268	A	11:2	11:5	The beginning of this sentence should be re-written as follows: "This best estimate is a factor of two smaller ...", since the TAR (p.379) gave the best estimate for contrail	Too detailed for such a small term. The current estimates are the important

				radiative forcing of 0.02 W/m ² , which is two times smaller than the value of 0.010 W/m ² in the AR4. The confusion about factor 3-4 came from the IPCC Special Report on Aviation and the Global Atmosphere (1999), which provided the best value of 0.034 W/m ² and was erroneously referred here as TAR. [Michael Danilin (Reviewer's comment ID #: 55-12)]	information.
TS-269	A	11:3		"...factor of two uncertainty..." as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty consistent across chapters or at least contain a prefacing discussion in the TS. [Govt. of United States of America (Reviewer's comment ID #: 2023-886)]	Text revised to avoid this usage.
TS-270	A	11:7	11:7	TS- 7 discussed land use change as a direct source of CO ₂ emissions. Need to make clear at TS-11 but this is not what is being addressed. [Govt. of Australia (Reviewer's comment ID #: 2001-76)]	Text re-ordered for clarification
TS-271	A	11:7	11:22	The distinction between these two paragraphs is not clear to me even after reading them three times. Whatever their messages are, they need to be clarified somehow. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-58)]	Paragraphs re-ordered for clarity. One is about RF and specified RF values the other is about other ways in which land cover affects climate – and says so.
TS-272	A	11:14	11:22	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer's comment ID #: 137-23)]	See TS-145
TS-273	A	11:16	11:19	Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the text. [Lenny Bernstein (Reviewer's comment ID #: 20-22)]	See TS-145
TS-274	A	11:18	11:18	between "...with most net deforestation" and "occurring in temperate regions." write "and decadal scale variations in managed vegetation" see also addition to [CH2 p. 48 line 28] [Govt. of Hungary (Reviewer's comment ID #: 2012-12)]	Sentence removed.
TS-275	A	11:20	11:20	Change "VOLCANOES" to "VOLCANIC ERUPTIONS." My geologist friends insist that volcanoes do not cause climate change, but volcanic eruptions do. -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-4)]	Accepted
TS-276	A	11:24	11:24	Significant' in what respect? [Govt. of Australia (Reviewer's comment ID #: 2001-77)]	Situation too variable to quantify more than this without a lot of detail – no change
TS-277	A	11:24	11:25	I think the phrase "local scales" is inadequate. Having looked back at the SMIC and SCEP	Accepted. Revised wording

				reports, etc., it is pretty clear that there can be important effects over metropolitan areas, megalopolises, etc. Saying just "local scales in urban areas" is really understating the importance of this issue. [Michael MacCracken (Reviewer's comment ID #: 152-68)]	
TS-278	A	11:27		sub-chapter 2.4 sounds as if only sun spots were included in the forcing due to solar parameters. What about the anticipated increase in irradiation due to the "Standard Model" of Sun development? What about parameters concerning the Earth's orbit around the sun and the Earth's own rotation ? If these parameters are --not-- included, we might want to state it (cf. SPM-6, line 20; TS-12, lines 48 and 53). Applies also to chapter 2, page 53, line 7 ff [Govt. of Germany (Reviewer's comment ID #: 2011-223)]	Accepted. Revised wording notes millennial scale of considerations here.
TS-279	A	11:29	11:29	"exists for" should be changed to "spans" [Michael MacCracken (Reviewer's comment ID #: 152-69)]	Will use "covers" - thanks
TS-280	A	11:30	11:30	In that the first sentence uses W per square meter, this sentence should also give that figure and not just the percentage value. [Michael MacCracken (Reviewer's comment ID #: 152-70)]	Disagree the two ways of expressing the variability are comparable.
TS-281	A	11:34	11:34	Change "the total solar irradiance" to "the variation in the total solar irradiance over the" [Michael MacCracken (Reviewer's comment ID #: 152-71)]	Accepted
TS-282	A	11:43	11:43	Change "and a" to "and is associated with a" [Michael MacCracken (Reviewer's comment ID #: 152-72)]	Will use "with" for consistency
TS-283	A	11:50	11:57	I think this bullet paints cosmic rays slightly too negatively. Correlations have not necessarily diminished -especially in light of the new Harrison and Stephenson paper. I would say that "hypotheses.... are not proven" and "correlations are not consistant" or something.. [Piers Forster (Reviewer's comment ID #: 73-23)]	Taken into account. Has been reworded and is consistent with chap. 2 language.
TS-284	A	12:2	12:2	Change "concentrations" to "concentration" or people will wonder if there are multiple types of sulfate aerosols [Michael MacCracken (Reviewer's comment ID #: 152-73)]	Accepted
TS-285	A	12:2		no comment about how volcanic activity in the last 50 years has changed relative to previous (even pre-industrial) time periods; at least could say it is uncertain. [David Rind (Reviewer's comment ID #: 214-4)]	Emphasis of text throughout is the episodic role of volcanic eruptions- long term average data not in chapters. No change
TS-286	A	12:2		There's no comment about how volcanic activity in the last 50 years has changed relative to previous (even pre-industrial) time periods; at least say it is uncertain. [Govt. of United States of America (Reviewer's comment ID #: 2023-887)]	See TS-285
TS-287	A	12:3	12:4	Add a few words explaining how volcanoes perturb climate [Govt. of Australia (Reviewer's comment ID #: 2001-78)]	Covered in subsequent sections
TS-288	A	12:11	12:13	Either provide a basis for this statement or delete it. The statement does not appear in the Executive Summary, Synthesis section, or in the underlying text of Chapter 2, and is not	Background to this statement is now

				intuitively obvious from the material presented in the Technical Summary. [Lenny Bernstein (Reviewer's comment ID #: 20-23)]	included in Ch. 2
TS-289	A	12:11	12:13	What is the basis for the conclusion that net natural forcing has been negative since 1978. This finding does not appear in either the Executive Summary, Synthesis Section, or underlying text of Chapter 2. The TS should not be making new analyses of the information in the underlying report. [Jeff Kueter (Reviewer's comment ID #: 137-24)]	See TS-288
TS-290	A	12:11	:13	Either provide a basis for this statement or delete it. The statement does not appear in the Executive Summary, Synthesis section, or in the underlying text of Chapter 2, and is not intuitively obvious from the material presented in the Technical Summary [Govt. of United States of America (Reviewer's comment ID #: 2023-888)]	See TS-288
TS-291	A	12:13	12:13	I think the reference is section 2.7 and 2.9 [Piers Forster (Reviewer's comment ID #: 73-24)]	Accepted
TS-292	A	12:16		Section TS.2.5 should contain mention of the Annual Greenhouse Gas Index developed by the NOAA/ESRL Global Monitoring Division (formerly Climate Monitoring and Diagnostics Lab, see http://www.cmdl.noaa.gov/aggi/). This sums the radiative forcing due to a variety of long-lived, well-mixed greenhouse gases, and normalizes the total to 1990 values. It has certain limitations and caveats; these should be placed in the main text and cross-referenced in the TS. [Govt. of United States of America (Reviewer's comment ID #: 2023-889)]	This term has not been used in peer reviewed literature so featuring it here would be unwarranted.
TS-293	A	12:19	12:19	Insert after "climate" "but not necessarily activities related to the emission of greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-1924)]	Rejected – no basis for change given
TS-294	A	12:19	12:19	Replave "very likely" with "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2127)]	Rejected – no basis for change given
TS-295	A	12:19	12:24	Relates to a comment made to chapter 2 and also SPM. The point whether the net forcing is greater than zero is not very interesting, and stressing that too much implies that because the net forcing is positive, the whole picture of the observed trends is consistent. Several studies (e.g. Forest Science 2002, Knutti Nature 2002, see Andersen Science 2003 for a summary) have shown that the net forcing must be substantially greater than zero (at least 0.8 W/m ² depending on the method used) to be able to explain the observed warming (taking into account uncertainties in the observed warming, natural forcing, etc.). So I think we should focus more on whether the net forcing is large enough to be consistent with the observed warming than whether it is positive. Suggest something like 'the net forcing is very likely positive, and likely larger than about 0.8W/m ² , the minimum value suggested by inverse methods to be consistent with the observed warming.' [Reto Knutti (Reviewer's comment ID #: 133-20)]	Total RF is now specified with its uncertainty to cover tyhis.
TS-296	A	12:19	12:24	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. However, if this change is not made, the text needs to clearly state that the uncertainty range for RF is +/- one standard deviation. This information must	See TS-145

				accompany each and every use of this limited uncertainty range. [Jeff Kueter (Reviewer's comment ID #: 137-25)]	
TS-297	A	12:19	12:19	Saying "very likely" here greatly understates our confidence in this statement--the increase in GHGs is much greater than of aerosols--by many standard deviations--so I would simply delete "very likely" [Michael MacCracken (Reviewer's comment ID #: 152-74)]	See TS-295
TS-298	A	12:20	12:21	Chapter 2 (Pg 2-3, Footnote 1) indicates that the uncertainty band for radiative forcing estimates is +/- one standard deviation. This should be changed to +/- two standard deviations, but if it is not, this text need to clearly indicate that the uncertainty band is +/- one standard deviation, especially in light of the statement on TS-4, lines 41-42, which states that exceptions from the +/- two standard deviation default value will be noted in the text. [Lenny Bernstein (Reviewer's comment ID #: 20-24)]	See TS-145
TS-299	A	12:21	12:21	Replace "anthropogenic" with 'human-induced' [VINCENT GRAY (Reviewer's comment ID #: 88-2128)]	Rejected – text is standard usage
TS-300	A	12:21	12:21	Revise the magnitude of the total error (Is it 0.4 instead of 0.3?) [Govt. of Spain (Reviewer's comment ID #: 2019-37)]	Value is correct
TS-301	A	12:24	12:24	Replace "very unlikely" with "somewhat improbable" [VINCENT GRAY (Reviewer's comment ID #: 88-2129)]	Text changed for other reasons
TS-302	A	12:24	12:24	Change "very unlikely" to "exceptionally unlikely". [David Parker (Reviewer's comment ID #: 195-71)]	Text has been edited to a clearer form for the TS and bridge to SPM; see chapter for details on the specific point being noted here.
TS-303	A	12:26	12:26	In Fig TS-5, The lack of a figure for the timescale for CO2, and the deeply uninformative comment in the caption are very unhelpful. Surely the figure could say "Centuries (see caption)" and the caption could include a more informative comment, e.g. that from page TS-5, lines 50 to 53, which says it nicely ? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-65)]	The timescales for CO ₂ removal from the atmosphere are now dealt with explicitly in the main text.
TS-304	A	12:32	12:33	I think this conclusion needs to be more carefully stated as it is directly contradicted by the situation vis-à-vis orbital elements, which are mentioned on lines 52-53. That is, orbital elements would be calculated to have near zero radiative forcing but they are viewed as the cause of the glacial cycling. [Michael MacCracken (Reviewer's comment ID #: 152-75)]	Text on this point has been restructured and now takes account of this concern
TS-305	A	12:33	12:33	Replace "are" by "seem to display" [VINCENT GRAY (Reviewer's comment ID #: 88-2130)]	text changed
TS-306	A	12:33	12:34	Add at the end of the sentence"... , although such feedback is partially countered by the enhanced hydrological cycle and increased surface evaporation." [William Kininmonth (Reviewer's comment ID #: 128-9)]	Rejected – the hydrological changes have to be dealt with separately
TS-307	A	12:36	12:36	"GCM" stands for "General Circulation Model". This is a well established technical term,	Accepted. This is consistent with TAR

				and not to be confused with "global climate model" which is more vague and could also be applied to EMICs or energy balance models [Chris Jones (Reviewer's comment ID #: 120-5)]	etc. Text has been checked for consistency with this definition.
TS-308	A	12:36		The text explicitly states that GCM stands for "global climate model" while the glossary says "general circulation model." Pick one. [Govt. of United States of America (Reviewer's comment ID #: 2023-890)]	See TS-307.
TS-309	A	12:37	12:40	Given amount of past public debate over role of water vapour and clouds, it would be valuable for policymakers to have discussion on TS-12, TS-10 and TS-14 that included some material explaining to the policy reader what the processes are concerning water vapour. Current drafting assumes reader has this scientific knowledge already. [Govt. of Australia (Reviewer's comment ID #: 2001-79)]	Text is now less technical but no space for tutorial information.
TS-310	A	12:42		very misleading statement. Orbital variations during the HOLOCENE represent a completely different type of climate forcing, not at all indicative of the ability of models to assess future climate response. Many questions remain about the ability of models to depict the LGM world, or indeed what that climate was really like in the tropics. Large uncertainty exists in how orbital variations, particularly the 100K cycle, could drive ice ages. All of these effects are underplayed in this paragraph. [David Rind (Reviewer's comment ID #: 214-5)]	Text restructured and edited and now deals with this concer. See TS-304 also
TS-311	A	12:42		Very misleading statement. Orbital variations during the HOLOCENE represent a completely different type of climate forcing, not at all indicative of the ability of models to assess future climate response. Many questions remain about the ability of models to depict the LGM world, or indeed what that climate was really like in the tropics. Large uncertainty exists in how orbital variations, particularly the 100K cycle, could drive ice ages. All of these effects are underplayed in this paragraph. [Govt. of United States of America (Reviewer's comment ID #: 2023-891)]	See TS-310
TS-312	A	12:55	12:55	Actually, in footnote 4, it should say "The lapse rate is the rate ..." [Michael MacCracken (Reviewer's comment ID #: 152-76)]	Unnecessary
TS-313	A	13:3	:45	An estimate of the radiative forcing associated with the Milankovitch cycle would put this process in the same context as the radiative forcing associated with anthropogenic processes. [Govt. of United States of America (Reviewer's comment ID #: 2023-892)]	See TS-304 also – point is that globa avg RF is small but seasonal distribution drives climate change slaso.
TS-314	A	13:5	13:45	I don't see the need for a box about orbital-scale changes in a technical summary. [Melissa Free (Reviewer's comment ID #: 76-10)]	Based on other comments seeking clarity of this information we will retain it
TS-315	A	13:10	13:34	On lines 10-11 it says that there is "no comprehensive mechanistic explanation" and then on line 34 it says that the theory is "now well-developed". These two summaries seem in conflict with each other--and in fact the orbital element 3explanation is not readily reconciled with the IPCC paradigm of net global forcing being the cause of climate change. Also, in this box, change "Earth" to "Earth's" on line 22, capitalize "Northern Hemisphere" throughout, and capitalize "earth" on line 44.	Text edited. Concern addressed

				[Michael MacCracken (Reviewer's comment ID #: 152-77)]	
TS-316	A	13:12	13:13	For clarification, modify the sentence to read: "... Given that orbital forcing of annual solar insolation variations in this frequency band is relatively weak." [William Kininmonth (Reviewer's comment ID #: 128-10)]	Accepted
TS-317	A	13:13	13:13	The authors should explain that while the Milankovich theory is well accepted, the figure of 100,000 year periods dominating ice age cycles, remains controversial. [Govt. of Australia (Reviewer's comment ID #: 2001-80)]	Accepted. This is exactly what is meant in the lines 15 to 16.
0-107	A	13:24		TS: it should be noted that although tilt changes do not affect global average insolation, they do impact the absorbed fraction of the insolation since, for example, the Southern Hemisphere contains a larger fraction of oceans which are less reflective than land [Richard Allan (Reviewer's comment ID #: 3-65)]	→ Ch06 Rejected. Too technical for TS. To be discussed partly in Chapter 6.
TS-318	A	13:34	13:36	These two sentences are inconsistent with lines 10 to 13 above, and the explanation is probably wrong. Glacial sheets built over areas of the southern hemisphere concurrently. Cooling of polar regions would inevitable increase the equator to pole temperature gradient and result in increased poleward transport of energy by the atmospheric circulation - a negative feedback. The ice age cycle is about 100,000 years, corresponding to eccentricity variations, and not the precession and obliquity variations as described. [William Kininmonth (Reviewer's comment ID #: 128-11)]	Rejected. Current understanding points to a large role of precession in driving the last glacial inception in the NH (see chapter 6.4.1.7) probably associated with a bipolar cooling and symmetric more active transport of moisture from low to high latitudes (role of obliquity) providing the moisture to build the ice sheets. Several studies point to the role of non linear feedbacks inside the climate system in the generation of 100 000 year ice ages associated with ice sheet and carbon cycle dynamics.
TS-319	A	13:35	13:35	Delete "changes in". [David Parker (Reviewer's comment ID #: 195-72)]	Accepted text changed
TS-320	A	13:35	13:35	This is difficult to follow: suggest that you replace "changes" by "extreme minima", and delete "minima" later in the same line [Govt. of United Kingdom (Reviewer's comment ID #: 2022-59)]	see TS-319
TS-321	A	13:39	13:39	replace "are to a large extent" by "appear to be" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-60)]	Accepted
TS-322	A	13:43	13:45	This is a very important statement that deserves its own paragraph. [Andy Reisinger (Reviewer's comment ID #: 210-66)]	Accepted
TS-323	A	13:43	13:43	Here & elsewhere: search out & replace all statements saying "There is no evidence that..." by something saying something like "Available evidence indicates that" [Because all such statements do not exclude their converse either: if necessary say there is no evidence either way] [Govt. of United Kingdom (Reviewer's comment ID #: 2022-61)]	Accepted
TS-324	A	13:43	13:43	Replace "there is no evidence that" by "Available evidence indicates that" and add "not" after "will" later in the same line.	Accepted

				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-62)]	
TS-325	A	13:44	13:45	Given the statement of page 13, lines 12 to 13 this statement should be qualified as having low scientific understanding! [William Kininmonth (Reviewer's comment ID #: 128-12)]	Rejected. The scientific understanding on NH summer insolation minima leading to the onset of ice ages is not low. This differs from the understanding of the duration of ice ages. The astronomical forcing is well known and no insolation minima is expected in the next 30 kyrs.
TS-326	A	13:45	13:45	Add "or likely longer" at the end (?) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-63)]	It already says "at least" – no change made
TS-327	A	13:47	13:47	Replace "increase confidence" by "indicate" [VINCENT GRAY (Reviewer's comment ID #: 88-2131)]	Rejected – this is not a new finding but an area where confidence has increased
TS-328	A	13:47	13:54	A further reference to Fig TS-5 here would be helpful. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-64)]	Text removed for other reasons
TS-329	A	13:48	13:48	Replace "show" by "suggest" [VINCENT GRAY (Reviewer's comment ID #: 88-2132)]	text removed for other reasons
TS-330	A	13:52	13:52	I would change "realistic" to "most". As aerosol indirect effects are now treated as part of the climate response they need to be included in any efficacy term for the first indirect effect. This makes the first indirect effect efficacy closer to 2.0? [Piers Forster (Reviewer's comment ID #: 73-25)]	text removed for other reasons
TS-331	A	13:54	13:54	Insert "realistic" or "adequate" before "quantitative" (?) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-66)]	text removed for other reasons
TS-332	A	14:4	14:4	Not clear why Kyoto Protocol is referred to here, when exactly same approach is used also for UN Framework Convention on Climate Change. Suggest delete 'under the Kyoto Protocol'. [Govt. of Australia (Reviewer's comment ID #: 2001-81)]	UNFCCC refers to all GHGs, the statement here is intended to refer to GHGs specifically identified in the KP.
TS-333	A	14:15	14:15	I don't think that it is necessary or helpful to include Table TS-2 in the TS chapter. Delete it ? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-67)]	Noted – this is basically an editorial decision to provide easier reference access to GWPs
TS-334	A	14:19	14:22	I would suggest deleting "However" at the start of the sentence, and then combining this sentence with the next by deleting "This comes about" from the start of the next sentence. Then on line 22 change "are affected" to "are also affected" [Michael MacCracken (Reviewer's comment ID #: 152-78)]	Unnecessary
TS-335	A	14:27	14:27	For parallel structure, change "are likely to" to "can" and delete "some" as duplicative. [Michael MacCracken (Reviewer's comment ID #: 152-79)]	Unnecessary
TS-336	A	14:35	14:35	I would suggest changing phrasing of the end of the sentence ending on line 35 to read "land-sue exhibit considerable seasonal and latitudinal variability, contributing to	Proposed statement is not backed by chapter

				uncertainty in estimation of their climatic influences." [Michael MacCracken (Reviewer's comment ID #: 152-80)]	
TS-337	A	14:36	14:38	This statement is inconsistent with the observation that the northern hemisphere is warming faster than the southern hemisphere (see Figure TS-7 - Top) [William Kininmonth (Reviewer's comment ID #: 128-13)]	Cannot directly relate interhemispheric difference in RF to that in warming rates due to differences in ocean heat uptakes
TS-338	A	14:37	14:38	Suggest amendment to clarify: '...very likely exceeds that in the Northern Hemisphere because warming in the latter is dampened by the negative aerosol radiative forcing which is concentrated more in the Northern Hemisphere'. [Govt. of Australia (Reviewer's comment ID #: 2001-82)]	This would confuse warming response with forcing. The explanation for the inter-hemispheric difference is already given
TS-339	A	14:40	14:55	The impact of H ₂ O vapour has not been covered sufficiently (ie. the authors should explain how it effects incoming solar radiation). In addition this point will also need to be included in the SPM. [Govt. of Australia (Reviewer's comment ID #: 2001-83)]	Water vapour feedbacks dealt with explicitly earlier and later.
TS-340	A	14:43	14:43	Change "linking to the hydrologic cycle" to "linking the changes in atmospheric composition to the hydrologic cycle." [Michael MacCracken (Reviewer's comment ID #: 152-81)]	text edited
TS-341	A	14:45	10:48	In this sentence the "very likely" is applicable only to the former part of the sentence i.e. "reduced the global temperature increase during the 20th century" and not to the effects on precipitation and other aspects of the hydrologic cycle which are still uncertain. It is therefore suggested that the sentence may be modified as "In particular, changes in total aerosols have very likely reduced the global temperature increase during the 20th century, but may also have a likely effect on precipitation and other aspects of the hydrologic cycle more strongly than other anthropogenic forcing agents." [Govt. of India (Reviewer's comment ID #: 2013-5)]	Text edited
TS-342	A	14:45	14:48	Somewhere in this sentence it needs to indicate that aerosol loading (including amount, height, location, and lifetimes) varied greatly over the 20th century. [Michael MacCracken (Reviewer's comment ID #: 152-82)]	Unnecessary as we are not considering time evolution of the responses
TS-343	A	14:48	14:48	"Suggest adding to end of italicized sentence: "...through their effects on clouds." (or something like that, assuming this is in fact the mechanism at work)." [Govt. of Canada (Reviewer's comment ID #: 2004-126)]	Linkage to clouds now noted explicitly
TS-344	A	14:48	14:52	Although 'radiative forcing' is the primary tool used for comparing different forcing agents it must also be recognised that 'radiative forcing' does not directly impact on global surface temperature. The net longwave radiation loss of the troposphere is more than an order of magnitude greater than the expected 'radiative forcing' from a doubling of CO ₂ - 100 W/m ² ongoing loss by the troposphere versus 4W/m ² decrease at the top of the atmosphere. The real impact on surface temperature (and the climate response) is the change in back radiation at the surface due to the increase in CO ₂ concentration. This can be calculated. However, as noted, the actual surface temperature response is reduced because of partially compensating increases in conduction and evaporation of energy from the surface to the	Rejected. Chapter 2 provides assessment of many studies demonstrating the quantitative links between RF and global temperature, including updated efficacy studies covering the issues raised.

				atmospheric boundary layer. It is likely that changes in back radiation due to different agents can be calculated as effectively as can 'radiative forcing' be calculated. Suggest delete the sentence: "However, unlike radiative forcing, it cannot be used quantitatively to compare the effects of different agents on the equilibrium global-mean temperature change." [William Kininmonth (Reviewer's comment ID #: 128-14)]	
TS-345	A	14:51	14:51	After "change", insert "because it does not represent the thermal imbalance of the entire climate system" [David Parker (Reviewer's comment ID #: 195-73)]	Taken into account
TS-346	A	15:0		Table TS-2. Is Perchloroethylene (used in dry-cleaning) a greenhouse gas? [David Parker (Reviewer's comment ID #: 195-74)]	Not in Chapter 2 so can not consider for TS
TS-347	A	15:1	15:3	It is not clear from this table if the comparisons are being made based on carbon or CO ₂ -- and given the way these numbers are used in policy and mitigation studies, this has to be made very clear. [Michael MacCracken (Reviewer's comment ID #: 152-83)]	GWPs are defined in the chapter – this is standard usage
TS-348	A	16:46	18:46	Add at end " Globally averaged temperature in the lower troposphere from satellites showed no overall change from 1979 to 1997. Since this region is supposed to be influenced by greenhouse gas increases, the absence of a measurable effect over this period indicates that the influence of greenhouse gas increases must be small. There was a large temperature peak in 1998, attributed to the unusual El Niño ocean event of that year. From 2002 to 2005 the record shows a constant rise above the average which does not indicate a trend, and is difficult to attribute to greenhouse gases when these have been undetectable so far. The Weather balloon record showed no overall temperature rise from 1958 to 2002, with a cool period from 1964 to 1978. A rise has occurred since 2002, but again, could hardly be related to greenhouse gases." [VINCENT GRAY (Reviewer's comment ID #: 88-1929)]	Rejected – inconsistent with recent literature and reviews.
TS-349	A	17:4	17:4	The validity of this formula for the long term should be discussed. It implies that 22 % of the emitted CO ₂ will stay for ever in the atmosphere. Indicate that a more in depth discussion will be presented in the TS page 43, line 15 to 26. An alternative would be to indicate in note (a) that TS2 (not the report) uses the Bern carbon cycle model, without expliciting the formula. [Govt. of France (Reviewer's comment ID #: 2010-120)]	Noted – the formula is consistent with other statements on CO ₂ persistence in the atmosphere throughout the report.
TS-350	A	18:1	18:1	Change "observations of changes" in "observed changes" [Aristita Busuioc (Reviewer's comment ID #: 35-6)]	Keep as is, which seems clearer; also may help reader since we consider here that not everything has changed and have added that now.
TS-351	A	18:1	33:22	The structure of first describing the individual components of the climate, atmosphere, ocean, cryosphere, then the consistency between the components leads to an unnecessarily long and difficult to follow description of Observations of Changes in Climate. What matters to climate is the interaction between the three components (i.e., the consistency	Rejected. While some observations can be used to make physical arguments about consistency, others cannot, requiring that we use the existing

				between the components). Recommend that the pertinent facts from the individual component descriptions be integrated in the consistency discussion. For example, under the heading “Changes in the atmosphere, cryosphere and ocean strongly support the view that the world is warming,” place the information from the three previous sections that support this statement. This would give the reader the pertinent information in one location and obviate the need for continuous referral to the previous sections to find the evidence for this statement. Using this approach would also place the observations into an understandable framework rather than just a list of findings without context. [Govt. of United States of America (Reviewer’s comment ID #: 2023-893)]	structure.
TS-352	A	18:3	18:7	These three sentences are so important in understanding climate variation and change that they should be shifted to and elaborated in the Introductory section TS.1. The fact that the statements come after the discussion of the radiative forcing concepts gives a distorted view of how the climate system and its forcing agents interact. [William Kininmonth (Reviewer’s comment ID #: 128-15)]	Rejected. Introduction covers broader issues and is not intended to get into this level of detail for each of the sections. That occurs when they occur. Believe the point of the radiative forcing section is clear already based upon its own introductory paragraphs and the suggestion is unwarranted.
TS-353	A	18:3	18:4	Some qualification to this sentence is needed to indicate that these variations are within reasonable bounds and that the climate can't be just anything--it is strongly influenced by the various external factors affecting it. [Michael MacCracken (Reviewer’s comment ID #: 152-84)]	Accepted in part. ‘all’ changed to ‘many’
TS-354	A	18:5	18:5	Replace "with its" by "where there is a" and also replace "its" later in the line by "where there is a". [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-68)]	accepted
TS-355	A	18:5		The reference to "excess of radiation" in the tropics and "deficit" in higher latitudes is not meaningful by itself. The tropics experiences warming due to an excess of incoming over outgoing radiation, and higher latitudes experience cooling due to an excess of outgoing over incoming radiation. [Adrian Simmons (Reviewer’s comment ID #: 242-6)]	Rejected; this is implied.
TS-356	A	18:6	18:6	Why is the land SURFACE singled out? A surface has no mass, no water-holding capacity, no heat capacity, etc. A global search of the report for references to "land surface" might be made and, where appropriate, the terminology might be changed. [P.C.D. Milly (Reviewer’s comment ID #: 179-4)]	Land surface changed to land
TS-357	A	18:8	18:8	"," is missing between "However" and "the" [Govt. of Spain (Reviewer’s comment ID #: 2019-80)]	accepted
TS-358	A	18:11	18:11	Changes _below_ the land surface are also documented in the assessment. [P.C.D. Milly (Reviewer’s comment ID #: 179-5)]	Text has been edited
TS-359	A	18:12		Delete second "changes"? [Richard Soulen (Reviewer’s comment ID #: 248-28)]	accepted
TS-360	A	18:24	18:24	Cross-references should be to 3.2, 3.3, 3.4 and 3.8	Accepted

				[David Parker (Reviewer's comment ID #: 195-75)]	
TS-361	A	18:27	18:28	In my opinion, the statement have to be rephrased. There are important atmospheric patterns which arise from internal nonlinear dynamics (NAO/NAM and SAM). One could say that the differential effects on the atmosphere of land and ocean, mountains, and anomalous heating modulate the statistics of these annular patterns occurrences. [Roxana Bojariu (Reviewer's comment ID #: 24-24)]	<i>Text edited</i>
TS-362	A	18:33	18:33	Cross-references should be to 3.5, 3.6 and 3.7 [David Parker (Reviewer's comment ID #: 195-76)]	Accepted
TS-363	A	18:37	18:37	Insert after "record", "according to the upwardly biased compilation of weather station and ship observations [VINCENT GRAY (Reviewer's comment ID #: 88-1925)]	Rejected, no basis given for suggestion
TS-364	A	18:40	18:41	The quoted values of 0.6+/-0.2 C and 0.65+/-0.2 C correspond to linear warming, but not to linear warming TREND. Delete the word ``trend" here. [Michael Danilin (Reviewer's comment ID #: 55-13)]	Disagree. The figures are for a trend and it is important to note that this is computed as the linear trend.
TS-365	A	18:40	18:46	Delete from "The global" on 40 to "years" on 42, [VINCENT GRAY (Reviewer's comment ID #: 88-1926)]	Rejected. No basis given for suggestion
TS-366	A	18:40	18:41	In that the record shows substantial variability, I would not phrase the second sentence as indicating a "linear warming trend"--rather this should say that the increase in temperature over the 20th century was 0.6 plus or minus 0.2--so give the change, not the trend. This will then also fit better with the third sentence. It is also interesting that there are two quite anomalous points around 1880--it would be fascinating to know where the regions are that are so warm--or perhaps one will find that these points are simply not representative due to spatial coverage, etc. [Michael MacCracken (Reviewer's comment ID #: 152-85)]	The warm years 1877-8 resulted from a well-documented major El Niño but it is not practical to call out every El Nno here nor to explain every fluctuation. This is the globally averaged record. Trends discussion has been clarified
TS-367	A	18:40	18:40	0.7 instead of 0.65 [Govt. of Spain (Reviewer's comment ID #: 2019-38)]	Edited
TS-368	A	18:40	18:43	Here and elsewhere, all estimates of linear trends should be stated per unit of time (I.e per year or per decade or (as here) per century, as appropriate), otherwise there can be serious confusion created... [Govt. of United Kingdom (Reviewer's comment ID #: 2022-69)]	Edited
TS-369	A	18:41	18:41	Replace "additional" by "recent" [William Kininmonth (Reviewer's comment ID #: 128-16)]	Rejected. These years are additional warm years, not just recent warm years since e.g., previous 5 years also warm
TS-370	A	18:42	18:43	Replace from "Most" on line 42 to "decade to the minus 1)" on line 43 with "A small cooling took place from 1850 to 1910, followed by a temperature rise from 1910 to 1942 of 0.4°C (0.12°C per decade) . This period had only modest greenhouse gas emissions. The 1910=1942 rise was therefore attributed by the IPCC "Climate Change 1990" to a "recovery from the Little Ice Age", but a more plausible explanation is the growth of cities around the early weather stations. A fall in temperature of 0.05°C from 1943 to 1978 can be attributed to a move of weather stations to airports, This was followed by a rise of 0.45°C from 1978 to	Rejected due to multiple errors of fact. Growth of cities is not the reason for warmth in this record. Interpretation of the 1990 report is incorrect. Misleading to say there has been a fall since 1998; 1998 was warm because of the record ENSO.

				1998 (0.15°C per decade} .Apart from the large El Niño peak in 1999,. this could be explained by a combination of larger cities and energy usage, the shutting down of smaller stations, and the development of airports. A contribution from greenhouse gas increases is difficult to justify, as there was no apparent effect from 1943 to 1978". There has been a fall in temperature since 1998" [VINCENT GRAY (Reviewer's comment ID #: 88-1927)]	
TS-371	A	18:42	18:43	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 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. 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 TS-7 and comment about that figure), 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 #: 152-86)]	See response to SPM-521. Text edited to show trends past century, versus past 50 yrs, etc. 1).The error bounds on SST are not as high in 1942-5 as previously because we did not need to make bucket-corrections so we avoided their uncertainties. There was an increase in data-sparsity related uncertainty but this was moderated on a global scale because there were nonetheless data from most regions except Antarctica, and global temperature anomalies have fewer than 100 degrees of freedom. 2)There was a prolonged El Niño in the early 1940s and the peak in global temperature is very likely to have been real. 3) Point is to show that the early 20th Century warming, even when seen in a most favourable light, was not as strong as the most recent warming. 4) Agreed that we should show the amount of warming over the period.
TS-372	A	18:42		Please elaborate on the phrase "not a good fit to the data" and discuss the warming during the period 1910 to 1945 as recommended in comments on the SPM and Chapter 3. [Govt. of United States of America (Reviewer's comment ID #: 2023-894)]	Text edited regarding the trend and the fit.
TS-373	A	18:43	18:46	Delete from "Three" on line 43 to end. This statement is untrue> The different records are NOT "consistent". . [VINCENT GRAY (Reviewer's comment ID #: 88-1928)]	Rejected. Underlying chapter provides detailed backup.
TS-374	A	18:43		I believe that the reader should be told what happened between 1945 and 1979 also. This would give him or her the complete story and further illustrate the "substantial variability" mentioned earlier	Text edited

				[Richard Soulen (Reviewer's comment ID #: 248-29)]	
TS-375	A	18:48	18:48	See comments regarding Figure TS-7 (page 61) [Michael MacCracken (Reviewer's comment ID #: 152-87)]	See TS-371
TS-376	A	18:50	18:55	The TAR concluded that the urban heat island effect could have affected the global average 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 #: 20-25)]	The 0.12°C is an upper limit, 2 standard deviations, of the uncertainty in global land surface air temperature rise. The lower limit was zero. Brohan et al. (2006) analysis, which we use in Chapter 3, is conservative in retaining an urbanisation-uncertainty term of 0.0055°C/decade since 1900, the same as that used in the TAR.
TS-377	A	18:50	18:51	Replace "Recent studies have shown that affects of urbanisation and land-use change on the land-based temperature record (since 1950) are negligible as far as hmispheric and continental scale averages are concerned" with "McKittrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data" Climate Research Vol 26 pages 159-173, showed that there were significant effects of a number of socioeconomic factors on the data. When the record was corrected for the influence of fuel consumption, populatiuon increase,and defective data, the residual temperture rise was 0.011°C per decade , Comprehensivecorrection can be carried out by a procedure called "homogeneity adjustment" which requires a large number of weather stations. When this procedure is carried out over the continental United States, "global warming" all but disappears.(see Figure 3.2.3 since 1930). A similar study has recently been carried out in China, with the same result (see Zou, Ding, Luo and Wang 2005 Acta Meteorologiica Sinica Vol 19 pages 389 to 400. If "homogeneity adjustment" were applied to the whole set, "global warming" would disappear" [VINCENT GRAY (Reviewer's comment ID #: 88-1930)]	See Ch 3. Urban stations have been removed.
TS-378	A	18:50	18:55	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 #: 137-26)]	See TS-376
TS-379	A	18:50	18:51	Change to read "Recent studies show that the likely effects of urbanization and land use change on land-based temperature record (since 1950) that have not been accounted for are negligible as far as hemispheric- and continental-scale averages are concerned." As it reads now, it seems to be saying that the total effects are negligible, which seems questionable. [Michael MacCracken (Reviewer's comment ID #: 152-88)]	Text edited
TS-380	A	18:51	18:51	Change "are negligible" by "seem to be negligible" [Govt. of Spain (Reviewer's comment ID #: 2019-39)]	Rejected. Studies show that these effects are negligible on the large scales

					referred to.
TS-381	A	18:53	18:53	Change "these effects" to "these effects in the station network that is used" [Michael MacCracken (Reviewer's comment ID #: 152-89)]	Rejected, confusing. Chapter explains in more detail how this is done.
TS-382	A	18:53	18:53	Change "are negligible" by "seem to be negligible" [Govt. of Spain (Reviewer's comment ID #: 2019-40)]	See TS-380
TS-383	A	18:53	18:53	DTR expand the acronym due to is the first time that diurnal temperature range (DTR) appears on the text. This is done in the following page: pg 19, line 2. [Govt. of Spain (Reviewer's comment ID #: 2019-81)]	Accepted
TS-384	A	18:53	19:2	The acronym DTR should be explained at the first instance of its use. [Govt. of Australia (Reviewer's comment ID #: 2001-84)]	accepted
TS-385	A	18:53		Please include a full explanation of DTR (diurnal temperature range) as this abbreviation is used for the first time. [Govt. of Austria (Reviewer's comment ID #: 2002-32)]	accepted
TS-386	A	18:53		DTR to be spelled out and in a table of acronyms? It may also be more sensible to move TS-19, lines 1-3, before this point so that the idea of changes in DTR does not come out of the blue. [Govt. of United States of America (Reviewer's comment ID #: 2023-895)]	DTR spelled out but stays here because we are discussing UHI effects
TS-387	A	18:54	18:55	This sentence does not read well. 'Are' should be deleted in row 54, ie '... but local effects of urban areas, are removed from the land temperature datasets used, and are not relevant...'. [Govt. of Australia (Reviewer's comment ID #: 2001-85)]	Sentence has been clarified
TS-388	A	18:54	18:55	The meaning of this sentence is unclear! [William Kininmonth (Reviewer's comment ID #: 128-17)]	See TS-387
TS-389	A	18:54	:55	This sentence has some crazy syntax. Also why does it contrast urban heat islands to oceanic warming, rather than the more general global-scale warming? [Govt. of United States of America (Reviewer's comment ID #: 2023-896)]	Point is that there is no urban heat island over the ocean. Sentence has been clarified.
TS-390	A	18:55		"not relevant" is awkward at best. I think what is meant here is that urban effects are non-existent over the ocean, but that is also warming. Redo sentence. [Kevin Trenberth (Reviewer's comment ID #: 265-1)]	Language kept similar to that in chapter 3 ES
TS-391	A	19:1	19:3	Is this really an important enough finding, especially given there has been non change for 25 years, to include here? Is it really true that nowhere on Earth have there been such changes over the last 25 years, or is there canceling out going on? [Michael MacCracken (Reviewer's comment ID #: 152-90)]	Spatial extent clarified. Important to retain to clarify difference from TAR
TS-392	A	19:3	19:3	"The result reported on in the last sentence, that DTR did not change over period 1979-2004, could be explained. I presume the lack of change in DTR over the later period is because daytime max temps were increasing as much as nighttime minima were increasing. Section 3.2.2 of Ch. 3 (para 3) seems to say so. " [Govt. of Canada (Reviewer's comment ID #: 2004-127)]	Rejected. By definition if DTR does not change it is because daytime max and nighttime min are changing by the same amount.

TS-393	A	19:5	19:7	Replace "New analysis of radiosonde and satellite measurements of lower tropospheric temperatures now show warming rates that are quantitatively consistent within error bars with the surface temperature record over the periods 1958-2005 and 1979-2005 respectively" with "The satellite and radiosonde records show no overall change between 1979 and 1997. The surface record shows an increase of 0.4°C over this period; .The radiosonde record shows no overall change between 1958 and 2002. The surface record shows a rise of 0.45°C over this period, Both the saterllite and radiosonde records show that there is no evidence of a greenhouse effect in the region where it is supposed to happen from 1979 to 1997 for the satellite record and between 1958 and 2002 for the radiosonde record. The surface record rise between 1978 and 2005 cannot possibly be caused by increases in greenhouse gases. The El Niño peak in 1999 can hardly be claimed to justify an overall "rise" since 1979". [VINCENT GRAY (Reviewer's comment ID #: 88-1931)]	See TS-394
TS-394	A	19:7	19:7	Insert after "(see Figure TS8)" the following." However, there are important differences between the three records The satellite and radiosonde records show no overall change between 1979 and 1997. The surface record shows an increase of 0.4°C over this period; hardly within the range of error bars! .The radiosonde record shows no overall change between 1958 and 2002. The surface record shows a rise of 0.45°C over this period, also, hardly within error bars. The absence of a temperature change in the lower troposphere over such long periods shows that the greenhouse effect, which is supposed to happen in this region, must be negligible. The rise in the surface temperature between 1978 and 2005 must therefore have some other cause " [VINCENT GRAY (Reviewer's comment ID #: 88-1932)]	Rejected. Reviewer is quoting outdated information. Please read ch 3 analysis of surface and upper-air datasets, and their revisions. See also CCSP report and other material referenced in ch 3.
TS-395	A	19:12	19:12	Delete "issues of" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-70)]	accepted
TS-396	A	19:14	19:14	Change MSU estimate of tropospheric" to "MSU-derived estimates of changes in tropospheric" [Michael MacCracken (Reviewer's comment ID #: 152-91)]	accepted
TS-397	A	19:15	19:16	With respect to the troposphere, this overstates our knowledge and goes beyond the conclusions in Chapter 3. [Melissa Free (Reviewer's comment ID #: 76-11)]	Text edited
TS-398	A	19:15	19:15	Insert after "temperatures", "but this is misleading because of the very large peak in the MSU record in 1998 from the El Niño event of that year. If this event is omitted from both records there is no rise in the satellite record until 2001, after which there is a period of constant higher temperature for four years. The surface record, however, shows a steady rise of 0.4°C between 1980 and 2005 which is unaffected by the presence of the El Niño peak in 1998." [VINCENT GRAY (Reviewer's comment ID #: 88-1933)]	Rejected. The surface record certainly does show the ENSO peak in 1998. And the satellite record does not depend upon 1998 for this statement, again see TS-394
TS-399	A	19:16	19:16	"Add to end of sentence: ""..a signature consistent with forcing by GHGs"" (if true, that is.)"	Dealt with in the attribution section

				[Govt. of Canada (Reviewer's comment ID #: 2004-128)]	
TS-400	A	19:19		Add text to the caption of the figure: "The indices are for different satellite data sources". [Govt. of Hungary (Reviewer's comment ID #: 2012-13)]	Rejected – would be misleading as the records are composites in which the differences between satellite data sources have been corrected as far as possible.
TS-401	A	19:22	19:24	It is really strange that the uncertainty (range of estimates) is greatest for the period for which we have the most data. And the final sentence and the figure do not seem to agree as there do not seem to be decreases in temperature except as a result of jumps during volcanic inspired jumps--makes one wonder if calibrations are right. [Michael MacCracken (Reviewer's comment ID #: 152-92)]	The problems in recent years arise from structural uncertainty in the analyses of MSU retrievals: see Chapter 3.4.1 and CCSP (2006). The final sentence refers to the overall change. Chapter 3, page 3-30, notes that a linear trend is a poor representation of the jumps. However the plotted course of temperature is likely to be a real effect of anthropogenic forcings modulated by the volcanic eruptions.
TS-402	A	19:22	19:22	Change "quantitative" to "qualitative" and change 0.6 to 0.75 (to agree with Figure 3.4.3). [David Parker (Reviewer's comment ID #: 195-77)]	Qualitative is accepted. 0.6 is kept because that is what chapter 3 ES has.
TS-403	A	19:24	19:25	Add at the end of the sentence: ", although cooling of the lower stratosphere apparently ceased around 1995 (see Figure TS-8) [William Kininmonth (Reviewer's comment ID #: 128-18)]	See TS-404
TS-404	A	19:24	19:25	It is stated that "The rate of [stratospheric] cooling has been significantly greater since 1979 than between 1958 and 1978". Yet the top panel of FIGURE TS-8 actually shows a slight warming of the lower stratosphere since 1997, which might be noted in the report. I asked colleagues (L.Haimberger and C. Tavolato) with access to the data if they would check the latitudinal structure of this. Both sondes and microwave radiance data indicate continued cooling in the tropics and subtropics, but sharp warming at the poles (which may well be associated with lack of NH stratospheric warmings from 1990-1998, and several occurrences since then, plus one in the SH - see page 3-43). [Adrian Simmons (Reviewer's comment ID #: 242-7)]	Noted. Decreases in ozone depletion could have contributed to this, along with changes in dynamics. We have not space for this degree of detail.
TS-405	A	19:29	19:29	Insert after "Observations". "by the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-1934)]	Accepted, text edited
TS-406	A	19:31	19:31	Add at end "These differences are not shown by the satellite temperature record in the lower troposphere, but this record does show greater variability over land than over the sea." [VINCENT GRAY (Reviewer's comment ID #: 88-1935)]	Rejected. Not relevant here

TS-407	A	19:31	19:31	The TS refers here to land warming in last two decades of 0.25 deg C per decade, but Ch. 3 (page 8, line 26) refers to 0.27 deg C per decade since 1979. Is the difference of 0.02 deg C real, and caused by slightly different start dates of measurement, i.e., 1986 vs. 1979? Or is this accidentally inconsistent? [Melinda Marquis (Reviewer's comment ID #: 162-111)]	Corrected. Now consistent with Ch03
TS-408	A	19:33		The changes in temperature discussed here: warming from 1920-1945 and cooling from 1946-1978 are not visible in the closest referenced figure to this text (Fig. TS-7, lower panel). These changes mentioned in the text here ARE seen later in Fig. TS-26. Perhaps the reader can be directed to the correct figure in this discussion? I am also bothered that these two figures, both showing global temperature, are so different. Why is this so? One possibility is that they refer to different temperatures perhaps? I can't say since NEITHER actually says what temperature is being plotted. Is it at the bottom of the atmosphere in both cases? [Terrence Joyce (Reviewer's comment ID #: 122-7)]	Global data shown in Fig TS-7. Insufficient space for additional graph with zonal break down. Text supported by section 3.2
TS-409	A	19:35	19:38	"The non-italicized sentences here do not adequately follow-through on the last italicized sentence and leave questions in the reader's mind. 1) Why mention only the Arctic? Is the Arctic presented as a region that DOES or DOES not mirror the global pattern - it's hard to tell from the text. 2) Does the Arctic show the pattern of cooling mid-century? 3) The last sentence suggests that because temps were warm in the past as well in the Arctic, we shouldn't assume the current warming trend there will continue. Is this supportable? Don't models project ongoing warming in northern high latitudes? Overall, the message in these last few sentences is convoluted. Perhaps the point to make is that the signal to noise ratio in the Arctic is larger than for many other regions, therefore despite strong recent warming, this is set against a background of high variability. " [Govt. of Canada (Reviewer's comment ID #: 2004-129)]	Text has been edited.
TS-410	A	19:38	19:38	Add at end "The satellite record in the lower troposphere shows a quite different pattern (Figure 3.4.4), with cooling at both poles and most warming above both northern and southern mid-latitudes;" [VINCENT GRAY (Reviewer's comment ID #: 88-1936)]	Rejected. Errors of fact in the comment; also would be misleading. Arctic shows warming. Antarctic cooling is likely linked in part to ozone depletion as noted.
TS-411	A	19:38	19:38	This needs to make clear that the 1920-45 warming was not Arctic wide and that the present Arctic wide warming is very different in spatial extent. The early warming was mainly an Atlantic basin phenomenon, and there are no indications it was Arctic-wide; we just did not have many observations in the other sectors. [Michael MacCracken (Reviewer's comment ID #: 152-93)]	A slightly longer warm period, almost as warm as the present, was observed from 1920-1945, but its geographical extent cannot be proved owing to lack of data. Text edited.
TS-412	A	19:40		however, the more positive west winds have not continued in the 2000s, which sort of invalidates this discussion. [David Rind (Reviewer's comment ID #: 214-6)]	Reject. Statement is longer term than last 5 years.
TS-413	A	19:40		However, the more positive west winds have not continued in the 2000s, which sort of	see TS-412

				invalidates this discussion. [Govt. of United States of America (Reviewer's comment ID #: 2023-897)]	
TS-414	A	19:43	19:45	Does this mean that the area influenced by westerly winds has increased or that the strength of the westerly winds has increased, or both? [William Kininmonth (Reviewer's comment ID #: 128-19)]	Headline of paragraph says strengthening.
TS-415	A	19:43	19:43	Insert "mid-latitude" before "westerly". [David Parker (Reviewer's comment ID #: 195-78)]	accepted
TS-416	A	19:44		About here should also cross reference Fig TS-9 (described later) [Kevin Trenberth (Reviewer's comment ID #: 265-2)]	accepted
TS-417	A	19:52	20:50	I would prefer a de-emphasis on the discussion of "modes", although I realize that this request is going against the prevailing wisdom. To the non-expert, the statement that the extratropical circulation has shifted polewards, with the westerlies strengthening on average, is much more informative than the statement that the NAM is shifted to a positive phase. From a dynamical perspective, I am particularly concerned about statements like "observed changes can be expressed as positive biases in the occurrences of these observed patterns". When one looks at models that produce a poleward shift in response to warming or ozone depletion, the annular variability moves with the mean and is not "positively biased" or skewed about the new climate. I would be happier with this language if the variability were skewed in the warmer climate, indicating that there is a "mode" with some stable existence that does not itself change as the climate changes, with only the phase or amplitude changing. Also, i could not find in Ch. 3 the justification for the statement that trends in eastern N. America, towards cloudier and wetter conditions, have been attributed to changes in PNA/ENSO. [Isaac Held (Reviewer's comment ID #: 105-61)]	Text edited for clarity. Discussion of PNA and N. America is in ch 3.
TS-418	A	19:54	19:55	Phrasing here needs to be improved (in particular, change "and described by the NAO"). The fluctuations are analyzed to indicate the presence of an NAO--the NAO is not somehow independent of the observations and then the variations match it. On line 55, change "importance" to "characteristics" [Michael MacCracken (Reviewer's comment ID #: 152-94)]	Accepted in part. Change from importance to characteristics has been accepted. Do not accept the other suggestion. The NAO is one way to describe the circulation, and this is what the text says. It isn't a unique way.
TS-419	A	19:55	19:55	Add "for an explanation of this and other preferred patterns" after "Box TS 3.1" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-71)]	accepted
TS-420	A	19:57	19:57	Change "positive biases in the occurrence" to "an increased likelihood of the positive phase". [David Parker (Reviewer's comment ID #: 195-79)]	Text has been edited
TS-421	A	20:1	20:13	This whole paragraph is a bit disjointed, skipping around a good bit. Perhaps making it into bullet form would help. [Michael MacCracken (Reviewer's comment ID #: 152-95)]	Rejected; not the format being used in this summary. No explanation given of what the reader finds unclear.
TS-422	A	20:2	20:2	Change "identified with" to something like "evident as a result of" or "a consequence of" or "indicative of"--but not "identified with".	Rejected. Identified indicates that this is one way to describe this. It is not a

				[Michael MacCracken (Reviewer's comment ID #: 152-96)]	cause but a definition
TS-423	A	20:2	20:2	Expand the acronym "SH" [Govt. of Spain (Reviewer's comment ID #: 2019-82)]	accepted
TS-424	A	20:6	20:13	The description of the interaction between EN and lower-frequency climate modes is simplistic and confusing. The phase change in the PDO is not toward more ENs, but creates a spatial pattern (possibly independent of EN processes) that combine with the ENSO cycle to give an appearance of, for example, warmer SSTs during EN events in the positive PDO pahse. The frequency of EN is the same during the positive and negative PDO phases. The text also seems to suggest that the PDO influences North American temperatures through ENSO and PNA teleconnections. [Franklin SCHWING (Reviewer's comment ID #: 230-22)]	<i>Text edited to some degree but see chapter for more on these issues</i>
TS-425	A	20:7	20:7	Replace PDO with Pacific Decadal Oscillation [Govt. of Australia (Reviewer's comment ID #: 2001-86)]	Accepted
TS-426	A	20:7	20:7	"El Niños" is not the correct plural in Spanish for "El Niño" (plural: Los Niños). I suggest replace "El Niños" with "El Niño events" or "El Niño episodes" [Govt. of Spain (Reviewer's comment ID #: 2019-83)]	accepted
TS-427	A	20:12		Capitalize East and West [Kevin Trenberth (Reviewer's comment ID #: 265-3)]	Accepted
TS-428	A	20:15		Suggest to replace "Southern Annual Mode index" before the first appearance of SAM, and then use SAM only later. [Govt. of Hungary (Reviewer's comment ID #: 2012-14)]	accepted
TS-429	A	20:17	20:17	. Figure TS 10 is for the UPPER troposphere only. [VINCENT GRAY (Reviewer's comment ID #: 88-1937)]	Rejected. Both total column and upper trop are shown
TS-430	A	20:18	20:51	Box TS.3.1: Background material such as this should be placed near the start of the report (or appended), not mid-way as this breaks the flow of the TS. [Govt. of Australia (Reviewer's comment ID #: 2001-87)]	Rejected. The material is in a box and so does not break the flow.
TS-431	A	20:20	:50	Some of the information in this box is old and not consistent with discussions later on in the text. For example, in the ocean section, it is frequently stated that the frequency of ENSO events has changed, thus invalidating the preferred time scales for these events given in the table. Similarly, the fact that the NAO may be a component of the NAM as stated in other section should be made in this box. [Govt. of United States of America (Reviewer's comment ID #: 2023-898)]	Rejected. Statement in box is accurate and consistent
TS-432	A	20:27		"with fluctuations elsewhere"??? Where else other than global? Makes no sense. Drop the last bit or clarify. Maybe what is meant that global influences are clear but some vary from event to event so they are not predictable? [Kevin Trenberth (Reviewer's comment ID #: 265-4)]	Accepted
TS-433	A	20:35	20:36	The description of SAM should be more complete and not just refer to NAM. SAM is year round, more so than NAM	Accepted

				[Kevin Trenberth (Reviewer's comment ID #: 265-5)]	
TS-434	A	20:37	:39	Text mentions correlation between NAM and NAO, but not between PNA and ENSO. [Govt. of United States of America (Reviewer's comment ID #: 2023-899)]	See TS-435
TS-435	A	20:42	20:42	Insert the following after the last sentence, "In fact, some researchers regard the PDO as the North Pacific expression of the near global Inter-decadal Pacific Oscillation (IPO)". [Govt. of Australia (Reviewer's comment ID #: 2001-88)]	Too detailed for TS but see chap 3, including text in final two sentences of Box 3.4 in Chapter 3.
TS-436	A	20:46	20:46	Insert "regional" after "amplitude". [David Parker (Reviewer's comment ID #: 195-80)]	accepted
TS-437	A	20:47	20:50	This finishing statement is inconclusive - do the climate models simulate the modes of variability well or not and what are the internal processes? Previous statements identified that the westerly winds have been stronger and thermodynamic theory suggests this strengthening should be associated with a stronger meridional temperature gradient and stronger wind increase with height (jet streams). The stronger meridional temperature gradient is not consistent with greater greenhouse forcing and greater surface temperature warming over high latitudes. Alternatively, a stronger Hadley circulation (inferred elsewhere) generates Relative Atmospheric Angular Momentum (jet streams and surface westerlies), dynamically increases the low troposphere temperature gradient through vertical motions, increases the strength of subtropical anticyclones, and increases the potential for more middle latitude storms - all reported in this Section or Chapter 3. An increased Hadley Circulation and increased poleward transport of energy to warm middle and high latitudes is consistent with the findings of Trenberth and Stepaniak (2004, The flow of energy through the earth's climate system. QJRMS 130 pp 2677-2701) identifying the seamless poleward transport of energy by the Hadley Cells and middle planetary waves and cyclones. The sentence should be changed to read: "It is therefore important that climate models be analysed to determine how well they reproduce these patterns of internal variability and how the patterns are changed by anthropogenic forcing." [William Kininmonth (Reviewer's comment ID #: 128-20)]	Text edited
TS-438	A	20:47	20:47	Change "them" to "these nodes" [Michael MacCracken (Reviewer's comment ID #: 152-97)]	Edited
TS-439	A	20:48	20:51	The final statement does not seem to accurately represent the current predictive strength of climate models. Authors should review this sentence to ensure it accords with the discussions in Chapter 8. [Govt. of Australia (Reviewer's comment ID #: 2001-89)]	Text edited
TS-440	A	20:55		I suppose , it is needed a title. A new and short subject about extreme begins here and there is not any point to it in title TS.3.1.2. It may be solved by adding "extreme to end of the TS. 3.1.2., Or it may be solved by replacing the paragraph(TS-20 line 56 until TS-21 line 13) in page 19 of TS (line 39) that relate to extremes of temperature. [FATEMEH RAHIMZADEH (Reviewer's comment ID #: 205-3)]	Rejected. These extremes are 'related variables' to tempeature and circulation changes, listed already in title TS3.1.2.
TS-441	A	21:1	21:1	It is not clear what box is being referred to here--is it perhaps Box TS-4? [Michael MacCracken (Reviewer's comment ID #: 152-98)]	Don't understand comment. It is box TS-3.4 as stated

TS-442	A	21:15	21:15	Title should include streamflow [P.C.D. Milly (Reviewer's comment ID #: 179-24)]	Rejected. Information on streamflow has been considered but does not merit elevation to the title.
TS-443	A	21:17	21:17	Given that this will be used by policy makers, the source of the increased water vapour in the troposphere should be disclosed (i.e. increased evaporation from the oceans as affected by temperature increase). [Govt. of Australia (Reviewer's comment ID #: 2001-90)]	This is done later, in the section on consistency
TS-444	A	21:17	21:17	A trend cannot be established observationally in the present tense, only in the past tense. "is increasing" should instead read "has increased" A global search and replace for other instances throughout the report seems advisable. [P.C.D. Milly (Reviewer's comment ID #: 179-23)]	Accepted
TS-445	A	21:19	21:19	Here and elsewhere: when giving error bounds on percentages, estimates like 1.2 +/- 0.3% are ambiguous (how is the % error expressed, absolutely or relatively ??). Better to put (1.2 +/- 0.3)% which is unambiguous [Govt. of United Kingdom (Reviewer's comment ID #: 2022-72)]	Agreed
TS-446	A	21:20	21:21	Chapter 3.4 (page 32, lines 28-31) includes the statement: "Since the trends are similar in magnitude to the interannual variability, it is likely that the latter impacts the magnitude of the linear trends. The trends are overwhelmingly positive in spatial structure, but also suggestive of an ENSO influence. The sentence at TS 3.1.3 page 21 lines 20-21 does not convey this fully and should have an additional clause "..... , although the magnitude of interannual variations associated with ENSO are of similar magnitude and impact on the magnitude of the trend." [William Kininmonth (Reviewer's comment ID #: 128-21)]	Text edited
TS-447	A	21:20	21:23	This section is too short for its own good and the accompanying figures are quite misleading. The key points are really two. The first is the increase in precipitable water, as described through line 21, but that should relate to a figure with a pattern and time series (Figure 3.4.5), instead only the time series is shown. A point missing is that this change is extremely important for the hydrological cycle and the water vapour is a resource for precipitation. The second point is the increase in water vapour in the upper troposphere, which while physically linked to the precipitable water and lower tropospheric moisture, is not important for the hydrological cycle but vitally important for the greenhouse effect and radiation feedback. This relates to the rest of the figure which does have a time series and pattern, but the time series covers different times than the one for precipitable water. So the suggestion here is to emphasize these two points and split the figure into the two parts, as in the chapter 3. [Kevin Trenberth (Reviewer's comment ID #: 265-8)]	Partly agreed. Slightly amplified text but the figure is kept as is in view of need for compactness here
TS-448	A	21:20	:21	A sentence or two on how correlations between SST and water vapor support a 4% increase in the latter is needed.	Text edited

				[Govt. of United States of America (Reviewer's comment ID #: 2023-900)]	
TS-449	A	21:27	21:28	This sentence seems out of place here--perhaps put with DTR discussion at top of page 19. [Michael MacCracken (Reviewer's comment ID #: 152-99)]	Rejected. Reasons for the DTR change are not known and this could be misleading. Kept here to link to water vapor and precip and structure of TS (hydrologic cycle section)
TS-450	A	21:27		The statements here on clouds should stay closer to the paragraph on this in the exec summary of chapter 3. [Kevin Trenberth (Reviewer's comment ID #: 265-6)]	Text edited
TS-451	A	21:27	:32	The International Satellite Cloud Climatology Project shows a decrease in cloud amount in the tropics from 1985. The paragraph is very confusing and needs to be rewritten. Is the decrease in DTR related to a change in the diurnal cloud amount? [Govt. of United States of America (Reviewer's comment ID #: 2023-901)]	ISCCP data are discussed in the chapter, along with other cloud data that show other changes. No, changes in cloud but not with diurnal cycle. There has been no change in DTR since 1979.
TS-452	A	21:29	21:32	This statement is inconsistent with the summary paragraph in Chapter 3.4.4.1, which says in part: ".. Although there is independent evidence for decadal changes in TOA forcing over the last two decades the evidence is equivocal. Changes in the planetary and tropical TOA radiative fluxes are consistent with independent ocean heat storage data, and are expected to be dominated by changes in cloud radiative forcing. To the extent that they are real, they may simply reflect natural low-frequency variability of the climate system." In order to make the TS reflect Chapter 3 the wording of line 31 should be changed to: "..... in tropical upper-level cloud cover, and are consistent with changes in the energy budget". [William Kininmonth (Reviewer's comment ID #: 128-22)]	Accepted
0-114	A	21:30		TS: The mean tropical and global changes in top-of-atmosphere radiation from the 1980s to the 1990s have not yet been clearly shown to be related to ENSO; indeed the spatial signature of the changes appears statistically distinct from ENSO. I suggest removal of the phrase: ", possibly related in part to the ENSO phenomena," [Richard Allan (Reviewer's comment ID #: 3-68)]	The reviewer is mistaken. The link to ENSO is in the literature
TS-453	A	21:35	21:35	Would be clearer to change "in sign" to "in sign since 1990" [Michael MacCracken (Reviewer's comment ID #: 152-100)]	accepted
TS-454	A	21:36	21:37	For clarity, change to read "human activities degrades regional air quality and reduces the amount of solar radiation reaching the Earth's surface." (note--capitalize Earth) [Michael MacCracken (Reviewer's comment ID #: 152-101)]	Believe text is clear
TS-455	A	21:39	21:45	The findings of this section are confusing. The heading is 'global dimming' and the discussion is about potential evaporation and actual evapotranspiration as indicators. Pan (potential) evaporation is suggested to have decreased 'in many places' while 'observations in many areas' inferred from water balance exhibits increase due to wetter soils. Elsewhere it is claimed that precipitation over low-latitude land areas has decreased and soils are drier!	Agreed. Sentences deleted

				These lines could be deleted without any loss to the Technical Summary. [William Kininmonth (Reviewer's comment ID #: 128-23)]	
TS-456	A	21:39	21:45	This seems to be a separate point and should likely be a new paragraph. I would also urge including, perhaps as a footnote, an explanation of what "pan evaporation" is and why it is relevant--or maybe say "potential evaporation". [Michael MacCracken (Reviewer's comment ID #: 152-102)]	See comment TS-455. These lines have been deleted.
TS-457	A	21:41	21:45	This summary is inconsistent with 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 do 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 #: 217-5)]	See TS-455
TS-458	A	21:41	:45	Consider separating ideas into more sentences. [Govt. of United States of America (Reviewer's comment ID #: 2023-902)]	See comment TS-455. These lines have been deleted.
TS-459	A	21:47	21:47	What does it mean to say that patterns are emerging. The wording here seems vaguely and presciently to imply a climate-change signal that is on a trajectory to rise above internal variability literally tomorrow. Is this supported by analysis from the cited sections? Or should one simply describe the trends that are observed. If so, is the reader to understand that these are consistent with model simulations, or are they internally generated by the climate system? Suggest hydrologic focus instead on the streamflow results, which have linked model simulations to observations: Milly et al. (2005). [P.C.D. Milly (Reviewer's comment ID #: 179-25)]	Edited
TS-460	A	21:47	22:4	There is no mention of precipitation patterns in the southern hemisphere. Even if there are insufficient observations or studies have been inconclusive - these comments are appropriate to show that SH has been considered. [Govt. of Australia (Reviewer's comment ID #: 2001-91)]	Edited
TS-461	A	21:47	22:4	I also think the description of changes in precipitation should be closer to that in the executive summary of chapter 3. The Figure TS-11 is not one I would have chosen, but rather Fig 3.3.3 if it can be fitted in. [Kevin Trenberth (Reviewer's comment ID #: 265-7)]	OK re text. No room for additional figure.
TS-462	A	21:48	21:49	I think a sentence needs to be inserted between these two sentences that makes the point that these changes in precipitation patterns are a result of changes in atmospheric circulation that are likely being induced by human-induced consequences. I say this because there is general confusion about how global warming can lead to both increases and decreases in precipitation--and that differences indicate that understanding is uncertain--somehow, there needs to be an explanation that we expect both to occur--net global increase, but with complex pattern as atmospheric circulation changes.	Rejected. This section does not deal with attribution at the level indicated (i.e., to global warming).

				[Michael MacCracken (Reviewer's comment ID #: 152-103)]	
TS-463	A	22:1	22:4	On line 1, change "indicates" to "shows". Regarding point made on line 4, the diagram starts in 1979, so the point about changes after 1976/1977 is not evident--in fact it is not even clear a trend is evident from the figure. [Michael MacCracken (Reviewer's comment ID #: 152-104)]	Shows has been accepted. See chapter for discussion of changes over time.
TS-464	A	22:3	22:4	Qualify the sentence by restricting the claim to land areas. "Precipitation over land generally decreased in the deep tropics from 10N to 10S, especially after 1976/77." The hydrological cycle is claimed to have increased with a shift of equatorial precipitation from land to ocean, as is typical during an El Nino event. [William Kininmonth (Reviewer's comment ID #: 128-24)]	Text edited
TS-465	A	22:8	22:12	This section should be deleted because of the demonstrable inadequacy of the statistics. The background material in Chapter 3.8.2.2 has the statement: "Many analyses indicate that the evolution of rainfall statistics through the second half of the 20th century is dominated by variations on the interannual to inter-decadal time scale and that trend estimates are spatially incoherent, as would be anticipated with the relatively high spatial and interannual variability of precipitation". Many locations receive measurable rainfall about one day in three. At best there are only five days per year with daily rainfall above the 95 percentile. A 2 percent per decade increase in rainfall above the 95 percentile represents one additional heavy rainday per decade, which would be difficult to differentiate from chance. [William Kininmonth (Reviewer's comment ID #: 128-25)]	Rejected. Statistics of rainfall have been carefully considered in chapter 3 and appropriate likelihoods assigned. The quoted statement does not pertain to the increases in heavy precip. See Figure TS-10.
TS-466	A	22:9	22:12	Somehow, these points need to more clearly say that the changes are evident with respect to some base period (though, of course, our data from base periods is also contaminated by human influences). [Michael MacCracken (Reviewer's comment ID #: 152-105)]	Changes are since about 1980 as shown in the figure. See Figure caption for base period.
TS-467	A	22:14	22:14	Figure TS-12 is incomplete the lower figure is missing [Govt. of Germany (Reviewer's comment ID #: 2011-245)]	Caption and figure have been corrected
TS-468	A	22:16	22:30	The heading here on line 16 is a bit misleading. My suggestion is to combine these two bullets and the two headings. The problem is that I don't know of any good reason why there ought to be an increase in numbers of TCs: the theory suggests that there ought to be an increase in activity, but one big storm has more "action" than two small ones, and so it is not just numbers but also intensity, duration and size. The TS can go a long way to help educate on what the expectations ought to be in this regard. On line 29 the reference to the figure TS-13 is in the wrong place (it is a figure on SST not storm intensity). [Kevin Trenberth (Reviewer's comment ID #: 265-9)]	Text edited
TS-469	A	22:21	:22	Variations in the number of tropical cyclones have been shown to be dominated by multidecadal variability and not decadal variability (e.g., Goldenberg et al.) [Govt. of United States of America (Reviewer's comment ID #: 2023-903)]	Rejected. Chapter discusses the evidence for changes beyond decadal variability in detail.
TS-470	A	22:26	22:26	Change "there is evidence" with standard terminology of uncertainty (see uncertainty guidance) [Aristita Busuioc (Reviewer's comment ID #: 35-7)]	Considerable uncertainty precludes a more quantitative statement on this.

TS-471	A	22:26	22:30	What is the basis for the claim that there is a trend towards longer lifetime and greater storm intensity? Figure TS-13 supports a finding that sea surface temperature has been rising, but no more. [Jeff Kueter (Reviewer's comment ID #: 137-27)]	See chapter for more detail.
TS-472	A	22:26	:27	Please clarify the phrase "there are concerns about the quality of the historical data" since the phrase can apply to both the pre-satellite and satellite era. There are reasonable concerns about changes in the satellite data used to classify hurricane strength. [Govt. of United States of America (Reviewer's comment ID #: 2023-904)]	Text has been edited
TS-473	A	22:29	22:29	Figure TS-13 shows evidence of increasing sea surface temperature, but not of " ... a trend towards longer lifetimes and greater storm intensity." [Lenny Bernstein (Reviewer's comment ID #: 20-26)]	Text edited
TS-474	A	22:29	22:29	"The reference here to what TS-13 should show does not seem accurate since TS-13 shows trend in SST rather than info on storm characteristics, as the text suggests. " [Govt. of Canada (Reviewer's comment ID #: 2004-130)]	Text edited
TS-475	A	22:29	22:29	"(see Figure TS-13)"should be moved to the end of next sentence, line 30 [Govt. of France (Reviewer's comment ID #: 2010-121)]	Text edited along the lines suggested
TS-476	A	22:29	22:29	Figure TS-13 appears to be of SST, and not what the text says this plot shows. [Michael MacCracken (Reviewer's comment ID #: 152-106)]	Text edited
TS-477	A	22:29		What is the basis for the claim that there is a trend towards longer lifetime and greater storm intensity? Figure TS-13 supports a finding that sea surface temperature has been rising, but no more. [Govt. of United States of America (Reviewer's comment ID #: 2023-905)]	See chapter for details.
TS-478	A	22:34	22:36	Rather than using the phrase 'nature of high temperatures' include the phrase 'increased stress due to high temperatures and heat waves' - carry more information. [Govt. of Australia (Reviewer's comment ID #: 2001-92)]	Text clarified
TS-479	A	22:34	22:36	The sentence is in direct contradiction to the statement at TS page 21 lines 11-12, which correctly identifies the role of the hydrological cycle and surface wetness in regulating surface temperatures. Droughts will amplify local daytime maximum temperatures because of the relatively dry soil that develops through lack of rainfall. Globally, warmer temperatures are associated with an enhanced hydrological cycle and increased rainfall, albeit tropical and low-latitude rainfall has tended to shift to ocean regions. [William Kininmonth (Reviewer's comment ID #: 128-26)]	There is both a cause and effect. Higher temperatures cause drying, and less moisture means more sensible heating, as stated in ch 3.
TS-480	A	22:34	22:36	This statement is not really very helpful because it does not indicate a trend or change--just that there have been widespread droughts. Has the number increased? [Michael MacCracken (Reviewer's comment ID #: 152-107)]	See TS-479 Not just number but areal extent, duration and intensity.
TS-481	A	22:34	22:36	The statement on droughts is a bit wimpy: see chapter 3 exec summary. I would like to see this expanded somewhat. [Kevin Trenberth (Reviewer's comment ID #: 265-10)]	edited

TS-482	A	22:34	:36	There should be some quantitative evidence here to substantiate changes in drought frequency, duration and/or magnitude. Also, information about location of changes in drought characteristics should be mentioned. [Govt. of United States of America (Reviewer's comment ID #: 2023-906)]	Text edited. See TS-479 See FAQ 3.2. This has a sound basis in Ch 3.
TS-483	A	22:40	22:42	In these numbers, is West Antarctica, so ice resting on the sea floor, at least in part, counted as land or ocean. Does Greenland count as part of the snow-covered area of land? [Michael MacCracken (Reviewer's comment ID #: 152-108)]	Insufficient space to discuss this in TS.
TS-484	A	22:44	22:44	Should say "fresh snow" can have an albedo up to 90%--not true generally. [Michael MacCracken (Reviewer's comment ID #: 152-109)]	Rejected. Covered by 'up to 90%'
TS-485	A	22:45	22:47	This is really not very clear. Does this mean that there is frozen ground out beyond the maximum winter extent of snow cover, or just beyond the minimum extent of permanent snow cover. And how frozen does ground have to be to be called frozen. I just don't think the key point about all of this is coming across. [Michael MacCracken (Reviewer's comment ID #: 152-110)]	See chapter 4 for details. Insufficient space to elaborate here.
TS-486	A	22:49	22:50	The relevance of the second sentence needs to be questioned - especially since it is unlikely all of Antarctica would melt under current future scenarios. If it is to be kept in, suggest the following, "The current volume of the Greenland and Antarctic ice sheets are equivalent to approximately 7m and 57m of sea level change respectively". [Govt. of Australia (Reviewer's comment ID #: 2001-93)]	Accepted. Sentence edited and moved to ice sheet stability box, where it has been carefully qualified.
TS-487	A	22:49	22:50	The TAR had Antarctica at 61 m rather than 57 m--and it also in a footnote made the point that the actual sea level equivalents are greater, but that ultimate change would be less due to land rebound. So, where does AR4 come up with 57 m--why the change? [Michael MacCracken (Reviewer's comment ID #: 152-111)]	57 m refers to the new BEDMAP data by Lythe et al.
TS-488	A	22:52	22:53	This sentence is misleading. It should be qualified to read: "On a regional scale, variations in mountain snowpack, glaciers and small ice caps play a crucial role in regulating fresh water availability." [William Kininmonth (Reviewer's comment ID #: 128-27)]	Text edited along the lines suggested
TS-489	A	22:53	22:54	ice to liquid water at specific temps (for FW ice and saline ice) [Roger Barry (Reviewer's comment ID #: 13-2)]	Text edited
TS-490	A	22:53	22:55	This sentence is incomplete and misleading as a supply of latent energy is also required to melt ice when its temperature is elevated to the melting point. This is particularly relevant for the Greenland and Antarctic ice sheets located in the polar regions where there is annual average top of the atmosphere radiation deficit. Melting of these ice sheets would require an enhanced poleward transport of energy from the tropics. The sentence could be deleted without loss of meaning to the following sentences. [William Kininmonth (Reviewer's comment ID #: 128-28)]	Changed to 'following sufficient warming'
TS-491	A	22:54	22:54	I would urge changing to read "ice in specific areas is a" as not all ice is at the same temperature and it would not everywhere change at the same time. [Michael MacCracken (Reviewer's comment ID #: 152-112)]	Rejected. Believe current text is clear
TS-492	A	23:4	:10	Aren't satellite observations of snow cover for the SH available for the same period as for	Satellites are not all polar orbiters with

				the NH (i.e., 1966-2004)? If so, why can't similar trends be determined for the SH? [Govt. of United States of America (Reviewer's comment ID #: 2023-907)]	global coverage.
TS-493	A	23:7	23:8	It is not clear if this sentence is referring to records of snow cover--and if so, where it is located. Is this about glaciers in the Andes or what? [Michael MacCracken (Reviewer's comment ID #: 152-113)]	Rejected. Believe text is clear and doesn't require naming of specific regions.
TS-494	A	23:8	23:8	""The decrease IN BOTH HEMISPHERES (add words) has been...."" , that is, unclear as written whether this sentence applies only to SH, or both hemispheres." [Govt. of Canada (Reviewer's comment ID #: 2004-131)]	Text has been edited
TS-495	A	23:15	23:17	Change the ending of the sentence to read: "... is often closely associated with the altitude of the freezing level". Snow mass is relatively stable at temperatures below freezing because of the large amount of latent heat required for ablation. However, when the temperature warms to freezing and the ice is able to melt then significantly less latent energy (about one-eighth) is required for the phase change. A statement could be added indicating that local freezing levels have risen with the global increase in surface temperatures. [William Kininmonth (Reviewer's comment ID #: 128-29)]	Accepted
TS-496	A	23:20		Snow occurs at altitudes ABOVE the snowline. Rephrasing needed here. [Adrian Simmons (Reviewer's comment ID #: 242-8)]	accepted
TS-497	A	23:23	23:13	"It is not correct to say that permafrost and seasonally frozen ground display large changes in recent decades. The magnitude of changes in permafrost temperature and thaw depth is variable and in some regions (southern Mackenzie Valley for eg.) permafrost temperatures have changed very little. The data presented in section 4.7 illustrates this variability and the statements made in the TS should reflect this." [Govt. of Canada (Reviewer's comment ID #: 2004-132)]	Suggested text: Permafrost and seasonally frozen ground in most regions display....
TS-498	A	23:23	23:23	It is not correct to say that permafrost and seasonally frozen ground display large changes in recent decades. The magnitude of changes in permafrost temperature and thaw depth is variable and in some regions (southern Mackenzie Valley for eg.) permafrost temperatures have changed very little. The data presented in section 4.7 illustrates this variability and the statements made in the TS should reflect this. [Sharon Smith (Reviewer's comment ID #: 244-79)]	See TS-497
TS-499	A	23:24	23:24	The sentence should be revised: "Changes in permafrost conditions may affect river runoff, water supply, carbon exchange, landscape (including slope) stability and infrastructure integrity." Note that landscape stability is a more inclusive term than rock falls. [Sharon Smith (Reviewer's comment ID #: 244-83)]	Text has been edited
TS-500	A	23:25	23:25	permafrost temperature up to 3C (at -3m depth) [Roger Barry (Reviewer's comment ID #: 13-3)]	<i>Edited along the lines suggested:</i> Temperature at the top of the permafrost layer has increased by up to 3°C since the 1980s.

TS-501	A	23:25	23:25	<p>"The depth at which these increases in permafrost temperature are observed should be provided. Greater increases in temperature will occur at shallower depths and changes in temperature in the upper few metres of the ground will reflect more recent changes in climate than those observed at greater depths. The statement focusses on the maximum rate and gives no indication that there is a fair bit of variability. It is not until 2 sentences later that the variability in the trends is mentioned and perhaps the 4th sentence in the paragraph should be moved up to follow this sentence that discusses the change in permafrost temperatures."</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-133)]</p>	With the suggested text in TS-500 this should be clear.
TS-502	A	23:25	23:26	<p>"Some of the data in this paragraph seem inconsistent with data in paragraph 2 section 4.7.2.3 of the WG1 report where it says the basal thaw rate in Alaska has been 0.04 m/yr since 1992 and about 0.01 to 0.02m/yr since the 1960s over the Tibetan Plateau."</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-134)]</p>	Accepted
TS-503	A	23:25	23:26	<p>"The thickness of permafrost that these rates of thawing are associated with should be given as well as the time period over which the changes have occurred (see further comments in review of ch 4)"</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-135)]</p>	Accepted
TS-504	A	23:25	23:25	<p>Permafrost temperature (singular) where has increased--is this average for world or what. Is this average for permafrosted areas?</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-114)]</p>	Clarified
TS-505	A	23:25	23:25	<p>The depth at which these increases in permafrost temperature are observed should be provided. Greater increases in temperature will occur at shallower depths and changes in temperature in the upper few metres of the ground will reflect more recent changes in climate than those observed at greater depths. In addition larger changes our observed at shallower depths as well as shorter-term fluctuations. The statement focusses on the maximum rate and gives no indication that there is considerable variability (these larger changes are generally observed in colder permafrost). It is not until 2 sentences later that the variability in the trends is mentioned and perhaps the 4th sentence in the paragraph should be moved up to follow this sentence that discusses the change in permafrost temperatures.</p> <p>[Sharon Smith (Reviewer's comment ID #: 244-80)]</p>	Edited, thickness indicated
TS-506	A	23:25	23:26	<p>The thickness of permafrost that these rates of thawing are associated with should be given as well as the time period over which the changes have occurred (see further comments in review of ch 4, comment #38)</p> <p>[Sharon Smith (Reviewer's comment ID #: 244-81)]</p>	Accepted
TS-507	A	23:25		<p>Should read: "Temperature at the top of the permafrost has increased"</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-908)]</p>	Accepted
TS-508	A	23:25	:26	<p>Drop this sentence since bottom up thawing is due in many places to loss geothermal</p>	Text edited

				gradient and not present climate. The 0.4m does not agree with the 0.01-0.02 in text (Chap 4; page 4-31). [Govt. of United States of America (Reviewer's comment ID #: 2023-909)]	
TS-509	A	23:26	23:26	I think the text should read "0.02m/year on the Tibetal Plateau to 0.04 m/year in Alaska". [David Parker (Reviewer's comment ID #: 195-81)]	Corrected
TS-510	A	23:28	23:29	"It is important to indicate that this northward movement of permafrost boundaries in Canada has occurred since the Little Ice Age and has occurred over more than a century." [Govt. of Canada (Reviewer's comment ID #: 2004-136)]	Deleted
TS-511	A	23:28	23:28	"Permafrost boundaries have moved northwards in Canada" supplement "and parts of Russia" (ACIA report 2004) [Govt. of Germany (Reviewer's comment ID #: 2011-85)]	Deleted
TS-512	A	23:28	23:29	It is important to indicate that this northward movement of permafrost boundaries in Canada has occurred since the Little Ice Age and has occurred over more than a century. [Sharon Smith (Reviewer's comment ID #: 244-82)]	See TS-510
TS-513	A	23:33	23:35	This is a surprising finding and not in accord with many reports of a delayed freezing and earlier onset of thawing that has increased the growing season by about 14 days in many parts of northern Europe and Russia. [William Kininmonth (Reviewer's comment ID #: 128-30)]	Deleted as this applies only to the Eurasian Arctic.
TS-514	A	23:34	23:34	Do you really mean to say that the freeze date in autumn advanced (meaning toward beginning of year)? Should it not be going in the opposite direction of the thaw change? [Michael MacCracken (Reviewer's comment ID #: 152-115)]	See TS-513
TS-515	A	23:41	23:44	I think it would be useful to say here that the present warming is Arctic wide, in contrast to the early 20th century warming in the Arctic, which was focused in the Atlantic sector--the entire character of the warming is different. [Michael MacCracken (Reviewer's comment ID #: 152-116)]	Out of place in the cryosphere section. See discussion of Arctic warming in the atmosphere section
TS-516	A	23:41	23:43	The values differ slightly from those in the caption of Figure TS-15 where 1979, not 1978, is given as the start year. [David Parker (Reviewer's comment ID #: 195-82)]	Data start in Nov 1978, but 1979 is first full year. TS text and figure caption will be made consistent with chapter 4.
TS-517	A	23:46	23:48	Even if it is true that a decrease in sea ice does not directly change the sea level, the referred sentence is ambiguous because a decrease in sea ice extent is a major source of positive feedback which will contribute to an increase in sea level. It would be better to write "do not directly contribute to seal level change " instead of "do not contribute to seal level change ". I also suggest to add "and it is recognised as a major source of positive feedback" at the end of the referred sentence. [Govt. of France (Reviewer's comment ID #: 2010-122)]	First point is accepted. Second point is accepted in part, as 'recognized as a source of positive feedback' since it is important not to confuse this this the dominant feedback to the global mean through water vapor.
TS-518	A	23:46	:48	Minority portions of above-hydroface ice melting may contribute to sea level rise. [Govt. of United States of America (Reviewer's comment ID #: 2023-910)]	True but too small to merit mention here.
TS-519	A	23:52	23:53	I think it would also be helpful to give the change as a percentage and not just as 1 m.	<i>Accepted.</i>

				[Michael MacCracken (Reviewer's comment ID #: 152-117)]	Ice thickness data in average sea-ice thickness in the central Arctic of about 1m (40%) from ...
TS-520	A	23:56	:57	The observation of widespread glacial and ice cap mass retreats during the 1930s and 1940s and the relation of this retreat to anthropogenic versus natural causes needs to be explained here. [Govt. of United States of America (Reviewer's comment ID #: 2023-911)]	Rejected. This is the observations section, not attribution
TS-521	A	24:2	24:3	Please write 0.81 ± 0.43 mm per year [Govt. of Austria (Reviewer's comment ID #: 2002-33)]	Accepted
TS-522	A	24:2		Sea level equivalent needs to be defined. [Govt. of United States of America (Reviewer's comment ID #: 2023-912)]	See glossary
TS-523	A	24:10	24:10	Here and maybe also elsewhere: all mass (im)balances should be given as rates, i.e. Gt per year (or whatever) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-73)]	Of course all (im)balances are given in rates. Moved per year within the sentence to make this clear.
TS-524	A	24:12	24:18	I think it would be helpful here to differentiate between the situations on West and East Antarctica. Also, on line 13, change "older" to "earlier" [Michael MacCracken (Reviewer's comment ID #: 152-118)]	First point is rejected. Believe further differentiation is not warranted given uncertainties. Second point is accepted.
TS-525	A	24:13	24:13	for older times ?? Clarify [Roger Barry (Reviewer's comment ID #: 13-4)]	See TS-524, changed to earlier
TS-526	A	24:20		Values towards the pink colours are not negative, as opposed to what is on the graphs now? [Govt. of Hungary (Reviewer's comment ID #: 2012-15)]	Caption and text appear to be correct.
TS-527	A	24:20		The legend to Fig. TS-16 is unclear. Do the maps show the difference between the periods 1989-2005 and 1992-2005 for Greenland and Antarctica, respectively? Are these rates? What are the units for the +/-50 color scale? What are the units for the inset? [Franklin SCHWING (Reviewer's comment ID #: 230-23)]	Caption explicitly says that rates are being shown so first part of comment rejected. Second part partly accepted. Units were given for the inset in the caption but have been added for the color bar.
TS-528	A	24:22	24:31	The presentation in this paragraph is incomplete. The underlying chapter (Pg 4-27, lines 8-9) includes the assessment that "Large ice-flow models do not accurately capture the physical processes involved in such dramatic iceberg calving (as the breakup of Larsen B), or the more common calving behavior." This assessment indicates that we lack the tools for forecasting the contribution of ice flow to sea level rise. This point is made less clearly in Box TS 3.2, but it should be related directly to the behavior of Larsen B, as was done in the underlying chapter. [Lenny Bernstein (Reviewer's comment ID #: 20-27)]	Accepted with minor editing to make consistent with the fact that this is the observations section.
TS-529	A	24:22	24:31	Please add the caveat from the underlying chapter (Pg 4-17. lines 8-9) "Large ice-flow models do not accurately capture the physical processes involved in such dramatic iceberg	See TS-528

				calving (as the breakup of Larsen B) or more common calving behavior." This caveat implies that this behavior cannot be forecast with currently available tools. The statement is made less clearly in Box TS 3.2, but it should be related directly to the behavior of Larsen B, as was done in Chapter 4. [Jeff Kueter (Reviewer's comment ID #: 137-28)]	
TS-530	A	24:22	:31	The presentation in this paragraph is incomplete. The underlying chapter (Pg 4-27, lines 8-9) includes the assessment that "Large ice-flow models do not accurately capture the physical processes involved in such dramatic iceberg calving (as the breakup of Larsen B), or the more common calving behavior." This assessment indicates that we lack the tools for forecasting the contribution of ice flow to sea level rise. This point is made less clearly in Box TS 3.2, but it should be related directly to the behavior of Larsen B, as was done in the underlying chapter. [Govt. of United States of America (Reviewer's comment ID #: 2023-913)]	See TS-528
TS-531	A	24:27	24:27	Delete "future" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-74)]	Accepted
TS-532	A	24:27	25:14	Within ice sheet, above basal interface, is density stratification and interstitial liquefaction included in dynamics/stability prognosis? [Govt. of United States of America (Reviewer's comment ID #: 2023-914)]	Too detailed to elaborate each term in ice dynamics models here. Believe page 25, lines 11-14 are sufficient for the TS.
TS-533	A	24:29	24:29	Should change "may have" to "has likely"--stick with your lexicon. [Michael MacCracken (Reviewer's comment ID #: 152-119)]	Accepted
TS-534	A	24:39	24:39	Need to capitalize Earth. [Michael MacCracken (Reviewer's comment ID #: 152-120)]	Copy editing to be done later
TS-535	A	24:40	24:40	Insert "several", or "one or more", "before orders of magnitude" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-75)]	Accepted
TS-536	A	24:42	24:43	This sentence is inconsistent because if the basal conditions were well-characterised it should reduce the uncertainty of ice sheet stability. Suggest substitute: "Basal conditions are generally poorly characterised," [William Kininmonth (Reviewer's comment ID #: 128-31)]	Agree
TS-537	A	24:42	24:42	Insert "only" before "well-characterized" and "a" before "few" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-76)]	See TS-536
TS-538	A	24:54	24:56	"Available data" suggests measurements of a relationship between temperature increase and ice shelf melt. Suggest the sentence begin: "Model studies suggest" [William Kininmonth (Reviewer's comment ID #: 128-32)]	Changed to studies to cover the fact that observations are part of such estimates, not just models
TS-539	A	25:1	25:14	The first sentence is really important--but seems to be in conflict with what this report is saying about sea level rise, which has ice sheets growing during warming (at the very least, the report should be indicating that this would only last for a while). The sentence also says shrinkage can be far faster than growth--yet this report does not seem to allow for this on Greenland, even though there is paleo evidence for this being the case entering the Eemian. The box is a bit strange in that after saying there can be a fast response, it talks about slow	Changed wording regarding meltwater to address first part of comment. Second part of statement is covered on lines 11-14. Models for sea level rise to be addressed

				responses--seeming to ignore what is being learned about how meltwater can carry the heat signal well into the glacier, not by conduction, but by advection. Then there is a statement that ice stream velocities only change slowly--yet we know from recent Antarctic experience that this is not correct--changes occurred very quickly behind the Larsen-B ice shelf after breakup. Thus, this whole paragraph seems to need some work. And then there is the last sentence here, saying that models may underestimate the change--and I agree with that, but the sea level projections IPCC is relying on seem to totally ignore the potential for this, focusing rather tightly on the model results. Much work is needed on all of this. [Michael MacCracken (Reviewer's comment ID #: 152-121)]	further later, not in the obs section
TS-540	A	25:1	:4	The first two sentences provide different details on ice sheet shrinking and growing. Specifically, the first sentence states that shrinking occurs faster than growing, but the second sentence states that current warming can occur on very slow time scales. [Govt. of United States of America (Reviewer's comment ID #: 2023-915)]	Disagree. Believe current text is clear.
TS-541	A	25:2	25:2	Qualify the sentence by concluding: "... and that shrinkage can be much faster than growth because of the differences in latent heat per unit water mass released during snow formation and that required for ice sheet melting". The latent heat released during snow formation must be a net loss to the climate system as ice is in a lower energy state than water; melting of the ice is a net increase of the energy state of the climate system. [William Kininmonth (Reviewer's comment ID #: 128-33)]	Largely true but we don't have space here to discuss each aspect of the processes. The reviewer has not identified any errors or inconsistencies in what is stated.
TS-542	A	25:6	25:8	This scenario could be illustrated by reference to the Hienrich events during the last ice age when there was regularly recurring variations in Greenland ice calving as evidenced by the changing ice-raft debris from Noerth Atlantic ocean sediment cores. [William Kininmonth (Reviewer's comment ID #: 128-34)]	No space for this level of detail here.
TS-543	A	25:18	25:28	This text should be given more prominence. The role of the oceans in the climate system is critical, but the average layman does not recognize this. Non-technical discussions of climate change focus on the changes in surface air temperature and extreme events. The information in this paragraph is almost never discussed. [Lenny Bernstein (Reviewer's comment ID #: 20-28)]	Noted – but no specific suggestions given
TS-544	A	25:18	25:25	It is the only the first of these sentences that is true. The ocean heat content is forced by solar insolation and the influence of solar radiation is reflected in the meridional temperature gradient. The heat exchange between the ocean and the atmosphere is from the ocean to the atmosphere and regulated by SST. The ocean's heat capacity may be about 10,000 times that of the atmosphere but that does not mean the ocean is taking up energy from the atmosphere to any significant degree. Despite the solar radiation of the past million plus years the tropical oceans continue to be a thin lens of warm water overlaying the cold deep oceans. This is because of the meridional overturning ocean circulation that has sinking cold dense waters in polar regions (density and wind driven) and compensating slow ascent of that cold water over the world's oceans. Tropical solar radiation penetrates and is absorbed in the surface 100 metres approximately but does not warm the ocean deep against the slowly ascending cold sub-surface water. It is fanciful to suggest that increased	Rejected - Mass, energy and momentum exchanges occur between the ocean and the atmosphere.

				back radiation from the atmosphere will do any more than marginally change the surface energy budget (ie, the back radiation that is directly absorbed at the surface will be directly compensated by increased conduction, evaporation and longwave radiation loss. It is ENSO variations and the change to the entrainment of cold sub-surface water into the surface mixed layer that have a major impact on tropical SST and the corresponding response of the atmospheric circulation. The changing heat budget of the ocean is also influenced by the intensity of the meridional overturning circulation - a declining intensity, as reported elsewhere, will contribute to increased downward flux of energy and increasing ocean heat content. Change after the first sentence to read: "The ocean is forced by absorption of solar radiation in the surface layer and by the exchange of mass, energy and momentum with the atmosphere. The ocean's heat capacity is about 1000 times larger than that of the atmosphere and the ocean's capacity for net heat uptake is potentially many times greater than the atmosphere (see Figure TS-17). Global observations of the heat taken up by the ocean are hence a definitive test of changes in the global energy budget, including changes resulting from changing intensity of the meridional overturning circulation. Changes in the amount of energy exchanged between the upper layers ocean and the atmosphere play a crucial role for climate variations on seasonal to inter-annual timescales, such as El Nino. Changes in the ocean transport of heat and regional changes of sea-surface temperature have many important effects on regional climates worldwide. Life in the sea" [William Kininmonth (Reviewer's comment ID #: 128-35)]	
TS-545	A	25:18	25:28	The general public does not understand the central role of the oceans in the climate system. The information in this paragraph should be emphasized and expanded. [Jeff Kueter (Reviewer's comment ID #: 137-29)]	Noted – but no specific suggestions given
TS-546	A	25:18	:28	This text should be given more prominence. The role of the oceans in the climate system is critical, but the average layman does not recognize this. Non-technical discussions of climate change focus on the changes in surface air temperature and extreme events. The information in this paragraph is almost never discussed. [Govt. of United States of America (Reviewer's comment ID #: 2023-916)]	Noted – but no specific suggestions given
TS-547	A	25:32	25:37	sea level changes are also measured locally against land and so can be affected by land tectonic movements. [David Rind (Reviewer's comment ID #: 214-7)]	See box on sea level
TS-548	A	25:32	:37	Sea level changes are also measured locally against land and so can be affected by land tectonic movements - would be useful to say that here [Govt. of United States of America (Reviewer's comment ID #: 2023-917)]	See TS-547
TS-549	A	25:36	25:36	Change "sea level" to "changes in sea level" [Michael MacCracken (Reviewer's comment ID #: 152-122)]	OK
TS-550	A	25:45	26:17	The title of the paragraph refers to changes in ocean circulation. In the paragraph text I see not reference to the ocean circulation. Is there a paragraph missing or is the title of that subsection to be changed ? [Philippe Tulkens (Reviewer's comment ID #: 271-5)]	MOC text has been added

TS-551	A	25:47	25:48	Replace "The heat content of the world's oceans has increased since 1955" with "The heat content of the world's oceans has fluctuated since 1955. It was steady from 1955 to 1970. It increased from 1970 to a peak in 1980. It then fell from 1980 to 1987, and then increased from 1987 to 2005. . . The 2005 figure is only slightly above the 1978 figure and there is no evidence to suggest a steady upwards trend" [VINCENT GRAY (Reviewer's comment ID #: 88-1938)]	Rejected – TS-18 gives clear exposition of trend over the whole period specified.
TS-552	A	25:47	25:48	Remove italics [VINCENT GRAY (Reviewer's comment ID #: 88-1939)]	Rejected – this is a consistent style used throughout to indicate headline statements
TS-553	A	25:49		What do the blue and pink colors represent in the upper panel of Fig. TS-18? [Franklin SCHWING (Reviewer's comment ID #: 230-24)]	This panel has been deleted
TS-554	A	25:55	26:1	The phrase 'average heating rate of 0.2Wm ⁻² (per unit of the Earth's Surface)' needs explanation. Is this figure per decade or for the whole period?. Is (per unit of the Earth's surface) necessary? [Govt. of Australia (Reviewer's comment ID #: 2001-94)]	Reviewer confusing units – text edited to try to clarify
TS-555	A	26:5	26:17	There seems to be a very strong similarity between the trends on Figures TS-18 and TS-7. Since warming is coupled with energy content, a para would be needed here to refer to section TS.3.4, and to make the point that results from independent measurement techniques suggest the same conclusions - thus the document could reinforce the robustness of statements on climate change. This would also be needed, because the text below the graph speaks about warming, although the graph itself only shows energy content. [Govt. of Hungary (Reviewer's comment ID #: 2012-16)]	Reviewer seems to be misreading the graphs. They are not closely correlated.
TS-556	A	26:7	26:7	Insert after "widespread", "since 1990" [VINCENT GRAY (Reviewer's comment ID #: 88-1940)]	Rejected, inconsistent with chapter.
TS-557	A	26:7	26:7	Insert after "While the", "recent" [VINCENT GRAY (Reviewer's comment ID #: 88-1941)]	See TS-557
TS-558	A	26:7	26:17	The basis for the claim that warming is penetrating deeper in the Atlantic Ocean (than the Pacific or Indian Oceans) being consistent with the deep overturning circulation cell is not clear. The last sentence of the paragraph states that there is no confirmed trend in the Atlantic meridional overturning circulation. Deeper penetration would require a reduction in the MOC. It could be noted that the cooling observed in parts of the North Atlantic and North Pacific are consistent with increased upwelling associated with the strengthened offshore westerly winds noted elsewhere. [William Kininmonth (Reviewer's comment ID #: 128-36)]	The NA has greater deep water formation rates due to its circulation patterns. This does not imply a change in the MOC, only a difference in its characteristics between basins. The reviewer is incorrect. Warming of surface water will be carried deeper into the North Atlantic because the MOC extends deeper into the North Atlantic compared with the other ocean basin. The MOC is assessed as unchanged in the observations and have added a statement to this effect in the text.

TS-559	A	26:14	26:14	Change "linked in part" to "that are apparently linked in part" as we really do not have much more than a correlation to work with--and a limited one at that. [Michael MacCracken (Reviewer's comment ID #: 152-123)]	Rejected. Believe current text is clear, given language 'in part'.
TS-560	A	26:15	26:16	East China Sea. [David Parker (Reviewer's comment ID #: 195-83)]	Accepted
TS-561	A	26:21	26:22	Either explain how the estimate of carbon uptake by the oceans was derived or delete this statement. The text in section 5.4 says that indirect methods were used, which is obvious since direct measurements were not made since 1750, but does not explain what those methods were. The text in section 7.3 merely restates the quantity of carbon absorbed, and says: "This inventory estimate is currently being revised by several authors." This seems an inadequate basis for inclusion of the information in the TS. [Lenny Bernstein (Reviewer's comment ID #: 20-29)]	Noted, has been clarified. Added text from Chapter 5 however, full details have to be left to the chapter.
TS-562	A	26:21	26:21	"The fraction of CO2 emissions absorbed..." - We presume you mean "anthropogenic CO2 emissions"? It is ambiguous without this clarification. [Govt. of Hungary (Reviewer's comment ID #: 2012-17)]	Agreed, corrected
TS-563	A	26:21	26:27	More explanation of the methodologies used to estimate carbon uptake since 1750 by the oceans is needed. The links to underlying chapters are insufficient. Section 5.4 merely says that indirect methods were used, without explaining what they were. Section 7.3 provides the estimates, but no explanation of how they were derived. It also says that the estimates are being revised. All of this leaves the reader with no basis for evaluating the quality of the estimate. [Jeff Kueter (Reviewer's comment ID #: 137-30)]	See TS-561
TS-564	A	26:21	:22	Either explain how the estimate of carbon uptake by the oceans was derived or delete this statement. The text in section 5.4 says that indirect methods were used, which is obvious since direct measurements were not made since 1750, but does not explain what those methods were. The text in section 7.3 merely restates the quantity of carbon absorbed, and says: "This inventory estimate is currently being revised by several authors." This seems an inadequate basis for inclusion of the information in the TS. [Govt. of United States of America (Reviewer's comment ID #: 2023-918)]	See TS-561
TS-565	A	26:23	26:24	Cross-link to Table TS-1 for detail. [Govt. of Australia (Reviewer's comment ID #: 2001-95)]	This specific statement has been removed for other reasons, but the paragraph is now colocated with Table TS-1
TS-566	A	26:24	26:25	The phrase 'this trend is expected to continue' needs explanation. Which trend? - the fraction of CO2 emissions absorbed to decrease, the volume of CO2 absorbed to increase, or both? [Govt. of Australia (Reviewer's comment ID #: 2001-96)]	Accepted, text has been edited
TS-567	A	26:24	26:25	The trend in decreasing ocean fraction is only expected to continue for future emission scenarios where emissions continue and do not decline. If emissions went to zero, for example, ocean uptake would continue making the ratio indefinite. Suggest that this	Rejected. Commitment to future warming implies that the trend will continue even with much smaller

				sentence either be removed or that this condition for this statement to be true be added. [Haroon Kheshgi (Reviewer's comment ID #: 125-29)]	emissions and accounts for reasonable expectations.
TS-568	A	26:29	26:34	The uncertainties in table TS1 are 1-sigma, therefore, I expect that these uncertainties are 1-sigma as well. Fix. [Haroon Kheshgi (Reviewer's comment ID #: 125-26)]	Caption now notes that only 65% confidence intervals can be provided – details in chapter
TS-569	A	26:32	26:32	Change 'the former' to "what has been called the"--saying "the former" somehow implies it was a real thing--it was a figment of our limited understanding. [Michael MacCracken (Reviewer's comment ID #: 152-124)]	Text edited
TS-570	A	26:33		“...and an uncertainty of a factor of 2...” as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty consistent across chapters or at least contain a prefacing discussion in the TS. [Govt. of United States of America (Reviewer's comment ID #: 2023-919)]	Text is now removed for other reasons
TS-571	A	26:38	26:38	Delete 'this' in 'this carbon uptake'. The previous paragraph discusses land sinks as well as ocean uptake. [Govt. of Australia (Reviewer's comment ID #: 2001-97)]	Text edited
TS-572	A	26:38	26:39	Either explain how the estimate of pH change in the oceans was derived or delete this statement. The text in section 5.4 provides an adequate explanation of the way this estimate was derived, but it needs to be repeated here. Clearly, it is not the result of direct measurements made in 1750. [Lenny Bernstein (Reviewer's comment ID #: 20-30)]	Text edited, material from chapter added
TS-573	A	26:38	26:38	Insert at beginning "Calculations suggest that" [VINCENT GRAY (Reviewer's comment ID #: 88-1942)]	Rejected. Based on more than calculations. Text edited
TS-574	A	26:38	26:38	Replace "has" by "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-1943)]	Rejected. No basis given for suggestion
TS-575	A	26:38	26:38	Remove italics [VINCENT GRAY (Reviewer's comment ID #: 88-1944)]	Rejected. No basis given for suggestion not to have headlines
TS-576	A	26:38	26:38	Insert after "1750". So far, this has not been confirmed by observations" [VINCENT GRAY (Reviewer's comment ID #: 88-1945)]	Text has been edited
TS-577	A	26:38	26:43	The explanation of the methodology used to determine the change in pH of the oceans since 1750 presented in section 5.4 should be repeated here to provide readers with the information they need to evaluate the quality of this estimate. [Jeff Kueter (Reviewer's comment ID #: 137-31)]	Text edited
TS-578	A	26:38	:39	Either explain how the estimate of pH change in the oceans was derived or delete this statement. The text in section 5.4 provides an adequate explanation of the way this estimate was derived, but it needs to be repeated here. Clearly, it is not the result of direct measurements made in 1750.	Text edited

				[Govt. of United States of America (Reviewer's comment ID #: 2023-920)]	
TS-579	A	26:49	27:4	Very important – should be emphasized. [Govt. of United States of America (Reviewer's comment ID #: 2023-921)]	Noted – but no specific suggestions given
TS-580	A	26:51	26:51	Insert "except the Arctic Ocean" after "poleward of 50°N" (see Figure TS-20). [David Parker (Reviewer's comment ID #: 195-84)]	Taken into account
TS-581	A	27:1	27:1	Insert "the Atlantic at" before "24N". [David Parker (Reviewer's comment ID #: 195-85)]	Text revised based on revisions to chapter.
TS-582	A	27:8		Section TS.3.3.3. The estimates of sea level changes are given in mm/year with an uncertainty range. Since the figures are small in per year values and the uncertainties relatively large, it would be useful to add also some estimates for sea level change over a longer period (50 or 100 years) with the uncertainty associated with such estimate. The evolution over several decades might be also more representative of the pace of sea level rise than the annual mean values only. [Philippe Tulkens (Reviewer's comment ID #: 271-6)]	Rejected. The period of averaging has been indicated in each case. The error bars remain the same in relative terms with the suggested approach.
TS-583	A	27:13	27:17	I can understand that it is possible to get a long estimate of thermal expansion, but is there really enough data to get a central estimate for ice sheets? Coming up with some sort of range seems plausible, but a central value? In that the missing term needs to be adding water to the ocean, how can "impoundment in reservoirs" add water to the ocean? [Michael MacCracken (Reviewer's comment ID #: 152-125)]	Clarify text Add reference back to ch 4.8 on line 17. Also see Table TS-3.
TS-584	A	27:15	27:15	Replace "estimated observed" with "central value for the observed." A value can be estimated or observed, but not both. [Lenny Bernstein (Reviewer's comment ID #: 20-31)]	clarified
TS-585	A	27:15		Replace "estimated observed" with "central value for the observed." A value can be estimated or observed, but not both. [Govt. of United States of America (Reviewer's comment ID #: 2023-922)]	clarified
TS-586	A	27:17	27:17	It would be nice if you could add a brief explanation about how human activities lead to SL rise. I think it's intuitive to understand human activities that lead to a SL decrease, but a bit less-intuitive just how our actions lead to SL rise, per se. Human activities that lead to SL rise all do so by leading to increased surface runoff of water into the oceans, and these include: deforestation, burning of fossil fuels because this chemical reaction always produces water, and drainage of wetlands. [Melinda Marquis (Reviewer's comment ID #: 162-112)]	Space limitations are a concern. Also need to avoid listing terms that are known to be negligible just for completeness given the space issue.
TS-587	A	27:22	27:22	Insert after "models" "although it is probably due to changes in ground level from urban developmen and removal of ground water" [VINCENT GRAY (Reviewer's comment ID #: 88-1946)]	Rejected. Believe the text is accurate as it stands, see ch. 4 and 5
TS-588	A	27:23	27:23	Do you mean "average rise per century" or the 'total rise' that has occurred over this period- -I would think the latter. [Michael MacCracken (Reviewer's comment ID #: 152-126)]	Clarified
TS-589	A	27:24	27:27	This statement is misleading because there is no information given in Chapter 5.5 on the	Rejected. The logic of the sea-level

				<p>variability of sea level over the past two millennia. Recently measured sea level using Topex/Poseidon is highly correlated with SST. The Roman period was also one of global warmth and probably comparable to the 20th century - that being the case we would expect sea level to be approximately the same then as now. There is no information on sea level change following the Roman period, such as falling sea level during the Dark Ages, rising sea level during the Medieval Warm Period and again falling sea level during the Little Ice Age. The current rate of rise in sea level may not be unusual in the context of recent global warming. The sentence should be restructured as: "Archaeological and geological data (such as information from ancient Roman fish ponds) suggest that modern sea levels have not changed appreciably since then, with an average millennial-scale sea level rise of between 0.0-0.2 mm/yr. This is about six times slower than the observed sea-level rise since 1961 but there is no reliable information on fluctuations in sea-level on multi-centennial timescales associated with global temperature fluctuations. [William Kininmonth (Reviewer's comment ID #: 128-37)]</p>	<p>correlation with SST for global sea-level is incomplete, see ch 5 discussion of deeper ocean. The current observation record clearly shows that the steric component of sea-level rise is not the only term in the total global sea-level; melting ice is very important. The current rate of sea-level rise is unusual compared with the inferred rates from geological and archaeological evidence.</p>
TS-590	A	27:26	27:26	<p>Is this really what is meant--taking the limits, 3000 years times 2 mm/yr gives 0.6 m--is this really what is meant? Was the rate negative leading into the Little Ice Age? It might be more helpful to indicate the best estimate of the range of sea level change--not its annual rate. [Michael MacCracken (Reviewer's comment ID #: 152-127)]</p>	<p>Text is not consistent with this interpretation, but has been clarified further.</p>
TS-591	A	27:29		<p>TS-21 should be redrawn using 1870 as zero level. Also, this figure seems to come also from Fig. 5.5.2, not only from fig. 5.5.1. [Govt. of Hungary (Reviewer's comment ID #: 2012-18)]</p>	<p>Reference to chapter figures will be corrected. Base line is chosen for consistency with other climate data.</p>
TS-592	A	27:32	27:38	<p>In the second sentence, I would suggest changing "rates are" to "rates over this period are", to change "mean rise" to "mean rate of rise" and to change "is falling" to "fell during this period." More generally, it was not clear to me why this whole paragraph was necessary--the point is to talk about variability--but only spatial differences are mentioned--presumably there are also ones in time that have not yet shown up. [Michael MacCracken (Reviewer's comment ID #: 152-128)]</p>	<p>Clarified in second sentence. Believe the rest of the paragraph is clear with reference to that.</p>
TS-593	A	27:42	27:51	<p>I would think this paragraph should be ahead of the preceding one, if the preceding one is kept at all. Or maybe even move it up further in this section. [Michael MacCracken (Reviewer's comment ID #: 152-129)]</p>	<p>Paragraph has been moved up</p>
TS-594	A	27:48	27:51	<p>The conclusion that climate models miss advances in land glacier and ice sheet melt because of internal variability is a strange statement to make. Chapter 8 on climate models points out that they do not include ice flow processes: which have been shown to dominate ice loss both in mountain glaciers and the large Greenland and Antarctica ice sheets. Suggest adding the following sentence at the end of line 51: "The missing sea level increase could also be caused by the inability of current climate models to simulate increasing ice flow rates in land based glaciers and ice sheets." [Bruce Wielicki (Reviewer's comment ID #: 287-4)]</p>	<p>Reviewer has drawn wrong connection between land ice and models lacking enough internal variability – text edited for clarity. Role of ice flow processes in sea level has been extensively clarified.</p>
TS-595	A	27:48		<p>Please refer to chapter 10, which finds that models underestimate sea-level rise over the</p>	<p>Text edited. Believe that it fully reflects</p>

				period 1961-2003 by 40%. [European Commission (Reviewer's comment ID #: 2008-4)]	the state of understanding.
TS-596	A	27:48		Chapter 10 states that models also underestimate the sea level rise 1961-2003 by 40%. This cannot be caused by decadal variability. As this validation error is far more significant than the underestimation of the 1993-2003 rate, please refer to this fact (1961-2003 by 40%) instead of discussing the underestimation during the period 1993-2003. [Govt. of Germany (Reviewer's comment ID #: 2011-86)]	See chapter 5 for discussion of the role of the Pinatubo eruption, which has greatly influenced SLR in the altimetry era. The cause of the differences earlier (1961-2003) are uncertain, see paragraph on this period above where the role of terrestrial water storage is noted as a possible explanation.
TS-597	A	27:48		From chapter 10 I learned that models also underestimate the rise 1961-2003 by 40% - this cannot be put down to decadal variability, and thus is far more significant than the underestimation of the 1993-2003 rate. [Stefan Rahmstorf (Reviewer's comment ID #: 206-39)]	See TS-595
TS-598	A	27:50	27:50	"...variability; models do not..." [David Parker (Reviewer's comment ID #: 195-86)]	Text edited
TS-599	A	27:53	27:53	There is no mention in this Table of changes in ground level due to urban development, and removal of ground water, oil and minerals which is an important influence on tide-gauge measurements [VINCENT GRAY (Reviewer's comment ID #: 88-1947)]	Reject: while an interesting question, the tide gauge data are carefully selected to be less affected by ground motion. The procedures for this are well understood.
TS-600	A	27:53	27:55	Add "Source" or "Source of sea level rise" to the heading of column 1. [Govt. of Hungary (Reviewer's comment ID #: 2012-19)]	Accepted
TS-601	A	28:3	28:47	This box would be improved if it included an unambiguous statement that sea level is not equal in all parts of the world. That thought is there on lines 18-20, but in terms of sea level change. However, even without climate change, there would be variations in sea level. A statement that sea level is not uniform appears on Pg TS-25, lines 32-34, but it needs to be repeated in the box. [Lenny Bernstein (Reviewer's comment ID #: 20-32)]	Accepted, The box has been redrafted by us to be more correct and include.
TS-602	A	28:3	:47	The fact that sea level is not equal around the globe is not intuitively obvious to readers who do not work in the area. This fact, and a brief explanation as to why it is a fact, should be presented at the start of this box. A discussion of this topic appears on Pg TS 25, lines 32-34, but it needs to be restated in this box. It would make the explanation of why sea level change is not equal around the globe much more understandable. [Govt. of United States of America (Reviewer's comment ID #: 2023-923)]	Accepted, see above comment.
TS-603	A	28:7	28:7	Insert after "owing to", "the weight of urban development, removal of groundwater, oil and minerals, and" [VINCENT GRAY (Reviewer's comment ID #: 88-1948)]	Rejected. See TS-599
TS-604	A	28:15	28:18	These two sentences are advocacy rather than an assessment of knowledge. In particular, the second sentence should be moved to the discussion of models and climate projections.	Text edited

				Suggest replace the sentence with: "Observations and models are consistent with this and indicate that thermal expansion and contraction are expected to contribute to any sea-level variations over the next hundred years. Observations and models show that" Alternatively, include in the second sentence (line 17) to read "... change only slowly, model projected thermal expansion would continue" [William Kininmonth (Reviewer's comment ID #: 128-38)]	
TS-605	A	28:17	28:20	It seems to me that the point needs to be made that these regional fluctuations are bounded--the changes cannot simply go on and on, but instead, sea level in particular places can vary over time scales of years to decades. Connecting these lines to those on lines 35-37 would help. [Michael MacCracken (Reviewer's comment ID #: 152-130)]	Text edited
TS-606	A	28:18	28:20	The fact that sea level is not equal around the globe is not intuitively obvious to readers who do not work in the area. This fact, and a brief explanation as to why it is a fact, should be presented at the start of this box. A discussion of this topic appears on Pg TS 25, lines 32-34, but it needs to be restated in this box. It would make the explanation of why sea level change is not equal around the globe much more understandable. [Jeff Kueter (Reviewer's comment ID #: 137-32)]	Text edited
TS-607	A	28:26	28:28	This sentence is somewhat confusing, not completely making clear that it is the mountain glaciers that will be restricted to the high latitudes. It would also be useful to say about how much SL equivalent there is in glaciers and icecaps. [Michael MacCracken (Reviewer's comment ID #: 152-131)]	Two estimates 0.15 and 0.37 m are given in Table 4.1.1; not elaborated here due to length, uncertainties in various regions, etc.
TS-608	A	28:29	28:31	This sentence does not make clear that it only will really apply for a while--once one gets warm enough, even the East Antarctic ice sheet would melt--that is, not all warm climates have an Antarctic ice sheet. So, put a limit on this. And, in fact, it should be mentioned that Antarctica as a whole has lost ice since the Last Glacial Maximum--so a warming climate really causes Antarctica to lose ice--except, the authors seem to be arguing, maybe in the narrow temperature range that we are in. As for my views, I would be hesitant to say this with confidence--making the ice amount a cubic taking its swing just at this time is a bit suspicious to me--it might well not be a cubic, but a monotonic function--with more ice when it is cold and less when warmer. In any case, the rather incomplete sentence here needs to be qualified. [Michael MacCracken (Reviewer's comment ID #: 152-132)]	Believe text is now clear.
TS-609	A	28:29	28:33	This needs work in rate determination. [Govt. of United States of America (Reviewer's comment ID #: 2023-924)]	No specific suggestion given
TS-610	A	28:45	28:47	This last sentence could be strengthened along the following lines 'Where sea level is rising even a modest rise in mean sea level can result in more frequent exceedance of a particular high water level'. [Govt. of Australia (Reviewer's comment ID #: 2001-98)]	Text edited as suggested by the reviewer
TS-611	A	29:7	29:7	Change to "Consistency Among Observations in the Last Few Decades." The industrial era is generally taken as beginning in 1750, but none of the information in this section starts	Text edited

				with that date. The majority of information is for the late 20th century, with some covering the whole 20th century [Lenny Bernstein (Reviewer's comment ID #: 20-33)]	
TS-612	A	29:7	31:2	The industrial era is usually taken as beginning in 1750. This section is mislabeled, since it provide no information for the period before the beginning of the 20th century. [Jeff Kueter (Reviewer's comment ID #: 137-33)]	See TS-611
TS-613	A	29:7		Change to "Consistency Among Observations in the Last Few Decades." The industrial era is generally taken as beginning in 1750, but none of the information in this section starts with that date. The majority of information is for the late 20th century, with some covering the whole 20th century [Govt. of United States of America (Reviewer's comment ID #: 2023-925)]	See TS-611
TS-614	A	29:19	29:19	Insert after "warming" "but, on land, this may be due to urban and land-use factors, and for both land and sea, by instrumental bias." [VINCENT GRAY (Reviewer's comment ID #: 88-1949)]	Rejected. See ch 3 and previous responses to the same issue
TS-615	A	29:20	29:20	Insert after "with" "urban development, and possibly even" [VINCENT GRAY (Reviewer's comment ID #: 88-1950)]	See TS-615
TS-616	A	29:21	29:21	and differences in land-ocean wetness' needs some explanation! Do the authors mean 'differences in potential evaporative cooling due to land-ocean differences in surface water availability'? [William Kininmonth (Reviewer's comment ID #: 128-39)]	Yes, this is what is meant. See ch 3 for details.
TS-617	A	29:23	29:23	"insert before "warming", "apparent" [VINCENT GRAY (Reviewer's comment ID #: 88-1951)]	Rejected. No basis given for suggestion
TS-618	A	29:26	29:27	Replace "are consistent quantitatively within their respective error bars with" by " show distinctive differences from" [VINCENT GRAY (Reviewer's comment ID #: 88-1952)]	Rejected. No basis given for suggestion
TS-619	A	29:26	29:32	Consistency on a global basis has improved, but does not yet exist on tropics versus higher latitudes, for which getting some apparently balancing offsets. Just as IPCC did not jump the gun and join the early MSU-based finding that there is a problem, it should not too rapidly jump to the conclusion that everything is resolved. [Michael MacCracken (Reviewer's comment ID #: 152-133)]	Is now carefully consistent with final draft of Chapters 3 and 9
TS-620	A	29:28	29:28	provided that' is vague in meaning. Redraft - eg 'now that' [Govt. of Australia (Reviewer's comment ID #: 2001-99)]	Text edited
TS-621	A	29:28	29:28	Replace "provided" with "even after" [VINCENT GRAY (Reviewer's comment ID #: 88-1953)]	Rejected, no basis given
TS-622	A	29:29	29:29	Change "Evidence suggests " with standard terminology of uncertainty (see uncertainty guidance) [Aristita Busuioc (Reviewer's comment ID #: 35-8)]	This is a broad statement of consistent patterns and language is intended to reflect that
TS-623	A	29:29	29:30	I suggest: "Some evidence suggests increasing warming with altitude from 1979 to 2004 through much of the troposphere in the tropics..."	Reject. Some evidence too vague.

				[David Parker (Reviewer's comment ID #: 195-87)]	
TS-624	A	29:30	29:31	With respect to the troposphere, this overstates our knowledge and goes beyond the conclusions in Chapter 3. [Melissa Free (Reviewer's comment ID #: 76-12)]	No specific concern has been given. Text is now carefully consistent with Chapter 3.
TS-625	A	29:31	29:32	physical processes' is a more appropriate term than 'basic physical models' as the latter implies constraining assumptions, which is not the case. Also, 'anthropogenic greenhouse gases and aerosols' is a more inclusive descriptor than 'increased greenhouse gases'. [William Kininmonth (Reviewer's comment ID #: 128-40)]	First point is accepted. On second point, the language is retained since here we are discussing the strat/trop distinction
TS-626	A	29:32	29:32	Delete "and observed greenhouse gases" Satellite and radiosondes in the lower troposphere show no evidence of effects from increased greenhouse gases. See comments above (Page 19, lines 5-7) [VINCENT GRAY (Reviewer's comment ID #: 88-1954)]	Rejected. Stratospheric temperature trends show evidence of cooling effect of GHG
TS-627	A	29:34	29:34	Change "...are believed to be consistent..." with standard terminology of uncertainty (see uncertainty guidance) [Aristita Busuioc (Reviewer's comment ID #: 35-9)]	See TS-622; text edited
TS-628	A	29:34	29:34	Here & elsewhere: avoid all statements of belief. Replace (e.g.) "are believed to be" by "appear to be" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-77)]	Text edited
TS-629	A	29:34	:35	Should not contain the words 'believed to be consistent' – they either are or are not consistent. [Govt. of United States of America (Reviewer's comment ID #: 2023-926)]	Text edited
TS-630	A	29:40	29:42	"The statement should indicate these changes in permafrost temperature of up to 3°C are observed in shallow permafrost (upper 20 m). It is not true that these larger changes have occurred in the subarctic. In the Canadian permafrost zone, for example, changes in permafrost temperature in warmer permafrost in the subarctic region (such as central to southern Mackenzie valley) has been much less, on the order of a few tenths of a degree since the 1980s at most as indicated in section 4.7." [Govt. of Canada (Reviewer's comment ID #: 2004-137)]	Text edited re. subarctic. Depth of the temperature measurements clarified. Discussion of further details is beyond the scope of the Executive Summary.
TS-631	A	29:40	29:42	The statement should indicate these changes in permafrost temperature of up to 3°C are observed in shallow permafrost (upper 20 m or at top of permafrost). It is not true that these larger changes have occurred in the subarctic. In the Canadian permafrost zone, for example, changes in permafrost temperature in warmer permafrost in the subarctic region (such as central to southern Mackenzie valley) has been much less, on the order of a few tenths of a degree at most since the 1980s as indicated in section 4.7. The statement should reflect this variability instead of emphasizing the extreme. [Sharon Smith (Reviewer's comment ID #: 244-84)]	See TS-630 Text edited re. subarctic. Variability dealt with elsewhere
TS-632	A	29:40	:41	The 3C change applies to the Arctic (not the Subarctic) and the top of the permafrost. [Govt. of United States of America (Reviewer's comment ID #: 2023-927)]	See TS-630
TS-633	A	29:45	29:45	Table TS-3 says 3.1+/-0.8, not 3.0+/-0.4 mm/yr as stated.	Text has been corrected

				[William Kininmonth (Reviewer's comment ID #: 128-41)]	
TS-634	A	29:45	29:45	I would suggest changing "increase from" to "is higher because of the availability of"--just being a bit more precise. [Michael MacCracken (Reviewer's comment ID #: 152-134)]	accepted
TS-635	A	29:45	29:45	3.0 +/- 0.4 mm/year from 1993 - 2003 is slightly inconsistent with the value in the TS Table TS-3 (adapted from Table 5.5.2, Ch. 5, page 33), which cites a value of 3.1 +/- 0.3 mm/yr. [Melinda Marquis (Reviewer's comment ID #: 162-113)]	See TS-633
TS-636	A	29:45		Is it 3.0, 3.1 or 3.2 mm/yr? [European Commission (Reviewer's comment ID #: 2008-5)]	See TS-633
TS-637	A	29:45		The value given for observed SLR is inconsistent between different chapters. In TS it is 3.0 mm/yr for 1993-2003, elsewhere it says 3.1 mm/yr. Chapter 5 shows 3.2 mm/yr for 1993-2005. Please update all values to the latter, consistently throughout the report. [Govt. of Germany (Reviewer's comment ID #: 2011-87)]	See TS-633
TS-638	A	29:45		Here it says 3.0 mm/yr for 1993-2003, elsewhere it says 3.1. And chapter 5 shows 3.2 mm/yr for 1993-2005 - I propose to update all values to the latter, consistently throughout the report. [Stefan Rahmstorf (Reviewer's comment ID #: 206-40)]	See TS-633
TS-639	A	29:45		Sea level rise from 1993-2003 was listed as 3.1 plus or minus 0.8mm on previous pages – here it says 3.0 plus or minus 0.4mm. Should use consistent number. [Govt. of United States of America (Reviewer's comment ID #: 2023-928)]	See TS-633
TS-640	A	29:47	29:47	1.2 +/- 0.6 mm/year is close to but not exactly the sum of the three values in the TS Table TS-3 (adapted from Table 5.5.2, Ch. 5, page 33): 0.81 +/- 0.43, 0.21 +/- 0.07, and 0.21 +/- 0.35, which sum to 1.23 +/- 0.85. Does this matter? [Melinda Marquis (Reviewer's comment ID #: 162-114)]	See TS-633
TS-641	A	29:52	29:52	May be modified to read as “Some evidence climate parameter.” to reflect that the evidence of consistency at this level is not so very strong at our present level of research findings. [Govt. of India (Reviewer's comment ID #: 2013-6)]	Accepted, headline changed to indicate importance of multiple observations, and text clarified.
TS-642	A	29:52	29:52	should include 'streamflow' also [P.C.D. Milly (Reviewer's comment ID #: 179-26)]	accepted
TS-643	A	29:52	:57	What is the difference between the two bullets describing evidence for changes in circulation patterns? [Govt. of United States of America (Reviewer's comment ID #: 2023-929)]	Merged
TS-644	A	29:54	29:54	What exactly is meant by "hydrologic cycle" here? Reference should be made to streamflow, which is a powerful and reliable measurement of areal-mean water flux. In any case, some observable variable should be cited. [P.C.D. Milly (Reviewer's comment ID #: 179-27)]	accepted
TS-645	A	29:56	29:57	The phrase 'increasing mid-latitude westerlies..' should read 'increasing strength of mid latitude westerlies ..'	accepted

				[Govt. of Australia (Reviewer's comment ID #: 2001-100)]	
TS-646	A	30:6	30:9	On line 6, change start to "Sea-ice extent has"--want singular here, I would think. Can delete "seasons". On line 7, change "circulation" to "ocean circulation". On line 8, change "reduced since" to "decreased since at least". On line 9, change "1970s, which" to "1970s; this" [Michael MacCracken (Reviewer's comment ID #: 152-135)]	Singular is accepted. Circulation refers to both atmosphere and ocean, see text. At least is accepted.
TS-647	A	30:7	30:7	Delete "temperatures". [David Parker (Reviewer's comment ID #: 195-88)]	accepted
TS-648	A	30:11	30:12	This sentence is more optimistic than a reading of the relevant chapters would suggest. For example, while models seem to be able to simulate changes in sea-ice observed during the satellite period there are differences when the models are used to reconstruct sea-ice histories from archived meteorological data. Replace the sentence with "Studies since the TAR have led to improved understanding of how Arctic and Antarctic sea-ice trends are linked to changes in both temperature and atmospheric circulation [4.4, 3.9, 9.5]. [William Kininmonth (Reviewer's comment ID #: 128-42)]	accepted
TS-649	A	30:15	30:15	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle (see chapter 10, p4, lines 6-9 and p16, lines 46-48; see also TS p52, line 44). [Govt. of France (Reviewer's comment ID #: 2010-123)]	accepted
TS-650	A	30:15	30:15	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle (see chapter 10, p4, lines 6-9 and p16, lines 46-48; see also TS p52, line 44). [Serge PLANTON (Reviewer's comment ID #: 199-5)]	See TS-649
TS-651	A	30:16	30:17	As comment 38: replace 1.2% +/- 0.3%, which is ambiguous, by (1.2 +/- 0.3)%, which isn't. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-78)]	Agreed
TS-652	A	30:18	30:18	It seem appropriate to delete "of simple physical models"--why should this be included. This result also comes from earlier models. [Michael MacCracken (Reviewer's comment ID #: 152-136)]	Rejected. Believe language is clear. Reviewer's comment is unclear regarding what kind of model he means.
TS-653	A	30:20	30:21	Minor changes--change "numbers" to "number" and "increased risk" to "the risk" and "from" to "for" [Michael MacCracken (Reviewer's comment ID #: 152-137)]	Changed to likelihood
TS-654	A	30:21	49:22	Temperatures were higher [Roger Barry (Reviewer's comment ID #: 13-10)]	Don't understand comment. Could be error in page/line citation?
TS-655	A	30:27	30:27	It is not clear that the word "regional" is appropriate here--for IPCC region means continental scale, and the droughts are a lot smaller--so maybe say "subcontinental-scale" [Michael MacCracken (Reviewer's comment ID #: 152-138)]	Rejected. Text refers to averages over broad scales, see ch 3.
TS-656	A	30:27	30:27	This statement is not so much coherent with this one of page TS-22, lines 8-12. At present we don't have enough information to generalize this positive trend in heavy precipitation, because of the lack of data (see page TS-22 lines 11-12) and the length and consistency of records (see page TS-22 line 2). Modification proposed: new sentence will be "Although global heavy precipitation has likely increased in many land regions, droughts have also	Text edited

				increased". [Govt. of Spain (Reviewer's comment ID #: 2019-53)]	
TS-657	A	30:29	30:29	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle (see chapter 10, p4, lines 6-9 and p16, lines 46-48; see also TS p52, line 44). [Govt. of France (Reviewer's comment ID #: 2010-124)]	accepted
TS-658	A	30:29	30:29	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle (see chapter 10, p4, lines 6-9 and p16, lines 46-48; see also TS p52, line 44). [Serge PLANTON (Reviewer's comment ID #: 199-6)]	accepted
TS-659	A	30:46	30:49	Studies during the late 1960s and early 1970s by Herbert Riehl pointed to changing number of rain days being an indicator of changing mean rainfall and the mean rainfall of each rain day. That is, not just a shift in the mean but also a change in the frequency distribution. A stronger statement than 'simple statistical reasoning' is warranted. [William Kininmonth (Reviewer's comment ID #: 128-43)]	No specific language suggested. Believe the statement is correct as it stands.
TS-660	A	30:50	31:2	Is it true that a normal distribution gives a good fit to the extremes of "many variables"? This is certainly not the case for rainfall. A list consisting of examples of variables where the normal distribution provides a good fit to extreme values is needed in figure T-S Box 3.4. [Govt. of Australia (Reviewer's comment ID #: 2001-101)]	Text has been edited such that "many" has been replaced with "some." Box is intended to be schematic; variable-by-variable evaluation of the shape of distributions not warranted here.
TS-661	A	30:50	31:2	TS Box 3.4: Why is only one sentence devoted to changes in the shape of the distribution? It is unlikely that the shape of the distribution will remain the same, and I think both the shift in the mean and change in the shape should be shown in the figure, as in the TAR. Otherwise one could get the impression that if the mean doesn't change (which is the case in many regions for precipitation), nothing will change, which is unlikely to be correct. Also it says that the distribution is gaussian for many variables. For one of the most important quantities, precipitation, it's not, and it's bounded at zero. So I would recommend mentioning several distributions, or not mentioning any of them, and just say it's an illustration case for a distribution. [Reto Knutti (Reviewer's comment ID #: 133-17)]	Box is intended to be schematic; variable-by-variable evaluation of the shape of distributions not warranted here. Text states that "Changes in the variability or shape of the distribution can complicate this simple picture."
TS-662	A	31:15	31:15	Change "so some" to "As a result, some" [Michael MacCracken (Reviewer's comment ID #: 152-139)]	Edited
TS-663	A	31:18		Table TS-4. The "confidence" terms as defined in the footnote to this table are not the same as stated in the Technical Summary (TS-4, line 24). Maybe "high confidence" could be interpreted the same as defined in TS-4 table, but SPM uses "Moderate" whereas TM uses "Medium" confidence. (The same comment as given for SPM). [A. Brett Mullan (Reviewer's comment ID #: 182-6)]	Usage is now consistent
TS-664	A	31:22	31:23	Remove italics in column headers. [Govt. of Australia (Reviewer's comment ID #: 2001-102)]	Accepted
TS-665	A	31:22	31:23	Should tropical cyclones be included in last cell (High sea level events)	Believe current text is clear; see text

				[Govt. of Australia (Reviewer's comment ID #: 2001-103)]	accompanying this.
TS-666	A	31:22	31:26	Box TS 1.1 provides the obvious definition for "More likely than not," i.e., more than 50% likelihood, but this is too nebulous a definition to be useful. Since "likely" is defined as >66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means. [Lenny Bernstein (Reviewer's comment ID #: 20-34)]	See box on uncertainty
TS-667	A	31:22	31:26	Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities. [Lenny Bernstein (Reviewer's comment ID #: 20-35)]	Rejected. More likely than not is not the same as unknown.
TS-668	A	31:22	31:26	Box TS 1.1. defines "High confidence" as about 8 out of 10 chances of being correct, and "medium confidence" as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term -- moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions, i.e., "It is our judgment that this statement has 8 out of 10 chances of being correct because ..." would be useful. [Lenny Bernstein (Reviewer's comment ID #: 20-36)]	Text edited
TS-669	A	31:22	31:26	The basis for the assessment that it is likely that the increase in warm temperature extremes and decrease in cold temperature extremes is attributable to human activities presented in Table SPM-1, and again on Pg. 11, lines 20-22, is unclear. The underlying text for this topic is section 9.4.3.2, which presents the results of modeling studies which show that including anthropogenic effects "improves the simulation of these changing temperature extremes", but stops well short of attributing those changes to anthropogenic effects. Unless a clear logic can be provided for attributing changes in temperature extremes to human activities, the assessment should be that it is unknown whether these changes can be attributed to human activities. [Lenny Bernstein (Reviewer's comment ID #: 20-37)]	Rejected. Believe text in chapter 9 is clear.
TS-670	A	31:22	31:26	The SPM does not provide a basis, either in Table SPM-1 or in subsequent text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. [Lenny Bernstein (Reviewer's comment ID #: 20-38)]	See ch 10 and 11. Believe this is clear.
TS-671	A	31:22	31:26	regarding tropical cyclones, both assessments of "more likely than not" are misleading and should be changed to "about as likely as not." This would be consistent with the terminology defined on TS-4. The underlying chapter states, "Nonetheless, detection and attribution of observed changes in hurricane intensity or frequency to external influences remains difficult given deficiencies in theoretical understanding of tropical cyclones, their	Accepted in part. mid-latitude cyclone statement changed. Believe text re tropical cyclones is clear and appropriate.

				modelling, and their long-term monitoring." Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is inappropriate. Mid and high level cyclones should also be changed. [Howard Feldman (Reviewer's comment ID #: 70-2)]	
TS-672	A	31:22	31:23	Does the increase in high sea level events indicate an absolute increase, or that the high extremes increase more than the mean sea level? Please clarify. [Govt. of Finland (Reviewer's comment ID #: 2009-1)]	Response: In many cases the secular changes in extremes are similar to the changes in sea-level. Text edited, also. See section 5.5.2.7
TS-673	A	31:22	31:23	the terms used in the third column of table TS-4 are only partly explained in Box TS. 1.1, please add explanations for "more likely than not", [Govt. of Germany (Reviewer's comment ID #: 2011-246)]	Now covered in Box TS-1.1
TS-674	A	31:22	31:26	The definition of "More likely than not" i.e., more than 50% likelihood, is vague, since it gives no indication of an upper bound. WG I needs to either provide an upper bound for the term "more likely than not" or not use it. [Jeff Kueter (Reviewer's comment ID #: 137-34)]	See Box TS 1.1
TS-675	A	31:22	31:26	Change the evaluation of human influence on mid to high latitude cyclones to "unknown." The current evaluation "more likely than not (>50% likelihood) but with low confidence (2 out of 10 chances of being correct)" indicates a high level of uncertainty about the conclusion. Especially in the SPM and TS, WG I has an obligation to be precise about what is known and what is not known. The low level of confidence assigned to this conclusion indicates that it is still unknown. [Jeff Kueter (Reviewer's comment ID #: 137-35)]	Reviewer has confused 2 lines in Table
TS-676	A	31:22	31:26	The definitions of confidence levels given here only add to the confusion over WG I's treatment of uncertainty. High confidence is defined differently from the definition provided in Box TS 1.1, and a new term, moderate confidence, is introduced. Should moderate confidence be considered equivalent to medium confidence, which is defined in Box TS 1.1? Either find different terminology for these terms or use the definitions provided in Box TS 1.1. [Jeff Kueter (Reviewer's comment ID #: 137-36)]	Text edited. Revised to just use likelihood language and usage is fully consistent with Box TS.1
TS-677	A	31:22	31:26	What is the basis for the findings that it is likely (66 to 90% probability) that warm temperature extremes have increased and cold temperature extremes have decreased? The text refers the reader to Section 9.4, which, on Pg. 9-34, lines 3-5, states that including anthropogenic effects improves the simulation of extreme events. No indication of how much improvement occurs or why such improvement should be taken as a strong indicator of human influence. Either make a more compelling argument supporting this finding, or change the assessment to unknown. [Jeff Kueter (Reviewer's comment ID #: 137-37)]	Reviewer has confused observations and attribution. Question addressed in ch 3.

TS-678	A	31:22	31:26	The TS does not provide a basis, either in this table or in the text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. [Jeff Kueter (Reviewer's comment ID #: 137-38)]	Rejected. See extensive discussion in Chapter 10 and 11. Reviewer appears to be confused because projection material is later in the TS. We feel that should be obvious.
TS-679	A	31:22	31:22	In Table SPM-1 and the entry for "Tropical cyclones", in the third column the phrase "(but with low confidence)" should be dropped--there is strong theoretical evidence that this should be the trend, and it is occurring. The confusion comes, perhaps in that the rate of change is faster than the models (doing very limited and idealized studies) suggest. But, climate change has to be contributing to this change--no one is suggesting it operates in the opposite sense. Then in the fourth column and in lines 7-9, it seems overly cautious to say "Moderate." One has theory on one's side in addition to the model simulations--with much more moisture in the air and with the additional CO2 tending to slightly stabilize the troposphere, it seems very clear that more energy will be needed--and much more is available, so one has to get intensification--and this is being seen. [Michael MacCracken (Reviewer's comment ID #: 152-140)]	Text has been modified.
TS-680	A	31:22	31:22	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 on page 171 of Chapter 3)? [Melinda Marquis (Reviewer's comment ID #: 162-149)]	Rejected. This is the global average.
TS-681	A	31:22	31:26	The SPM does not provide a basis, either in Table SPM-1 or in subsequent text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. [Govt. of United States of America (Reviewer's comment ID #: 2023-934)]	See TS-678
TS-682	A	31:22	:26	Box TS 1.1 provides the obvious definition for "More likely than not," i.e., more than 50% likelihood, but this is too nebulous a definition to be useful. Since "likely" is defined as >66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means. [Govt. of United States of America (Reviewer's comment ID #: 2023-930)]	See comment TS-60
TS-683	A	31:22	:26	Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities. [Govt. of United States of America (Reviewer's comment ID #: 2023-931)]	Rejected. The judgement that a particular effect is more likely than not is not the same as unknown. Reasoning is given in the chapter.
TS-684	A	31:22	:26	Box TS 1.1. defines "High confidence" as about 8 out of 10 chances of being correct, and	edited

				"medium confidence" as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term -- moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions, i.e., "It is our judgment that this statement has 8 out of 10 chances of being correct because ..." would be useful. [Govt. of United States of America (Reviewer's comment ID #: 2023-932)]	
TS-685	A	31:22	:26	The basis for the assessment that it is likely that the increase in warm temperature extremes and decrease in cold temperature extremes is attributable to human activities presented in Table SPM-1, and again on Pg. 11, lines 20-22, is unclear. The underlying text for this topic is section 9.4.3.2, which presents the results of modeling studies which show that including anthropogenic effects "improves the simulation of these changing temperature extremes", but stops well short of attributing those changes to anthropogenic effects. Unless a clear logic can be provided for attributing changes in temperature extremes to human activities, the assessment should be that it is unknown whether these changes can be attributed to human activities. [Govt. of United States of America (Reviewer's comment ID #: 2023-933)]	Edited; see chapter 9 for detailed assessment.
TS-686	A	31:22	:26	Change the evaluation of human influence on mid to high latitude cyclones to "unknown." The current evaluation "more likely than not (>50% likelihood) but with low confidence (2 out of 10 chances of being correct)" indicates a high level of uncertainty about the conclusion. Especially in the SPM and TS, WG I has an obligation to be precise about what is known and what is not known. The low level of confidence assigned to this conclusion indicates that it is still unknown. [Govt. of United States of America (Reviewer's comment ID #: 2023-935)]	edited
TS-687	A	31:22	:26	The definitions of confidence levels given here only add to the confusion over WG I's treatment of uncertainty. High confidence is defined differently from the definition provided in Box TS 1.1, and a new term, moderate confidence, is introduced. Should moderate confidence be considered equivalent to medium confidence, which is defined in Box TS 1.1? Either find different terminology for these terms or use the definitions provided in Box TS 1.1. [Govt. of United States of America (Reviewer's comment ID #: 2023-936)]	edited
TS-688	A	31:22	:26	The TS does not provide a basis, either in this table or in the text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. [Govt. of United States of America (Reviewer's comment ID #: 2023-937)]	Rejected, see chapter 9
TS-689	A	31:42		supplement: With supplementary statistical analyses it is shown an increase of frequency of special extreme events in some regional scales [Govt. of Germany (Reviewer's comment ID #: 2011-88)]	Rejected, too vague to include. Unclear what is meant by special extreme events or some regional scales.
TS-1334	B	32:1	32:3	This is false. Paleoclimatic studies generally depend on a very few number of proxies. The millennial reconstructions are dependent on a very few proxies and are jointly vulnerable to	This is not a specific reference to the 1000 year record. Text clarified.

				errors in bristlecones and tree ring selections. [Stephen McIntyre (Reviewer’s comment ID #: 309-3)]	
TS-690	A	32:12	:14	The stationary relationships that exist today are used to extrapolate back in time, which also is an important research issue that should be mentioned here. Also, changes to tree-growth (due to CO2 enrichment and nitrogen fertilization) impacts the interpretation of these proxies. Also, in some areas, trees that were previously temperature-limited are becoming more moisture-limited (Briffa et al. 1998). An effort needs to be made to review the potential impact of these changes on the climate-tree growth relationship and how it influences climatic reconstructions. The concern also applies to many other biological proxies. [Govt. of United States of America (Reviewer’s comment ID #: 2023-938)]	Text has been edited. Divergence issue now mentioned.
TS-691	A	32:17	32:17	Insert after "temperatures", "as measured by the unreliable surface record" [VINCENT GRAY (Reviewer’s comment ID #: 88-1955)]	Rejected. No basis given for suggestion, see previous responses to earlier comments re surface record
TS-692	A	32:17	32:20	The basis for this statement is unclear. The reasons for existing uncertainties have as much to do with possible limitations in the retention of low-frequency variability by certain proxies (e.g. tree-rings) as they have to do with limitations the available spatial network of proxy information. If the low-frequency information in proxies such as corals and ice cores--which give us information outside the extratropical land areas and during seasons other than summer---is more reliable than the low-frequency information in proxies such as tree-rings--which are indeed more plentiful, but confined largely to the extratropical land areas, and providing information limited to growing season conditions which in many cases relate to summer temperature---then it is possible that we have better low-frequency information from the regions outside the continental centers, and during seasons other than summer. It is impossible to reject this possibility based on our current understanding, and thus the statement in question as it currently stands is not entirely supportable. [Michael Mann (Reviewer’s comment ID #: 156-53)]	Rejected. The reviewer’s comments amount to speculation as to the possible low-frequency fidelity of some tropical proxies. The sense of the current statement, i.e., that it is based on interpreting proxy data that come predominantly from extra-tropical, land areas and that many respond mostly to summer, rather than cold-seasonal temperatures, is clear. It is true that it is not possible to firmly reject the possibility that low-frequency temperature information is “better represented” in tropical region proxies compared to those from the extra-tropics. However, there is little or no evidence that demonstraties this. The reviewer concludes their comment by stating that the statement in question is “not entirely supportable” in its current form, but this is consistant with the “very likely” qualification currently attached. No change to the text is , therefore, deemed appropriate.
TS-693	A	32:17	33:18	The suggestion that the temperatures of the second half of the 20th century were 'likely' warmer than any 50-year period in the past 1000 years indicates misplaced confidence. The statement conflicts with page 32, lines 34-41, which, although based on limited data,	Rejected - Statement is not inconsistent with 32:34 – 32:41

				indicates significant multi-centennial variability of Northern Hemisphere temperature and at least one study indicating warmer conditions of the 11th century than previously acknowledged. [William Kininmonth (Reviewer's comment ID #: 128-44)]	
TS-694	A	32:18	32:18	The TS (page 32, line 18) says that NH temps of the second half of the 20th century ... and likely the warmest in the past 1,000 years, as does the SPM (page 9, line 24). But Ch. 6 (RF&KU, page 41, line 41) cites past 1,300 years. Please make consistent in all (three) places. [Melinda Marquis (Reviewer's comment ID #: 162-128)]	Accepted. TS now uses 1300 years as does chapter.
TS-695	A	32:19	32:19	Insert after "years" . "This likelihood disappears if contemporary proxy measurements are considered, or if satellite or radiosonde records are used." [VINCENT GRAY (Reviewer's comment ID #: 88-1956)]	Rejected. Not relevant to this discussion of surface temperature. See also earlier discussion of new upper air data.
TS-696	A	32:19	32:19	After "years" add "(and even longer)" [Michael MacCracken (Reviewer's comment ID #: 152-141)]	Changed to 'at least'
TS-1335	B	32:20	32:20	For the millennial climate studies, this is too strong. The studies rely on tree rings except Moberg, where he relies on uncalibrated proxies. [Stephen McIntyre (Reviewer's comment ID #: 309-4)]	Rejected – text clearly listing tree rings as one of several examples.
TS-697	A	32:30	32:30	Add at end "To be perfectly honest, the Northern Hemisphere sites are also pretty limited as well" [VINCENT GRAY (Reviewer's comment ID #: 88-1957)]	Rejected. Figure shows distribution of sites explicitly.
TS-698	A	32:36	32:36	Change “cooler temperatures” in “ lower temperatures” or “cooler conditions” [Aristita Busuioc (Reviewer's comment ID #: 35-10)]	accepted
TS-699	A	32:37	32:38	The coldest curve in Figure TS-23(b) is often around -1 °C and is therefore below the lower uncertainty bound of Figure 2.20 of the TAR. [David Parker (Reviewer's comment ID #: 195-89)]	Accepted, text changed
TS-700	A	32:40	32:41	Move "in the 11th century" to after "weather conditions" [Michael MacCracken (Reviewer's comment ID #: 152-142)]	edited
TS-701	A	32:41	32:41	Add at end "which means that they may have exceeded recent maximum observed temperatures". [VINCENT GRAY (Reviewer's comment ID #: 88-1958)]	Rejected, not correct, see figure.
TS-702	A	32:47	32:48	The solubility of CO ₂ varies with temperature, suggesting one explanation for the correlation between historic CO ₂ and temperature. Reverse the order of the sentences and replace the now second sentence with: "Although changing CO ₂ concentration is an amplifying feedback for global temperature CO ₂ solubility in water varies with temperature and CO ₂ concentrations would also be expected to follow global temperature variations." [William Kininmonth (Reviewer's comment ID #: 128-45)]	Rejected. The statement in the TS is correct. The available data suggest that atmospheric CO ₂ changes acted as an amplifying feedback, but not as the initial trigger for glacial-interglacial climate variations. Changes in the solubility of CO ₂ in seawater have likely contributed to past variations in atmospheric CO ₂ as discussed in chapter, but only a limited part of the

					measured CO2 variations can be explained by solubility changes.
TS-703	A	32:48	32:48	Add at end "However, both the proxy information and the ice core results are based on so few samples that they may not be suitable to represent global averages" [VINCENT GRAY (Reviewer's comment ID #: 88-1959)]	Rejected. See extensive discussion in chapter. Text here already is clear regarding uncertainties.
TS-704	A	32:50	32:50	Add at beginning "The limited" [VINCENT GRAY (Reviewer's comment ID #: 88-1960)]	Rejected, see TS-703
TS-705	A	32:52	32:52	Add at end "so the "averages" may not be truly "global" [VINCENT GRAY (Reviewer's comment ID #: 88-1961)]	Rejected, see TS-703
TS-706	A	32:54	33:20	It would be nice if the paleo chapter were providing an analysis of the temperature changes over the Arctic for the early 20th century--showing that the early 20th century warming was mainly in the North Atlantic sector. The Arctic study had a lot of Indigenous knowledge, but evidence about new bird sightings, etc. is also available. [Michael MacCracken (Reviewer's comment ID #: 152-143)]	Rejected. Bird sightings appropriate for WG2; not assessed in WG1. Early 20 th century is not paleo.
TS-707	A	33:5	33:10	Is it really clear that the ocean circulation changed quickly--is there sufficient temporal resolution in the sediment data? While one could get large and rapid changes up on the Greenland Ice Sheet with changes in atmospheric circulation, does this really translate into sudden change in the ocean circulation? I ask this for it really needs to be made clear whether this is a potential problem with the models (which do not show such abrupt changes) with regard to observations. [Michael MacCracken (Reviewer's comment ID #: 152-144)]	Noted. Concerning resolution: changes within a few decades are documented for the surface ocean, resolution for deep ocean changes is less. We do not see an indication of systematic deficiency in models here, "which do not show such abrupt changes" - some models we cite in our chapter do.
TS-708	A	33:7		need to define 'rapidly' - 10 yrs, 100 yrs? [David Rind (Reviewer's comment ID #: 214-8)]	Statement is intended to be generic and relate to "abrupt" as used in previous sentence. The cited section 6.4 provides context and limits to understanding of time scales. No change made.
TS-709	A	33:7		Sea-level changes are also measured locally against land, so can be affected by land tectonic movements. [Govt. of United States of America (Reviewer's comment ID #: 2023-939)]	Repeats an earlier comment with incorrect page/line numbers here. Records take this into account.
TS-710	A	33:11	33:13	It would be helpful to give an indication of the time scales of 20 ppm changes. Resolution in some of the ice cores is at best centuries (or even millennia) so it would be helpful to know the timescales to understand if these changes were really abrupt. [Michael MacCracken (Reviewer's comment ID #: 152-145)]	Text edited, see chapter for details
TS-711	A	33:17	33:20	The statement linking Arctic warming to orbital forcing is inconsistent with the statement in Box TS 2.1 (TS page 13 paragraph 2 lines 10-13), which states no comprehensive mechanistic explanation for linking climate variations and orbital timescales. Suggest delete the clause "which was driven by orbital forcing".	Text has been edited

				[William Kininmonth (Reviewer's comment ID #: 128-46)]	
TS-712	A	33:18	33:18	Change "is believed" with standard terminology of uncertainty (see uncertainty guidance), may be "likely" is appropriate. [Aristita Busuioc (Reviewer's comment ID #: 35-11)]	Text has been edited
TS-713	A	33:19	33:19	Change "Antarctic" to "West Antarctic" [Michael MacCracken (Reviewer's comment ID #: 152-146)]	Rejected. No basis given for this statement.
TS-714	A	33:32	33:32	Add at end "which includes the possibility that the changes are not related to greenhouse gas increases, but to such factors as land-use change and urban development". [VINCENT GRAY (Reviewer's comment ID #: 88-1962)]	Rejected. No basis given.
TS-715	A	33:36		It is proposed to use the following language: ... contributions of recent climate change has further increased considerable... [Govt. of Austria (Reviewer's comment ID #: 2002-35)]	Believe current text is clearer
TS-716	A	33:37		Delete "in part" and remove the comma [Richard Soulen (Reviewer's comment ID #: 248-30)]	Disagree. The additional warm years are very important and need to be highlighted. Copy editing to deal with commas and the like.
TS-717	A	33:41	33:41	Insert "many" after "although" [VINCENT GRAY (Reviewer's comment ID #: 88-1963)]	Text edited
TS-718	A	33:43	33:43	Insert after "signal", "(which need not involve an influence of greenhouse gases)" [VINCENT GRAY (Reviewer's comment ID #: 88-1964)]	Rejected. No basis given. Would not be consistent with chapter.
TS-719	A	33:43	33:45	Milly et al (2005) showed that the correlation between ensemble-model streamflow trends and observed streamflow trends was larger than could readily have been generated by chance (the latter being estimated from models that showed good agreement in at-site variances), I.e., Milly et al (2005) detected streamflow change above internal variability and they showed it was consistent with model simulations of forced climate change. It might therefore be appropriate here to say that "An anthropogenic signal has now emerged in more aspects of the climate system..., as well as continental scale temperature trends, THE PATTERN OF STREAMFLOW TRENDS,..." [P.C.D. Milly (Reviewer's comment ID #: 179-28)]	Rejected. Statement can not be made with sufficient confidence to justify inclusion in the TS.
TS-720	A	33:43	:45	An anthropogenic cause for the circulation change in the atmosphere is suggested but not really proven; note that there has been a recent downturn in the NAO/AO index the past couple of years. A global ocean temperature change had to be available prior to AR4, given that global surface air temperature reconstructions were being performed (and the global warming difference between that in the TAR and that in AR4 is not large). The TAR had already identified changes in Arctic sea ice extent, as well. [Govt. of United States of America (Reviewer's comment ID #: 2023-940)]	Taken into account. Text of paragraph has been edited. Important to draw the distinction between observed changes cited by the reviewer and those formally attributed that are discussed here as new since TAR.
TS-721	A	33:44	33:44	changes in ocean temperature and ocean heat content are for all practical reasons proportional, so only one should be listed here. [Reto Knutti (Reviewer's comment ID #: 133-30)]	accepted

TS-722	A	33:44	33:44	Insert "surface" after "global mean". [David Parker (Reviewer's comment ID #: 195-90)]	No, the language used helps to include e.g., tropopause changes and stratosphere which have also been used
TS-723	A	33:50	33:50	Replace "anthropogenic" by "human-induced" and delete "is widespread and" [VINCENT GRAY (Reviewer's comment ID #: 88-1965)]	Rejected. No basis given.
TS-724	A	33:50	33:50	Insert after "surface" "which is attributable to land-use changes and urban development" [VINCENT GRAY (Reviewer's comment ID #: 88-1966)]	Rejected. No basis given.
TS-725	A	33:50	33:51	May be modified to add the degree of confidence. It could perhaps be written as "Anthropogenic warming of the climate system is widespread and can be detected with high confidence in temperature observations taken at the surface, in the atmosphere and in the oceans." [Govt. of India (Reviewer's comment ID #: 2013-7)]	Including the free atmosphere means we cannot say this in a blanket way across all variables. Confidence is lower for the free atmosphere, but the combined statement is the point here.
TS-726	A	33:51	33:51	Insert after "atmosphere" "temperature records from satellites and radiosondes show no warming, human-induced or otherwise, over most of the record (1979 to 1997 for satellites and 1958 to 2002 for radiosondes)" [VINCENT GRAY (Reviewer's comment ID #: 88-1967)]	Rejected. See chapter 3 and responses to earlier comment of the same nature by this reviewer
TS-727	A	33:51	33:51	Insert after "atmosphere" there was no warming between 1979 and 1997 (satellite measurements), or between 1958 and 2002 (radiosondes)," [VINCENT GRAY (Reviewer's comment ID #: 88-1968)]	Rejected. See chapter 3 and responses to earlier comment of the same nature by this reviewer
TS-728	A	33:51	33:51	Delete "and", and capitalise "In the oceans", and add "a periodic heat behaviour has been observed which cannot be clearly related to human influence" [VINCENT GRAY (Reviewer's comment ID #: 88-1969)]	Rejected. See chapter 5 and responses to earlier comment of the same nature by this reviewer
TS-729	A	33:51	33:51	References should include 3.4. [David Parker (Reviewer's comment ID #: 195-91)]	Accepted
TS-730	A	33:51	33:51	The term "free atmosphere" needs to be defined. [Govt. of United States of America (Reviewer's comment ID #: 2023-941)]	Glossary
TS-731	A	33:53	34:8	There is a basic inconsistency between the statement that most models reproduce internal variability of the climate system quite well (33, lines 56-57) and the statement that uncertainties remain in estimates of internal variability (34, lines 4-5). These are in contrast to TAR where the SPM indicated that, based on models, the climate system has only limited internal variability. Logic demands an acknowledgement that the role of internal variability is not well reproduced in computer models. [William Kininmonth (Reviewer's comment ID #: 128-47)]	Rejected. Text does not imply perfection.
TS-732	A	33:53	34:8	The reasons for confidence and/or uncertainty in the attribution of climate change to human activities are one of the most important results from WG I. We suggest that this information be highlighted in a table or box. [Jeff Kueter (Reviewer's comment ID #: 137-39)]	Rejected. Believe issue is well highlighted here already.
TS-733	A	33:55	33:57	Delete from "Model" to "well" I completely disagree with the sentence.	Rejected. No basis given

				[VINCENT GRAY (Reviewer's comment ID #: 88-1971)]	
TS-734	A	33:56	:57	Instead of the awkward phrase 'natural internal variability' why not call it 'unforced variability'? Regardless of what it is called, there is no proof that models produce the observed variability on decadal time-scales because we do not know what the real world values are - the document itself says there is no way to distinguish past 'natural internal variability' from past natural 'externally-forced' variability without a more complete record of past forcings. [Govt. of United States of America (Reviewer's comment ID #: 2023-942)]	Partly taken into account. Text has been edited as suggested. Second part of comment is rejected. Large body of literature on this topic; see chapter 9.
TS-735	A	33:57	34:1	With that in mind, there is no way to prove that past climate variations have been strongly influenced by external forcings (other than an occasional volcano). [Govt. of United States of America (Reviewer's comment ID #: 2023-943)]	Rejected. Large body of literature on this topic; see chapter 9.
TS-736	A	34:1	34:8	This is a good summary of the uncertainties in the attribution of climate change to human activities and should be retained and strengthened in subsequent drafts. It would be useful to have a table or box highlighting both the reasons for confidence and the uncertainties in attribution of climate change [Lenny Bernstein (Reviewer's comment ID #: 20-39)]	Rejected. Believe issue is well highlighted here already.
TS-737	A	34:1	34:1	Delete "strongly" [VINCENT GRAY (Reviewer's comment ID #: 88-1972)]	Rejected. No basis given
TS-738	A	34:1	34:8	The reasons for confidence and/or uncertainty in the attribution of climate change to human activities are one of the most important results from WG I. We suggest that this information be highlighted in a table or box. [Govt. of United States of America (Reviewer's comment ID #: 2023-945)]	Rejected. Believe issue is well highlighted here already.
TS-739	A	34:1	:8	This is a good summary of the uncertainties in the attribution of climate change to human activities and should be retained and strengthened in subsequent drafts. It would be useful to have a table or box highlighting both the reasons for confidence and the uncertainties in attribution of climate change [Govt. of United States of America (Reviewer's comment ID #: 2023-944)]	Rejected. Believe issue is well highlighted here already.
TS-740	A	34:2		The uncertainties in the external forcing are not due primarily to uncertainties in the model response, but to lack of suitable observations. In addition, added to the complicating factor for the uncertain model response are the uncertain observations of what really happened. [Govt. of United States of America (Reviewer's comment ID #: 2023-946)]	Noted, but this text does not imply that such uncertainties do not exist. Not what is being referred to here.
TS-741	A	34:4	:7	These lines back up the point made for page 33, lines 56-57; in fact, they make essentially the same point. [Govt. of United States of America (Reviewer's comment ID #: 2023-947)]	33:56-57 says that models do many things rather well. 34:4-7 says that we are aware of uncertainties and names several. These are not the same point.
TS-742	A	34:10	34:21	Fig TS26 would be better coming before TS 25, - This way the reader goes from the global to the regional. [Govt. of Australia (Reviewer's comment ID #: 2001-104)]	This has been considered but present order retained.
TS-743	A	34:12	34:18	Delete whole paragraph, which is untrue. The only "warming" that needs to be explained is that of the surface record, which can be almost entirely attributed to socioeconomic factors	Rejected. See chapter 3 and responses to earlier comment of the same nature

				such as population increase, energy usage and urban development (see McKittrick and Michaels 2004 Climate Research Vol 26 pages 159-173) [VINCENT GRAY (Reviewer's comment ID #: 88-1973)]	by this reviewer
TS-744	A	34:12	34:18	These statements and the material on which they are based are logically flawed and cannot be sustained. Material reported in this assessment report supports 1) tropical SST have increased since the 'climate shift' of the Pacific Ocean of 1976-77, leading to enhanced heat and latent energy exchange with the atmosphere; 2) the Hadley Cell circulation has increased (in response to the SST forcing); 3) a poleward shift and an increase in the strength of the surface westerly winds (in response to generation of relative atmospheric angular momentum by the Hadley circulation;) 4) a middle latitude strengthening of the meridional temperature gradient in response to the strengthened westerly winds; 5) a greater poleward transport of energy by the enhanced atmospheric circulation (the Hadley circulation and the stronger westerly winds); 6) accumulation of heat in middle latitude and polar regions as evidenced by warmer surface temperatures, raising of the altitude of freezing levels and melting of land and sea-ice. The preceding pattern is a reflection of natural internal variability although the cause of the warmer tropical SST and, globally, ocean heat content is conjectural. A reduction in the MOC would point to a reduction in entrainment of cold subsurface water across the thermocline and a warming of the tropical ocean mixed layer but there is no conclusive evidence for this. Notwithstanding, long-period atmosphere-ocean feedbacks that lead to changes in the intensity of the MOC (either density driven or wind driven) cannot be discounted. So long as the IPCC authors continue to deny internal variability of the climate system due to atmosphere-ocean interactions then they will have to rely on 'forcing' mechanisms to explain recent climate change. It should be noted that Trenberth and Caron (2001) computed the import of energy to the middle and high latitudes from the tropics by the atmospheric circulation as 1.6×10^{23} joules per year. A sustained increase of only one percent for two decades would greatly exceed the observed heat taken up by ice melt and continental heating. Whether the increase in tropical SST is a result of more El Nino events or to other longer term changes in ocean circulation, the possibility of internal variability that changes how solar insolation is processed in the ocean surface layer cannot be dismissed. [William Kininmonth (Reviewer's comment ID #: 128-48)]	Rejected. Global ocean heat content has increased.
TS-745	A	34:12	34:19	What is the basis for the claim that the temperature changes of the past half century took place at a time when natural forcing would be expected to have produced cooling? The discussion in Section 2.7 (Pg. 2-53, lines 39-53) states that there has been no trend in solar insolation for the past 25 years. Later in the section (Pg 2-58, lines 55-56) the authors state that stratospheric aerosol concentrations are at their lowest level since global measurements began in the late 1970s. This would seem to indicate that during the period of the fastest temperature rise, there was no cooling trend due to either solar variability or volcanic activity. [Jeff Kueter (Reviewer's comment ID #: 137-40)]	Rejected. See Figure TS-26. More information now given in ch 2. Episodic nature of volcanoes implies memory and the end point cannot be taken in isolation
TS-746	A	34:12	:13	This statement is written awkwardly and is hard to understand. Possible alternative: "It is	Rejected. Believe text is clear as it

				highly likely that external forcing is needed to explain the warming observed...” [Govt. of United States of America (Reviewer’s comment ID #: 2023-948)]	stands.
TS-747	A	34:13	34:13	Terms 'external forcing' and 'internal forcing' have not been explained in TS; and are not dealt with well in the Glossary. (Indeed in Glossary human-induced forcing is separate from external forcings). [Govt. of Australia (Reviewer’s comment ID #: 2001-105)]	Explained better in text now; also in glossary
TS-748	A	34:13	34:14	The basis for saying that the temperature changes of the past half century took place at a time when natural forcing would be expected to have produced cooling is far from clear. Section 2.7 (Pg 2-53, lines 39-53) indicates no trend in the past 25 years in solar irradiance. It also discusses two major volcanic eruptions (El Chichon in 1982 and Pinatubo in 1991), but then concludes that stratospheric aerosol concentrations are at their lowest concentrations since the satellite era and global coverage began in the late 1970s (Pg 2-58, lines 55-56). These facts would seem to indicate no effect of solar variability and a declining effect of volcanic activity for that period. [Lenny Bernstein (Reviewer’s comment ID #: 20-40)]	See TS-745
TS-749	A	34:13	34:13	Clarify, which external forcing is meant [Govt. of Germany (Reviewer’s comment ID #: 2011-89)]	Define in glossary. Cannot specify what specific forcing here. The point is that there must be a source of forcing.
TS-750	A	34:23	34:24	Delete from "It is very likely" to ".last 50 years" It is simply untrue. The only "observed warming" that needs to be explained in that of the surface record, and that is mainly due to socioeconomic factors (McKittrick and Mischeals l.c.) Other temperature records either show no warming, or in the case of the satellite record, only very recently. [VINCENT GRAY (Reviewer’s comment ID #: 88-1974)]	Rejected. See chapter 3 and responses to earlier comment of the same nature by this reviewer
TS-751	A	34:23	34:23	Replace "greenhouse gas forcing has" by "socioeconomic factors have" [VINCENT GRAY (Reviewer’s comment ID #: 88-1975)]	Rejected. See responses to earlier comment of the same nature by this reviewer
TS-752	A	34:23	34:29	This statement is only logically true if internal variability is eliminated. However there is every reason to believe that non-linear interactions between the atmosphere and the ocean fluids is a primary cause of tropical ocean SST variations and enhanced export of heat from the tropics to polar regions by the atmospheric circulation. Tuning of computer models to seemingly plausible natural and anthropogenic forcing is not proof positive that greenhouse gases are the cause of observed global warming. [William Kininmonth (Reviewer’s comment ID #: 128-49)]	Rejected. Global ocean heat content has increased.
TS-753	A	34:26	34:27	Delete "counteracting greenhouse warming" [VINCENT GRAY (Reviewer’s comment ID #: 88-1976)]	Rejected. No basis given
TS-754	A	34:26	:27	How can the temporal evolution be a key point in understanding the aerosol forcing when we don't know what the aerosol evolution with time has been? [Govt. of United States of America (Reviewer’s comment ID #: 2023-949)]	Rejected. See TS page 10, lines 7-48. Indicates some confidence especially for interdecadal timescales (and this has improved since TAR). Will note this in 9.2.2.3.

TS-755	A	34:31	34:31	"Replace "Widespread warming" with "A periodic change" [VINCENT GRAY (Reviewer's comment ID #: 88-1977)]	Rejected. No basis given
TS-756	A	34:31	34:33	It is highly implausible that a small increase in the back radiation to the ocean surface will cause energy to penetrate through the surface mixed layer while intense solar radiation over millions of years is confined to a thin warm lens of the tropical oceans. The observed warming could arise from multidecadal and longer changes in the dynamics of the ocean surface layer, similar in character to what is observed on the interannual timescale with ENSO. [William Kininmonth (Reviewer's comment ID #: 128-50)]	Rejected. Mixing in the ocean distributes warming downwards.
TS-757	A	34:32	34:32	Replace "likely" by "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-1978)]	Rejected, no basis given.
TS-758	A	34:35	34:35	Replace 'likely' with 'very likely' for consistency with Chapter 5. [Govt. of Australia (Reviewer's comment ID #: 2001-106)]	Rejected. Reviewer seems to be confusing confidence in observation and attribution.
TS-759	A	34:35	34:35	Insert "recent" after "observed" [VINCENT GRAY (Reviewer's comment ID #: 88-1979)]	Rejected. This statement is applicable for longer times as well
TS-760	A	34:35	34:35	Replace "likely accounts" by "may account" [VINCENT GRAY (Reviewer's comment ID #: 88-1980)]	Rejected, no basis given.
TS-761	A	34:35	34:36	This sentence is misleading because it does not qualify the importance of specification of sub-grid scale processes in addition to the large scale physical laws. Suggest replace with: "Both models use the physical laws and parameterizations for the impact of sub-grid scale processes to make their predictions of how the atmosphere, oceans move, change temperature, etc". [William Kininmonth (Reviewer's comment ID #: 128-54)]	Rejected. Not appropriate for the point being made here
TS-762	A	34:36	34:36	Insert "possibly" before "consistent" [VINCENT GRAY (Reviewer's comment ID #: 88-1981)]	Rejected, no basis given
TS-763	A	34:39	34:39	Replace ".anthropogenic forcing has" by "Recent higher temperatures have" [VINCENT GRAY (Reviewer's comment ID #: 88-1982)]	Rejected, no basis given
TS-764	A	34:39	34:40	Delete from "Changes " to "warming". The evidence for "warming" is suspect, because it is mainly based on neighbouring land stations which may be greatly influenced by local heating. Satellite measurements indicate that the Arctic is cooling (see Figure 3.4.4) [VINCENT GRAY (Reviewer's comment ID #: 88-1983)]	Rejected, ch 3 clearly shows evidence for Arctic warming over the average of 65-90N
TS-765	A	34:39	34:40	The sentence has mixed tenses. 'Expected' implies the future and so 'observed' should be replaced by 'modeled' or 'projected'. [William Kininmonth (Reviewer's comment ID #: 128-51)]	Rejected. Expectation is based on physical reasoning.
TS-766	A	34:39	:41	The discussion under the comment in italics does not prove it, or really even comment on the observations. While 'warming' would be expected to decrease sea ice, we don't know how much the warming has influenced the current trend, as the transport influence on Arctic sea ice is undoubtedly a large component of the Arctic sea ice trend. It is uncertain	Taken into account. Text clarified.

				how much of the transport change is anthropogenically induced (the document says a 'part' of it). [Govt. of United States of America (Reviewer's comment ID #: 2023-950)]	
TS-767	A	34:40	34:40	enhanced Arctic warming (air temperatures) also increases in Atlantic layer ocean temperatures? [Roger Barry (Reviewer's comment ID #: 13-5)]	Warming can also be in the ocean
TS-768	A	34:40	34:40	Insert "have been made" after "improvements" [VINCENT GRAY (Reviewer's comment ID #: 88-1984)]	Believe text is clear as it stands
TS-769	A	34:41	34:41	Delete from "strengthen" to "conclusion" [VINCENT GRAY (Reviewer's comment ID #: 88-1985)]	Does not seem to help clarity and complicates construction
TS-770	A	34:44	36:10	This is an excellent summary of the strengths and weaknesses of climate models and should be retained in future drafts. [Lenny Bernstein (Reviewer's comment ID #: 20-41)]	Noted. Thank you.
TS-771	A	34:44	36:10	This summary of the strengths and weakness of climate models should be given further prominence in either a table or box. [Jeff Kueter (Reviewer's comment ID #: 137-41)]	It is a box already – thank you
TS-772	A	34:44	36:10	This is an excellent summary of the strengths and weaknesses of climate models and should be retained in future drafts. [Govt. of United States of America (Reviewer's comment ID #: 2023-951)]	Noted. Thank you.
TS-773	A	34:46	34:46	Delete "the primary tool" [VINCENT GRAY (Reviewer's comment ID #: 88-1986)]	Rejected. No basis given
TS-774	A	34:47	34:49	Change the sentence to read: "... Are fully analagous to the human induced perturbations expected over the 21st century". There are apparently historical natural radiative forcings, such as solar intensity variations and volcanoes. [William Kininmonth (Reviewer's comment ID #: 128-52)]	Accepted
TS-775	A	34:49	34:49	Delete "substantial" [VINCENT GRAY (Reviewer's comment ID #: 88-1987)]	Rejected, no basis given
TS-776	A	34:49	34:50	Delete from "increasing" in line 49 to "models" in line 50 [VINCENT GRAY (Reviewer's comment ID #: 88-1988)]	Rejected, no basis given
TS-777	A	34:55	34:55	Insert at end "But it does tend to universalise both biases and uncertainties" [VINCENT GRAY (Reviewer's comment ID #: 88-1989)]	Rejected, no basis given
TS-778	A	35:8	35:8	Replace 'despite the fact' with 'so much so' [Govt. of Australia (Reviewer's comment ID #: 2001-107)]	Text edited
TS-779	A	35:8	35:8	There is a strong need to include a line on AR4's assessment on performance of models with respect to their simulations of Asian Monsoons in this TS. This is of particular relevance to many countries in the South and South-East Asian region that are influenced by the numerous and will be impacted if there are any changes in the circulations. [Govt. of India (Reviewer's comment ID #: 2013-8)]	A general staement on precip will be added here (see also text in projections section. Sppace constraints preclude regional detail in this box.
TS-780	A	35:9	35:10	The present phrasing is confusing for the non-specialist reading the TS only: "the fact that	Text edited

				the artificial constraint of flux adjustment has been eliminated in most models" may be understood as a degradation of the quality of the models [Govt. of France (Reviewer's comment ID #: 2010-125)]	
TS-781	A	35:11	35:12	There is no evidence presented in the report that the simulation of marine stratocumulus (or any cloud type) has improved (See comment 3). [Keith Williams (Reviewer's comment ID #: 290-1)]	Taken into account. This reflects an error in the underlying Ch 8 text, and that text will be corrected.
TS-782	A	35:14	35:16	This is an heroic statement and if true would imply an ability predict tropical cyclone initiation. In reality tropical cyclones are initiated with varying frequency and intensity under a variety of large scale conditions. Seasonal prediction has some skill but much uncertainty and prediction of individual tropical cyclones is reliant on initial formation. Suggest deletion of the clause: "..., some models can simulate the large scale conditions necessary to infer their frequency and distribution". [William Kininmonth (Reviewer's comment ID #: 128-53)]	Taken into account. The second part of the first sentence is false. However the Chapter 8 text will be clarified and strengthened to show the basis of this sentence. Change to TS text not required.
TS-783	A	35:15	35:15	add after "resolved" "in the models" [Govt. of Germany (Reviewer's comment ID #: 2011-247)]	Rejected. Believe text is clearer and shorter as it is.
TS-784	A	35:16	35:17	It is incorrect to say that improved simulations have been achieved for the MOC - Figure 10.3.13 shows this to be so - hardly a glowing endorsement of the models' abilities in simulating the MOC! A more accurate statement would be - While some improvements have been made in simulating the ocean it is clear that critical components, such as the MOC, require further improvements [8.3.2.2, Fig. 10.3.13]. [Meric Srokosz (Reviewer's comment ID #: 250-3)]	Rejected. Compare TAR Table 8.2 and see underlying text in Ch 8.
TS-785	A	35:17		Please include a full explanation of MOC (meridional overturning circulation) as this abbreviation is used for the first time. [Govt. of Austria (Reviewer's comment ID #: 2002-36)]	See glossary
TS-786	A	35:23		Please include a full explanation of MJO (Madden-Julian Oscillation) as this abbreviation is used for the first time. [Govt. of Austria (Reviewer's comment ID #: 2002-37)]	See underlying chapter for more detail. Spelled out here
TS-787	A	35:27	35:27	Change "on these timescales" to "on many timescales"? [David Parker (Reviewer's comment ID #: 195-92)]	Taken into account. Changed to 'various' for clarity.
TS-788	A	35:30	35:31	"aspects of the hydrological cycle" is unnecessarily vague. What observables are we talking about here? [P.C.D. Milly (Reviewer's comment ID #: 179-29)]	Accepted. Text changed.
TS-789	A	35:32		modeling of LGM when used with input boundary conditions does not represent much of a test [David Rind (Reviewer's comment ID #: 214-9)]	Rejected. Braod consistency with temperature proxies is not programmed into the models.
TS-790	A	35:32		Modeling of LGM when used with input boundary conditions does not represent much of a test. [Govt. of United States of America (Reviewer's comment ID #: 2023-952)]	Rejected. Braod consistency with temperature proxies is not programmed into the models.
TS-791	A	35:35	35:35	Insert "respectively" after "projections"?	Noted. This paragraph substantially

				[Melinda Marquis (Reviewer's comment ID #: 162-115)]	rewritten.
TS-792	A	35:35	35:35	Insert "types of" after "Both." [Melinda Marquis (Reviewer's comment ID #: 162-116)]	Noted. This paragraph substantially rewritten.
TS-793	A	35:35	35:36	This sentence seems a bit too casual. Needs a bit of work. Also, do climate models produce "predictions," or just "projections"? [Melinda Marquis (Reviewer's comment ID #: 162-117)]	Noted. This paragraph substantially rewritten.
TS-794	A	35:36	35:36	Delete "and". [David Parker (Reviewer's comment ID #: 195-93)]	Noted. This paragraph substantially rewritten.
TS-795	A	35:41	35:51	Suggest for ease of understanding, that the paragraph be broken into two; the first dealing with the transition from weather models to climate models (ie the importance of forcings and boundary conditions against initial conditions) and the second dealing with the transition from global to regional climate projections. Suggested text: 'Climate models project the climate for several decades or longer into the future. Since the details of individual weather systems are not being tracked and forecast, the initial conditions are much less important. For climate projections the forcings and boundary conditions are of much greater importance. These conditions include the amount of sunshine reaching the earth, the amount of particles from volcanic eruptions in the atmosphere, and the amount of anthropogenic gases and particles in the atmosphere. For paleoclimate models, icesheets are considered boundary conditions in some studies. Small errors in the boundary conditions or the models, can lead to unreliable forecasts. [Question 1.2]' 'As the area of interest moves from global to regional to local, so the predictability decreases. Uncertainties and errors on the planetary scale generally become magnified and more dominant on the smaller scales, whether these are represented explicitly in the global model or simulated in an embedded regional climate model. A characteristic of the projections is that the ratio of the climate change signal to the internal climate variability is reduced as the space scale is reduced.' Line 43-44 'Expected climate change at smaller space and time scales.' could be deleted, as it adds little. [Govt. of Australia (Reviewer's comment ID #: 2001-108)]	Some editing has been done on this paragraph along the lines suggested but the basic structure seems clearer as it stands.
TS-796	A	35:41	35:43	Although the details of individual weather systems are not being tracked and forecast the cumulative effect of internal processing of energy exchange is vitally important for climate simulation and prediction. For example, as Trenberth and Stepaniak have demonstrated, individual weather systems are crucial for the poleward transport of energy by the atmospheric circulation over middle and high latitudes. Suggest including an additional sentence: "... much less important in this case. However it is crucially important that the weather systems are capable of simulating the poleward transport of heat, moisture and angular momentum as occurs in the atmosphere. Expected climate change" [William Kininmonth (Reviewer's comment ID #: 128-55)]	Noted, but not what is being said here. See chapter for this level of detail.
TS-797	A	35:48	35:48	Consider changing "amount" to "concentration" or "number."	Rejected. Amount used because optical

				[Melinda Marquis (Reviewer's comment ID #: 162-118)]	depth is important
TS-798	A	35:49	35:49	Consider changing "amount" to "concentration." [Melinda Marquis (Reviewer's comment ID #: 162-119)]	accepted
TS-799	A	35:50	35:50	I suggest deleting the sentence : "For paleoclimate models, ice sheets are considered boundary conditions in some studies. ", which is too marginal to deserve being recalled in the TS. [Govt. of France (Reviewer's comment ID #: 2010-126)]	Rejected. This is important to explain the differences between forcing, boundary conditions, etc., used on various timescales
TS-800	A	35:51	35:51	Replace "bad forecasts" with "unreliable projections". The latter is more consistent with IPCC terminology. [Govt. of Australia (Reviewer's comment ID #: 2001-109)]	Text edited
TS-801	A	35:51	35:51	Need to change "bad" to "poor"--this is not a moral judgment. [Michael MacCracken (Reviewer's comment ID #: 152-147)]	Text eddited
TS-802	A	35:53	35:56	These two statements are confusing at best. Climate models show skill for a few days when initiated with actual data because the atmospheric component is derived from numerical weather prediction models. Climate models show no skill beyond a few days and it is misleading to suggest that useful predictions on annual timescales can be made when it is acknowledged that such models represent ENSO poorly. The claim that previous climate predictions have been borne out by observations needs qualification - the historical global temperature record (observation) has been simulated by climate models regulated (tuned) by predetermined and seemingly plausible forcing factors and there are no independent forecasts validated by later observations. [William Kininmonth (Reviewer's comment ID #: 128-56)]	Text edited
TS-803	A	35:53	35:54	Should "prediction" be changed to "projection"? I thought models *projected.* [Melinda Marquis (Reviewer's comment ID #: 162-120)]	Text edited
TS-804	A	35:53	36:7	"Skill" is a term of art among modelers and does not have the same meaning for laymen. It would help if it were defined. [Lenny Bernstein (Reviewer's comment ID #: 20-42)]	Text edited
TS-805	A	35:53	36:1	Text does not flow well. Move the first sentence to be third (after "...commitments.").Change the fourth sentence to: "This also increases confidence..." [David Parker (Reviewer's comment ID #: 195-94)]	Text edited
TS-806	A	35:55	35:55	Insert "sometimes" after "been" [VINCENT GRAY (Reviewer's comment ID #: 88-1990)]	Rejected , no basis given
TS-807	A	36:1		Please explain "hindcast" skill as this seems to be a technical term [Govt. of Austria (Reviewer's comment ID #: 2002-38)]	Text edited
TS-808	A	36:2	:5	This discussion, while theoretically true, is misleading because the AR4 models have not been run as weather forecasting models - so this form of validation is not available. [Govt. of United States of America (Reviewer's comment ID #: 2023-953)]	Rejected. Some AR4 models have been tested in this way. See Section 8.4
TS-809	A	36:14	36:15	Delete "tropospheric warming and" I have pointed out many times that "tropospheric warming" as measured by satellites and radiosondes cannot be explained by any of the	Rejected, see ch 3 for updated information on the issue of upper air and

				various varieties of "anthropogenic forcing" There was no "warming", at least between 1979 and 1997 and the "warming in 1999 was due to El Niño. [VINCENT GRAY (Reviewer's comment ID #: 88-1991)]	surface temperature trends.
TS-810	A	36:14	36:18	These statements are only partially correct. As Riehl and Malkus (1958, On the heat balance of the equatorial heat zone. Geophysica v 6, No 3-4 pp503-538) have demonstrated, the depth of the troposphere and the height of the tropopause are determined by convective overturning. Air in the boundary layer rises buoyantly in protected deep convective towers until the environmental dry static energy is equivalent to the moist static energy of the boundary layer. A warmer and moister boundary layer will result in deeper convection and a higher tropopause. Thus the increased height of the tropopause and increased convective overturning (Hadley circulation) of recent decades is a direct outcome of increased tropical SST. As McPaden and Zhang (2002. Slowdown of the meridional overturning circulation in the upper Pacific Ocean, Nature v 415 pp603-608) the increase in tropical SST is likely due to a slowdown in upwelling of cold subsurface water into the surface mixed layer. It is likely, therefore, that the increase in the height of the tropopause and increase in the Hadley circulation of recent decades can be linked directly to the natural internal variability of the ocean and is not reliant on external forcing for a plausible explanation. It should also be noted that tropospheric temperature variations are of significant magnitude and highly correlated with ENSO SST - because of the interannual variability the direction and magnitude of trends are dependent on the length of record and period over which they are taken. [William Kininmonth (Reviewer's comment ID #: 128-57)]	Rejected. Literature does not support assertion that this is relevant to the scale considered here.
TS-811	A	36:17	36:18	Delete from "some uncertainty remains" to "record". This statement is quite unfair. The "scrutiny" of the tropospheric records has been so intense, that they are both far more accurate than the surface and long-term proxy records which seem to escape from the same level of scrutiny. [VINCENT GRAY (Reviewer's comment ID #: 88-1992)]	Rejected. See ch 3 for discussion of data on both upper air and surface
TS-812	A	36:17	36:18	This statement seems a bit in conflict with the earlier discussion indicating that everything about MSU has been resolved. [Michael MacCracken (Reviewer's comment ID #: 152-148)]	Rejected. Believe text is clear regarding the improvements and the remaining uncertainties. It is not perfect.
TS-813	A	36:20	36:20	Insert after "signal" , "which is mainly socioeconomic", [VINCENT GRAY (Reviewer's comment ID #: 88-1993)]	Rejected, see earlier comment by this reviewer on same topic.
TS-814	A	36:20	36:20	While a global warming signal in surface temperature has likely been detected in all continents except Antarctica its attribution is by no means certain. Suggest replace 'anthropogenic' by 'global warming'. [William Kininmonth (Reviewer's comment ID #: 128-58)]	Rejected. See ch 9 and revised text
TS-815	A	36:21	36:23	Delete from "The chance" in line 21 to "variability" in line 23. This statement makes no sense at all. [VINCENT GRAY (Reviewer's comment ID #: 88-1994)]	Rejected. No basis given. Basic probability and statistics implies that consistent changes like these are not random (e.g., not roll of the dice)

TS-816	A	36:23	36:24	Delete from "The ability" in line 23 to "TAR" in line 24 This statement is not supported by evidence [VINCENT GRAY (Reviewer's comment ID #: 88-1995)]	Rejected. See TS 815
TS-817	A	36:23	36:24	The statement in this sentence is only true if internal variability on multidecadal timescales is discounted. Suggest replace the words after 'stronger' with 'supporting evidence of human influence on global climate'. [William Kininmonth (Reviewer's comment ID #: 128-59)]	Rejected. Internal variability is not likely to show consistent behavior in so many different places, across hemispheres, etc.
TS-818	A	36:23	:25	It is not the ability to simulate temperatures, but to simulate temperatures accurately that can provide evidence; similarly it is not the difficulty in simulating temperatures changes in some parts of the world, but simulating them accurately that is the issue. [Govt. of United States of America (Reviewer's comment ID #: 2023-954)]	Accepted, accurately has been added
TS-819	A	36:25	36:25	Delete "However" and capitalise "Difficulties" [VINCENT GRAY (Reviewer's comment ID #: 88-1996)]	Rejected. Text is clear as it stands
TS-820	A	36:25	36:25	Replace "some" by "most" [VINCENT GRAY (Reviewer's comment ID #: 88-1997)]	Rejected, no basis given
TS-821	A	36:25	36:25	Insert "and change" after "variability" [VINCENT GRAY (Reviewer's comment ID #: 88-1998)]	Rejected, not the point being made
TS-822	A	36:31	36:41	The trends in the northern and southern annular modes are consistent with a net transfer of absolute angular momentum from the earth (ocean and land) to the atmosphere generated by a more intense Hadley circulation. The increased westerlies of Figure TS-27 are consistent with increased relative atmospheric angular momentum and the shift in the surface pressure patterns (mass shift from the higher latitudes to lower latitudes) is consistent with an increase in 'earth' atmospheric angular momentum. The observed increase in intensity of the mean cyclonic and anticyclonic systems is also consistent with increased amplitude of the planetary waves necessary to transport additional heat, moisture and momentum polewards (see Trenberth and Stepaniak, 2004). The wind and pressure changes are not consistent with a weakening of the meridional temperature gradient (stronger warming over the polar regions) as inferred by anthropogenic global warming. This paragraph should be reconstructed to better reflect the inconsistency of the model simulations. [William Kininmonth (Reviewer's comment ID #: 128-60)]	Rejected. The instrumentally-recorded warming over the last 30 years is largest at high northern latitudes (chapter 3). Text on line 40 to 42, page TS-36, describes differences between observations and model results.
TS-823	A	36:32	36:32	Replace "likely" by "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-1999)]	Rejected, no basis given
TS-824	A	36:36	36:36	Replace "detectable" by "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-2000)]	Rejected, no basis given
TS-825	A	36:38	36:38	Replace "likely" by "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2001)]	Rejected, no basis given
TS-826	A	36:46	36:46	This opening sentence is not supported by the statements that follow, particularly lines 54 to page 37 line 2. It is implied that regional rainfall patterns are strongly influenced by SST patterns but a link between anthropogenic forcing and regional SST patterns has not been	Taken into account. Paragraph has been modified.

				established. The statement 'making attribution to human influences complex' is surely an admission of no credible linkage. [William Kininmonth (Reviewer's comment ID #: 128-61)]	
TS-827	A	36:53	36:53	Change "not likely" in "unlikely" [Aristita Busuioc (Reviewer's comment ID #: 35-12)]	Accepted
TS-828	A	37:0	37:	Footnote. Add at end. "The fact that CO2 is highly unlikely ever to increase at the rate of 1% a year renders these studies virtually useless" [VINCENT GRAY (Reviewer's comment ID #: 88-2009)]	Rejected. 1% per year change in CO2 is not a scenario. It does, however, approximate the actual change in RF in the past several decades and is a mode of physics testing.
TS-829	A	37:1	37:2	Add 'including stability' after atmospheric circulation. [Govt. of Australia (Reviewer's comment ID #: 2001-110)]	Rejected. Could cause confusion with atmospheric stability, not what is meant.
TS-830	A	37:4	37:4	Insert after "forcings" , "including land-use changes and urban development" [VINCENT GRAY (Reviewer's comment ID #: 88-2002)]	Rejected. Unblanced since this is not the dominant forcing on global scales for the phenomenon discussed
TS-831	A	37:9	37:9	Replace "likely" by "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2003)]	Rejected, no basis given
TS-832	A	37:9	37:9	Insert after "improvements , "have been made" [VINCENT GRAY (Reviewer's comment ID #: 88-2004)]	See TS-833
TS-833	A	37:9	:11	This point has already been made on p. 34, lines 39-42. [Govt. of United States of America (Reviewer's comment ID #: 2023-955)]	Accepted
TS-834	A	37:10	37:10	Delete from "strengthen" to "conclusion" [VINCENT GRAY (Reviewer's comment ID #: 88-2005)]	See TS-833
TS-835	A	37:15	37:26	Given 1) the inability of computer simulations to represent internal variability, 2) the limitation to paleo-reconstruction of the climate record that likely damps of the actual signal in the reconstruction, and 3) the limited knowledge of solar and volcanic forcing, the attribution can only be speculative. 'Very likely' should be replaced by 'as likely as not' in line 16 and 'likely' by 'as likely as not' in line 24. [William Kininmonth (Reviewer's comment ID #: 128-62)]	Rejected. Uncertainties unlikely to lead to a spurious agreement between temperature reconstructions and forcing reconstructions as they are derived from independent proxies.
TS-836	A	37:16	37:16	Replace "vary likely" with "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2006)]	Rejected, no basis given
TS-837	A	37:17	37:20	"Such forcing" at the beginning of the sentence refers to "natural external forcing", so "the emerging greenhouse gas signal" should not be included in the list of what "such forcing" includes. [Adrian Simmons (Reviewer's comment ID #: 242-9)]	Accepted
TS-838	A	37:20	37:20	This paragraph seems to be about natural external forcing, but then includes the "emerging greenhouse signal" which is not natural. [Michael MacCracken (Reviewer's comment ID #: 152-149)]	Agreed, edited
TS-839	A	37:20		greenhouse gases are not a natural external forcing.	Agreed, edited

				[David Rind (Reviewer's comment ID #: 214-10)]	
TS-840	A	37:25	37:25	Replace "greenhouse gas forcing" by "human urban and agricultural development" [VINCENT GRAY (Reviewer's comment ID #: 88-2007)]	Rejected, see earlier responses to same comment by this reviewer
TS-841	A	37:26	37:26	Insert after "years" ,"as measured by the surface record" [VINCENT GRAY (Reviewer's comment ID #: 88-2008)]	Rejected, believe text is clear
TS-842	A	37:29		In TS5 on projections of future climate change, the discussion of stabilisation rates, emission budgets and associated atmospheric concentrations of GHGs is weakly presented and provides no coherent storyline that would be of assistance to policy makers. The authors should include a section concerning how both the rates of emission and the mix of gases will effect climate change and the extent of possible changes, as this information is readily available in the literature. [Govt. of Australia (Reviewer's comment ID #: 2001-111)]	Reject. The type of material requested is in the scope of the WG3 report.
TS-843	A	37:29		On this page, several terms like transiate climate response (footnote 8) and AOGCM are explained, however, the scenarios selected for the projections are not. I find it necessary to mention these scenarios (B1, A1B, A2 and A1F1), e.g. in a footnote, for all those who will not read the underlying report, and for whom these scenarios it would remain difficult to understand several parts of the text, and several figures. [Govt. of Hungary (Reviewer's comment ID #: 2012-20)]	Accepted. A box explaining the SRES scenarios is now in the SPM which will be alongside this Technical Summary.
TS-844	A	38:0	38:	Footnote. When can I see "Glossary"? The last one contained several dubious definitions. [VINCENT GRAY (Reviewer's comment ID #: 88-2014)]	The glossary was made available to reviewers with the second draft.
TS-845	A	38:1	38:5	Although we have a much better picture of for climate sensitivity than in the TAR, including information about the distribution and a most likely value, the uncertainty has hardly changed (likely 2-4.5 in AR4, likely 1.5-4.5 in TAR). Also, the statement that in previous reports it was not possible to estimate the probability for sensitivity being outside 1.5-4.5 is incorrect. The TAR TS states that sensitivity is 'likely' 1.5-4.5. I still wait for someone to explain to me where the 'likely' came from in the TAR, but it's there. But strictly speaking there was a statement of likelihood in the TAR. [Reto Knutti (Reviewer's comment ID #: 133-31)]	Our understanding is that no probabilistic statement was intended in the TAR – even though the reviewer is correct that the “likely” word was used in one place. This is countered by several places in the TAR where the text carefully avoids making a probabilistic or “likelihood” statement – e.g. in the SPM.
TS-846	A	38:2	38:2	Insert after "estimated", "by sheer guesswork" [VINCENT GRAY (Reviewer's comment ID #: 88-2010)]	Rejected no basis given.
TS-847	A	38:5	38:5	Insert after "as well as," "some light on" [VINCENT GRAY (Reviewer's comment ID #: 88-2011)]	Unnecessary
TS-848	A	38:11	38:11	There are not three but only two SRES stabilization experiments with constant forcing after 2100, the third one is constant forcing after 2000. [Reto Knutti (Reviewer's comment ID #: 133-32)]	Agreed but we do not refer to “SRES stabilization experiments” only to “stabilization experiments” – no change
TS-849	A	38:11	38:11	Should a reference for SRES scenarios be cited? [Melinda Marquis (Reviewer's comment ID #: 162-122)]	See TS-843.
TS-850	A	38:15	38:15	Add at end "This is just as well, as several scenarios are hopelessly improbable"	Rejected, no basis given

				[VINCENT GRAY (Reviewer's comment ID #: 88-2012)]	
TS-851	A	38:17	38:17	AOGCM projections are made for THREE centuries, not two. [Reto Knutti (Reviewer's comment ID #: 133-33)]	Accepted – text changed.
TS-852	A	38:22	38:23	As it stands, it reads like some of these models refers to EMICS only (as EMICS are the subject of the previous sentence). This is wrong half of the C4MIP models are AOGCMs. "Some of the AOGCMs and EMICS contain prognostic carbon cycle components..." would be accurate [Pierre Friedlingstein (Reviewer's comment ID #: 77-37)]	Accepted – text changed
TS-853	A	38:22		Box T.S.5.1 One sentence describing the coupled climate-carbon cycle model should be added at the end, as they should be viewed as a further step in the hierarchy of models. [Pierre Friedlingstein (Reviewer's comment ID #: 77-38)]	Rejected as unnecessary. The role of climate - carbon models is covered in the text and the box has a limited tutorial role.
TS-854	A	38:23	38:23	Add at end "Such long-term projections should not be considered too seriously" [VINCENT GRAY (Reviewer's comment ID #: 88-2013)]	Rejected no basis given.
TS-855	A	38:41	:44	Obtaining quantitative results from EMICs or simpler models is a risky proposition since these models do not solve the full conservation equations. The ability to mimic the GCM results does not imply that the answers are being obtained for the right reason, nor that answers obtained with changing scenarios or parameters will be similar to what GCMs would produce. To the extent that conclusions are drawn from their quantitative, as opposed to qualitative, results, the level of confidence should be reduced accordingly. (Note that in chapter 8, the first requirement on p. 91 to have confidence in models is that they solve the full conservation equations.) [Govt. of United States of America (Reviewer's comment ID #: 2023-956)]	Rejected – Ch 8 states the necessary limitations on the use of EMICs and their use in the TS is consistent with that. The wording is changed here to make that clearer though.
TS-856	A	39:5	39:7	The word “commitment” has a very important policy connotation and its introduction in the TS may create some confusion. It is suggested to replace it with some suitable or appropriate phrase. It is felt that the definition contained in the box TS 5.2 is not very clear. This should accordingly be simplified to bring out the real issue at hand. Similarly, the reference to “commitment experiment” in line 47 of SPM-11 is not very clear, it accordingly should be simplified and modified. [Govt. of India (Reviewer's comment ID #: 2013-9)]	Accepted the word “commitment” will not be used in the TS.
TS-857	A	39:7	38:7	Replace "confirm" by "indicate" [VINCENT GRAY (Reviewer's comment ID #: 88-2015)]	Text changed
TS-858	A	39:9	39:9	Replace "would be expected to" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2016)]	Rejected no basis given.
TS-859	A	39:12	39:18	Delete this whole paragraph which is contrary to facts. It fails to realise that the surface record was primarily influenced by socioeconomic factors, and that the stratospheric records, by showing no warming over most of their record, are inconsistent with a greenhouse gas explanation. [VINCENT GRAY (Reviewer's comment ID #: 88-2017)]	Rejected. Comment is factually incorrect.
TS-860	A	39:14	39:16	Why are the FAR and SAR projected temperature increases cited with two significant	Accepted for the FAR result – now cited

				figures (0.29 deg C and 0.15 dec C, respectively) but the AR4 value cited with only one significant figure (0.2 deg C)? It seems that all three values, because the reader is asked to compare them to each other, should have the same number of significant figures, no? [Melinda Marquis (Reviewer's comment ID #: 162-121)]	as in their SPM and sources defined more carefully in footnote. Significant figures are now consistent
TS-861	A	39:15	39:15	This discussion would be a bit more informative if it indicated that the 0.29 C estimate is based on GHG only, without aerosols, and the 0.15 C value is based on the amount of sulfate continuing as it is not (though this is felt to be ecologically unacceptable)--so their biases with respect to the 0.2 estimate in this assessment are quite logical, and this bouncing around should not be thought of as the physics bouncing around, but the understanding improving as we hone in on the most likely results. [Michael MacCracken (Reviewer's comment ID #: 152-150)]	Accepted – text added
TS-862	A	39:21		TS-29: why do not all curves start from 0 in 1990? [Govt. of Hungary (Reviewer's comment ID #: 2012-21)]	Observed data deliberately normalised to match data shown in fig TS-7 – No change.
TS-863	A	39:21		In FIGURE TS-29, what is the source of the "observed" warming? There are several slightly different estimates to choose from. [Adrian Simmons (Reviewer's comment ID #: 242-10)]	Caption will be expanded.
TS-864	A	39:24	39:24	I do not think that the word "committed" is very helpful. I would change the word to "continuing" and maybe make the phrase "a continuing warming due to ongoing adjustment to past emissions" or something similar--and that is more understandable. [Michael MacCracken (Reviewer's comment ID #: 152-151)]	Accepted the word “commitment” will not be used in the TS.
TS-865	A	39:28	39:29	0.4/1.3 = 0.3 so you might want to change "about a quarter" to "about a quarter to a third." [Melinda Marquis (Reviewer's comment ID #: 162-123)]	Reject – fairer comparison is 0.4/ 1.5
TS-866	A	39:30	39:30	Add at end "Since all of these projections do not agree with actual measured climate indicators, allowing for urban and other effects in the surface record, they can be disregarded". [VINCENT GRAY (Reviewer's comment ID #: 88-2018)]	Rejected as factually incorrect
TS-867	A	39:38	39:38	I think the phrase "after commitment" here is very confusing and would urge the wording be changed. In this case, it seems that this could as easily be phrased "after stabilization" [Michael MacCracken (Reviewer's comment ID #: 152-152)]	Accepted – was a typo
TS-868	A	39:39	39:39	Add at end. "Since none of these projections sorresponds to current changes in the climate they have little credence" [VINCENT GRAY (Reviewer's comment ID #: 88-2019)]	Rejected as factually incorrect. Observed temperature trends match near term projections.
TS-869	A	39:43	40:19	The terminology used in Box. TS 5.2 is inconsistent with the terminology in 10.7. In 10.7 climate change commitment is used as the overarching term for constant composition commitment, constant emission commitment, and zero emission commitment. Using different definitions will only confuse the reader and I see no reason to introduce a new terminology in the TS and not to stick with the definitions used in 10.7 [Reto Knutti (Reviewer's comment ID #: 133-34)]	Revised language in the TS and CH10 will make both consistent..
TS-870	A	39:43	40:19	This whole definition of 'commitment' is not really well founded, at least use the twenty-	Definitions have been made clear so we

				<p>year old definition of 'unrealized warming' instead of inventing a new term. Also this is not the true commitment (per dictionary definition) as it does NOT include the commitment of current power plants, etc. Please see notes on SPM, this is a poorly formed idea, and does not help, or indeed misleads, the governments in understanding our commitment to climate change.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-957)]</p>	do not see why this would be misleading. 'Unrealized warming' seems to have failed to be understood broadly so we feel better language is useful.
TS-871	A	39:43	41:26	<p>Since no evidence has been presented that greenhouse gases have a measurable influence on the climate this whole section is irrelevant. Delete it</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2020)]</p>	Rejected – no basis for the reviewer's view is given
TS-872	A	39:43		<p>Is "commitment" the best word to use. Ask a well-informed lay person what a "climate change commitment" is and he or she would most likely talk about the commitments of (many) governments to cut carbon dioxide emissions to the levels of a few years ago.</p> <p>[Adrian Simmons (Reviewer's comment ID #: 242-11)]</p>	Accepted the word "commitment" will not be used in the TS.
TS-873	A	39:45	39:55	<p>This paragraph lacks logical consistency with known scientific facts. Changing atmospheric constituents change radiation in two ways: 1) there is a change change in the net radiation loss of the troposphere and 2) there is a change in the back radiation to the surface. The first change impacts on radiation cooling of the troposphere and the rate of convective overturning; the second change impacts on the surface temperature through adjustment to the surface heat balance (unlike solar radiation which penetrates the ocean surface layer, back radiation is absorbed at the earth's surface - both land and ocean. Through convective overturning the atmosphere quickly adjusts both its mean temperature (regulated by the temperature of the buoyant updrafts) and its rate of overturning. ENSO clearly demonstrates the linkage between SST and tropospheric temperature and the rapid adjustment of the atmosphere to changing SST. The proposition that increased back radiation is absorbed by and warms the ocean over long periods cannot be sustained. Changes in the rate of ocean overturning will however affect the climate over a long period. Variations in the rate of entrainment of cold deeper water through the thermocline will cause variations in tropical SST, ocean-atmosphere sensible and latent heat exchange, convective overturning of the troposphere, and the rate of poleward transport of heat. If the poleward transport exceeds the radiation loss to space over polar regions the impact will be surface warming and ice melt at the poles; if the poleward transport is insufficient to balance radiation loss to space over polar regions the impact will be cooling and snow accumulation at the poles. It may take a long time to restore the global top of the atmosphere radiation balance that was disrupted by a change in the meridional overturning circulation of the ocean, especially as the MOC has a natural period of the order of 1000 years. The concept of climate change commitment has not been substantiated; it is very likely a false concept and should be excised from the report.</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-63)]</p>	Rejected. The reviewer's view is not supported by the literature. The concept of global energy balance is well established in the literature. Changes in that balance give warming or cooling. Adjustment of the mixed layer is much faster than that of the deep ocean. Observations presented in ch 5 show clearly that the ocean has been absorbing a substantial amount of heat, down to 700 m and more, over the past decades. See e.g. ch 5.
TS-874	A	39:46	39:46	<p>"...the climate system, after the forcing is stabilised."</p> <p>[David Parker (Reviewer's comment ID #: 195-95)]</p>	text changed for other reasons

TS-875	A	39:46		Box T.S.5.2 I think that "radiative forcing were to be stabilized" would be less ambiguous here than just "forcing...". Otherwise forcing could be understood as external forcing, i.e. emissions ! [Pierre Friedlingstein (Reviewer's comment ID #: 77-41)]	Accepted
TS-876	A	39:48		Some atmospheric adjustments to changes in boundary conditions take substantially longer than a month. Stratospheric adjustments to changed emissions from the surface can take several years. [Adrian Simmons (Reviewer's comment ID #: 242-12)]	Accepted
TS-877	A	39:50	39:50	The statement that the deep ocean has response time scales longer than 1000 years is not true particularly in the North Atlantic and should be modified to indicate regional differences in these response times. [Govt. of United States of America (Reviewer's comment ID #: 2023-958)]	Text edited
TS-878	A	40:2	40:2	Change "aspect" to "way to consider". [David Parker (Reviewer's comment ID #: 195-96)]	Accepted
TS-879	A	40:5	40:8	This is a bit of a run-on sentence. Consider changing the last phrase to a new sentence: "This slow decrease in concentrations and forcing implies a long-term commitment to climate change." [WG1 TSU (Reviewer's comment ID #: 285-14)]	Text edited
TS-880	A	40:21	40:31	The proposition that commitment to sea level change occurs over a much longer time period than applies to sea surface temperature is not supported by recent evidence. Satellite analysis of MSL identifies a 25 mm rise in global mean sea level between 1994 and 1998 as global mean SST rose 0.5C. The subsequent fall in MSL was in concert with the fall in SST. Any future MSL rise from thermal expansion will be in concert with ocean warming, whether at the surface or within the deep ocean. The theory and modelling of delayed sea level rise needs a thorough review. [William Kininmonth (Reviewer's comment ID #: 128-64)]	Agreed that satellite data provide evidence for short term responses but this does not mean there are not also long term responses. The heat content data in Ch05 and model results consistent with them in Ch10 support the text here – no change.
TS-881	A	40:21	40:31	This discussion is confusing, particularly with respect to expected sea level rise over different periods of time and different scenarios, and thermal expansion v. total sea level rise. There is no mention of the accelerated expansion in 2080-2100 discussed in 10.6.1. The discussion in 10.6.5 does seem to be relevant but is not really represented here. Furthermore, given that the observations [TS-27] show that 1.8mm / yr (1961-2003) or 3.1mm / yr (1993-2003) are what is being observed now the model results seem rather low. [Meric Srokosz (Reviewer's comment ID #: 250-4)]	Accepted – text changed and will better reflect Ch10
TS-882	A	40:21		A clear statement on future sea level rise is needed, that takes into account all new recent data from both models and observations. Crucial observations that complement model results include a rise of sea level more rapid than projected by models, the loss of mass of the Antarctic ice sheet, and the more rapid loss of ice at the fringes of Greenland and Antarctica. The SPM should also highlight Chapter 10's finding that current models underestimate observed sea-level rise from 1961 to 2005 by 40%. [European Commission (Reviewer's comment ID #: 2008-6)]	Sea level material has been clarified along many of the lines suggested. In fact, these model results match observations rather well. See ch 9 for more detail regarding comparison of observed and modelled SLR.

TS-883	A	40:21		Please insert a clear statement on future sea level rise including its uncertainty range which takes the full information into account. This means beside of model results the new observations on ice sheet decay and paleoclimatic information also. Otherwise the new evidence of sea level is rising faster than any scenario shown in the TAR, the Antarctic ice sheet appears to be losing mass overall according to the GRACE satellite data (this was not anticipated in the TAR), and the ice loss at the fringes of Greenland and Antarctica is more rapid than expected will be not considered. These findings clearly point to greater sea level rise than projected in the TAR. It is the task of IPCC to give a comprehensive assessment of all these findings and state this clearly in the SPM, the TS as well as in Chapter 10. [Govt. of Germany (Reviewer's comment ID #: 2011-90)]	Ice sheet data has been considered and this is now indicated.
TS-884	A	40:21		The TS needs a clear statement on future sea level rise, specifying an uncertainty range which takes the full information into account (not just models - also the new observations on ice sheet decay, and paleoclimatic information), and does not just discuss single scenarios like A1B. In my view evidence since the TAR clearly points to the risk of higher sea level rise than given in the TAR - sea level is currently (1993-2005 satellite altimeter data shown in Chapter 5) rising faster than any scenario shown in the TAR, the Antarctic ice sheet appears to be losing mass overall according to the GRACE satellite data (this was not anticipated in the TAR), and the ice loss at the fringes of Greenland and Antarctica is more rapid than expected. Models do not capture these things, and chapter 10 finds that current models underestimate the observed sea level rise 1961-2005 by 40% - that is a major finding which is well hidden so far in the SOD. The data clearly point to greater sea level rise than projected in the TAR, and this needs to be brought out clearly in the SPM, the TS as well as in Chapter 10! [Stefan Rahmstorf (Reviewer's comment ID #: 206-41)]	See TS-882 and 883. Recent increases in SLR have been linked to variability, not an accelerating trend. See ch 5.
TS-885	A	40:27		Must include/discuss the projection of rapid short term ice sheet changes. [Govt. of United States of America (Reviewer's comment ID #: 2023-959)]	See TS-883
TS-886	A	40:28	40:31	These results seem much too low. While the last decade rate may be a fluctuation, but if the present rate of rise continues, sea level will be up by 0.15 m by 2050 without any further acceleration--so higher than estimated here. And with further warming, there is no basis for assuming the rate of rise will decrease. This estimate somehow seems to assume that all that we do not understand (perhaps due to changes on land or maybe poor knowledge about Antarctica or whatever) will lead to no further changes in sea level. In my view, this estimate of rise is totally unacceptable--no way has our knowledge increased since the TAR so that the rate drops this much; instead, the further warming makes even greater rise much more likely. The low numbers for 2100 are also way too low--the present rate of rise might be viewed as "the commitment" even if emissions stopped, and this would lead to a 0.3 m rise by then, even with no further warming. And with the warming extending further and further across Greenland, the rate of rise, at least the potential for a large change, is much, much higher. [Michael MacCracken (Reviewer's comment ID #: 152-153)]	See TS-884, 883, and 882.

TS-887	A	40:29	40:30	Can you please clarify whether the 0.30 - 0.80 m (from thermal expansion) includes -- or is in addition to -- the 0.14 - 0.43 m sea level rise predicted for the year 2100 by the A1B scenario. [Melinda Marquis (Reviewer's comment ID #: 162-124)]	Text has been edited
TS-888	A	40:29		See my comment to Chapter 10 to the range 0.14-0.43m [Gerrit Burgers (Reviewer's comment ID #: 34-19)]	Text has been edited
TS-889	A	40:30	40:30	Consider rewording, e.g., "... at the A1B concentrations, *such that the sea level rise would reach* 0.30 - 0.80 m by 2300." [WG1 TSU (Reviewer's comment ID #: 285-15)]	Unnecessary
TS-890	A	40:34	40:36	COMMENT: This phrase gives the misunderstanding that "3°C" are the threshold of Greenland Ice Sheet melting. Additional example and/or information are required. REASON: Chapter 10.7.4.3 description is "For a global warming of 3°C relative to present, models suggest Greenland would contribute 0.2-3.9mm/yr to sea level." Keep consistency with this Chapter. RECOMMEND: replace the phrase "would contribute to sea level rise by up to 0.4m per century." with "would contribute to sea level rise at a range of rate 0.02-0.39m per century". [Govt. of Japan (Reviewer's comment ID #: 2014-26)]	Text has been edited
TS-891	A	40:36	40:36	Change "was" to "were." (Subjunctive mood.) [Melinda Marquis (Reviewer's comment ID #: 162-125)]	Accepted
TS-892	A	40:39	40:43	This sentence is inadequate and misleading. The atmosphere is warmer near sea level and the temperature decrease with height. Also, the altitude of the atmospheric freezing level is critical because above this altitude ice will not melt and ice sheets are very stable because of the significantly increased energy needed to ablate ice to water vapour over that needed for melting ice. The Greenland and Antarctic ice sheets are currently very stable because even in summer their plateau are many degrees below freezing point. However, an increase in the atmospheric circulation and a lifting of the altitude of the mean summer freezing level to near the elevation of the respective plateaux would lead to extensive surface melting and instability of the ice sheets. The idea of irreversible melting of the Greenland ice sheet rather fanciful; a return to the current climate would result in regrowth of glaciers on the higher mountains acting as nuclei for the general expansion of the ice mass. Lines 39-43 are speculative and should be removed. [William Kininmonth (Reviewer's comment ID #: 128-65)]	Rejected: The sentences are not speculative at all but are based on a few model simulations. The additional rise of 7m upon a meltdown of the GrIS is a robust estimate of the total mass of the GrIS.
TS-893	A	40:39		Please be specific, not 'several' meters, but 4 to 6 ? [Govt. of United States of America (Reviewer's comment ID #: 2023-960)]	Accepted
TS-894	A	40:41	40:43	See above comment (#140) regarding irreversibility of the loss of the Greenland Ice Sheet. [Adrian Simmons (Reviewer's comment ID #: 242-141)]	Cannot identify comment being referred to by reviewer.
TS-895	A	40:43	40:4	Change "was" to "were." (Subjunctive mood.) [Melinda Marquis (Reviewer's comment ID #: 162-126)]	Accepted
TS-896	A	40:47	40:47	Is there a reason for switching to giving a rate of rise per century from the rate of rise being in units of m/yr? One set of units should be used throughout--and I would actually, I think,	Following practice in the literature. Readers should be able to multiply to

				prefer m/century. [Michael MacCracken (Reviewer’s comment ID #: 152-154)]	get their preferred units, and everyone has a different favorite
TS-897	A	40:49	40:52	On what basis should the estimates be leaving off what could potentially happen from changes in ice flow--at the very least the uncertainties caused by this need to be included in the estimates. In the TAR, this problem, after strenuous objections from prominent US glaciologists, was treated in a figure caption, as I recall--a barely satisfactory outcome. This time, giving estimates of rise and basically completely leaving off this very important process seems to me unacceptable. As a minor note, indicating that the models do not yet deal adequately with the fate of surface meltwater might also be indicated. [Michael MacCracken (Reviewer’s comment ID #: 152-155)]	Ice flow has been considered, see text.
TS-898	A	41:18	41:19	In that the preindustrial rate of rise of sea level was so small makes this statement a bit misleading--substantially higher than roughly zero is not really making this sound as important as is the case. [Michael MacCracken (Reviewer’s comment ID #: 152-156)]	Reject – preindustrial is an appropriate baseline
TS-899	A	41:30	42:21	This section is pure speculation, without any observational evidence. Delete [VINCENT GRAY (Reviewer’s comment ID #: 88-2021)]	Rejected – chapters 8 and 9 cover empirical evidence relating to climate sensitivity
TS-900	A	41:32	41:39	Should we add "Equilibrium" before climate sensitivity - check with Fons' definitions? [Piers Forster (Reviewer’s comment ID #: 73-26)]	Accepted
TS-901	A	41:32	41:39	COMMENT: suggest describe the definition of “equilibrium climate sensitivity”. REASON: ECS have been calculated using atmosphere models coupled with non-dynamic “slab” ocean, i.e., models omitting all ocean dynamics. The numbers of ECS are higher than actual temperature change. Explanation on ECS is necessary for easy understanding. RECOMMEND: add the following sentence, “It should be noted, however, that the equilibrium climate sensitivity which assumes 50m-depth ocean will give higher temperature rise than what will actually happen.” [Govt. of Japan (Reviewer’s comment ID #: 2014-27)]	Reject. Definition in glossary.
TS-902	A	41:32		The word "expert" might be deleted. All assessments included in the Report are supposed to be expert. [Adrian Simmons (Reviewer’s comment ID #: 242-13)]	Accept
TS-903	A	41:37		assumes climate sensitivity does not change with climate. [David Rind (Reviewer’s comment ID #: 214-11)]	Text has been edited
TS-904	A	41:37		This, of course, assumes that climate sensitivity does not change with climate change, is this possibly important, does it need a caveat? [Govt. of United States of America (Reviewer’s comment ID #: 2023-961)]	Text has been edited
TS-905	A	41:48	41:48	I would suggest changing "models' ability" to "the abilities of models"--one should not talk about all models having the same ability. [Michael MacCracken (Reviewer’s comment ID #: 152-157)]	Accept
TS-906	A	41:49	41:49	For clarity, consider changing "climate sensitivity" to "the climate sensitivity determined by	Reject – the focus has to be on the true

				the model." [Melinda Marquis (Reviewer's comment ID #: 162-127)]	climate sensitivity
TS-907	A	41:51	41:54	This statement underscores the uncertainty of the claim, based on model simulation, that 20th century warming is due to anthropogenic forcing. Clearly the 20th century record can be reconstructed by a range of forcing/sensitivity combinations. [William Kininmonth (Reviewer's comment ID #: 128-66)]	Agreed but some combinations are more likely than others – hence the probabilistic assessment – no change
TS-908	A	41:51	41:51	Suggest 'wide' range of possible net forcing instead of 'extreme'. The range is not really extreme, but problematic because it extends to or below zero. [Reto Knutti (Reviewer's comment ID #: 133-35)]	Accepted Text edited
TS-909	A	42:5	42:7	This statement recognises that model sensitivity to greenhouse forcing is related to the uncertainty of how low and middle level clouds should be specified. The positive feedback that amplifies the direct forcing may, in fact, be quite spurious. [William Kininmonth (Reviewer's comment ID #: 128-67)]	Rejected – no basis given for assuming the feedback might be spurious
TS-910	A	42:10		ignores nonlinear response in which sea ice changes affect water vapor and cloud cover response [David Rind (Reviewer's comment ID #: 214-12)]	No space for this level of detail.
TS-911	A	42:14	42:21	COMMENTS: suggest describe the “transient climate response: TCR” REASON: TCR is evaluating in the more actual condition than ECS. Therefore, TCR is useful information about more real estimation of future temperature changes. RECOMMEND: add this sentence with “TCR can better simulate what will actually happen than ECS.” between "The range of ... climate sensitivity." and "The transient climate response...", and add this sentence with "The median for TCR is 1.8°C." after "...be 1.2-2.4°C." [Govt. of Japan (Reviewer's comment ID #: 2014-28)]	Accepted – dealt with by reorganization of the text
TS-912	A	42:15	42:16	Chapter 9, P. 9.58 lines 53-54 mention an observationally constrained estimate of 1.5-2.8°C [Govt. of Finland (Reviewer's comment ID #: 2009-2)]	Text edited
TS-913	A	42:16	42:16	There are several problems in that paragraph: 1) I don't understand where the numbers for a 90% TCR range come from. The only evidence we have are the GCMs, and those cannot be used to estimate a 90% range, since the GCM ensemble is not designed to span any range of uncertainty (otherwise we could use the GCMs also to use a 90% range for sensitivity, and we don't). 2) I'm not aware of 'models of different complexity' that have produced a probabilistic range for TCR. The only study I know of is Stott, mentioned in section 9.6.2.3, where the text says that TCR is unlikely larger than 2.8. 3) The 'unlikely larger than 2.8' given in 9.6.2.3 is strongly inconsistent with the 'very unlikely larger than 2.4' given here. 4) More fundamentally, since there is a relationship between TCR and sensitivity, giving a 95% upper bound on TCR is inconsistent with the fact that we say we can't provide a very likely upper bound on sensitivity. Assuming we know the relationship of the two, any range of one can be translated into a range of the other. Therefore no 90% range for TCR should be given. The only thing that can be safely quoted is the range covered by the GCMs, which	Text edited.

				can be compared to the TAR range. [Reto Knutti (Reviewer's comment ID #: 133-36)]	
TS-914	A	42:18	42:19	It is not clear in this sentence whether the heat uptake is solar heat as a consequence of changing ocean mixed layer dynamics (eg., ENSO or longer period modes) or whether it is longwave heat uptake through increased back radiation. If the latter then the response time is likely to be short as the back radiation is absorbed at the surface and immediately adjusts the surface energy balance. Very little of the additional back radiation will be mixed into the surface layer through surface mixing. [William Kininmonth (Reviewer's comment ID #: 128-68)]	We think present text is clear that the heat uptake is what happens in the time frame of the transient – no change
TS-915	A	42:23	42:23	Projections as far ahead as 100 years are irresponsible, particularly as no attempt is made to check whether they comply with emerging climate observations. [VINCENT GRAY (Reviewer's comment ID #: 88-2022)]	Rejected – assumptions for all projections are made clear and are consistent with current obs
TS-916	A	42:23	44:57	I cannot find any descriptions regarding climate stabilizations in the paragraph of TS5.3 “LARGE SCALE PROJECTION OVER 21ST CENTURY”. Therefore, I strongly recommend adding enough descriptions of new findings regarding climate stabilizations after the paragraph of TS5.3. [Koki Maruyama (Reviewer's comment ID #: 169-2)]	Stabilization experiments are covered elsewhere. The TS has been reorganized though to make this material easier to find.
TS-917	A	42:25	42:29	Uncertainty range should be given as +/- two standard deviations (95% range) following conventional scientific practice. No justification is presented here or in the underlying text for departing from the conventional approach, and since the standard deviation is not indicated, there is no way for the reader to adjust these ranges to the more conventional approach. 65% probability has no general applicability. [Lenny Bernstein (Reviewer's comment ID #: 20-43)]	Accepted: ranges will be adjusted throughout to 5 – 95%
TS-918	A	42:25	42:29	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. [Jeff Kueter (Reviewer's comment ID #: 137-42)]	See TS-917
TS-919	A	42:25	42:29	I am not comfortable with these ranges expressed as probabilities since they are not the result of a formal probability analysis concatenating uncertainty. They are rather the result of a sensitivity study as explained in Ch10.5.3.1 and reiterated at the end of the section in the paragraph Ch10 p48 lines 4-10. The '(mean+/-1 standard deviation)' which appears in the Ch10 summary (page 3 line 44) refers only to uncertainty due to the 19 model tunings and this should no-doubt be clarified. I would be more comfortable with the words 'Projected likely range' but since 'likely' is formally defined as '66% probability' this may not help in the defence. Note that the ranges are based on a single estimate of the forcing magnitude as in the TAR. [Sarah Raper (Reviewer's comment ID #: 208-1)]	Text edited
TS-920	A	42:27	42:27	Add at end "Scerarios A2 and A1F1 are so ridiculous that they should be rejected out of hand. We are then left with around 2°C rise for the next century.Most of us could live with that"	Rejected – reviewer gives no basis for his view

				[VINCENT GRAY (Reviewer's comment ID #: 88-2023)]	
TS-921	A	42:38		is there to be a discussion of regions where the warming is not well-constrained by models (e.g., tropics)? [David Rind (Reviewer's comment ID #: 214-13)]	Rejected: This part deals with the largest-scale variables. Regions to be discussed below
TS-922	A	42:38		Is there to be a discussion of regions where the warming is not well-constrained by models (e.g., tropics)? [Govt. of United States of America (Reviewer's comment ID #: 2023-962)]	See TS-921
TS-923	A	42:39	42:39	Insert after "Atlantic" "Since this is in contrast to what is actually happening it shows that the projections are false" [VINCENT GRAY (Reviewer's comment ID #: 88-2024)]	Rejected – comment is factually incorrect
TS-924	A	42:42	42:45	There is a logical inconsistency that needs explanation. If the MOC is buoyancy driven as implied then the high latitude oceans will quickly become stratified and vertical mixing will cease. How does warming reach the interior against the increasing stratification of the oceans? [William Kininmonth (Reviewer's comment ID #: 128-69)]	Rejected: MOC is not the only process providing vertical exchange. Seasonal mixing, Ekman pumping, and Eddy activity are still operative.
TS-925	A	42:52		The definition of “heat wave” (e.g., degrees above normal) is needed. [Govt. of United States of America (Reviewer's comment ID #: 2023-963)]	Rejected: Definition provided in caption of Figure 10.3.17
TS-926	A	42:56	:57	“except where surface properties change” – Is this related to terrain height above sea level, oceans, and associated qualities? [Govt. of United States of America (Reviewer's comment ID #: 2023-964)]	Explanation added
TS-927	A	42:57	43:4	This section is confusing ass written. I think the point is that extreme temperature events, defined as outside of the 95% of present-day cases (?) are likely to occur 90% of the time in the tropics, and 40% elsewhere. Please clarify. [Franklin SCHWING (Reviewer's comment ID #: 230-25)]	Accepted: Will be formulated more clearly.
TS-928	A	43:2	43:4	The 40% value is very low compared with those reported in Ch. 11, where Table 11.2 indicates a large majority of the individual DJF and JJA seasons in late 21st century to be warmer than the single warmest DJF and JJA seasons in 1980-1999. Please consult Ch. 11 authors for more information. [Govt. of Finland (Reviewer's comment ID #: 2009-3)]	Value indicated is from a multi-model study and reflects the chapters..
TS-929	A	43:2	43:4	The meaning of this sentence should be clarified [Govt. of France (Reviewer's comment ID #: 2010-127)]	Accepted – text changed
TS-930	A	43:2	43:2	Change "the cases" to "observed cases". [David Parker (Reviewer's comment ID #: 195-97)]	Text edited – it should be simulated for the 20th century, not observed.
TS-931	A	43:2	:4	Should define what is meant by “extreme warm seasons” as it’s a little confusing that the probability in the tropics increases by 90% compared to 40% elsewhere when places “elsewhere” are projected to warm more. Should clarify that “extreme warm season” refers to surpassing some threshold average temperature, if that’s in fact how it’s defined. [Govt. of United States of America (Reviewer's comment ID #: 2023-965)]	Accepted – text changed
TS-932	A	43:7	43:7	Add at end "There is no observational evidence to support any of these projections"	Rejected – see Chapter 8 and much of

				[VINCENT GRAY (Reviewer’s comment ID #: 88-2025)]	the rest of the report
TS-933	A	43:9	43:20	Changing patterns of streamflow shown in Milly et al (2005) are worthy of mention here. [P.C.D. Milly (Reviewer’s comment ID #: 179-30)]	Not in chapter so cannot be here.
TS-934	A	43:12	43:14	The last part of the sentence, "particularly outside the tropics", is not correct, as tropical Amazon is one of the important areas where extreme precipitation would increases more than the mean, while in most high-latitude areas the percentage changes in extreme and mean precipitation would not be very different, according to Emori and Brown (2005). See my comment #15. [Seita Emori (Reviewer’s comment ID #: 62-29)]	Rejected: "particularly outside the tropics" refers to the areas which exhibit consistent response across the multi-model ensemble. Amazon is not among these.
TS-935	A	43:26	43:26	Add at end "Again, there is no observational evidence to support these projections" [VINCENT GRAY (Reviewer’s comment ID #: 88-2026)]	Rejected – see Chapter 8 and much of the rest of the report
TS-936	A	43:28	43:45	No definition of abrupt given nor is it made clear that there may be an abrupt slowdown (rather than shutdown) and there is some observational evidence that this might be occurring 5.3.2, Box 5.1. Figure 10.3.13 - some models show quite abrupt changes (slowdown) but the overplotting of the curves makes this difficult to see. The statements made are somewhat misleading. Re-word - No model suggests a complete shutdown of the MOC during the 21st century, but an abrupt slowdown cannot be ruled out. In addition, the wording "very unlikely" should be changed to "unlikely" as many uncertainties remain in the models' ability to represent the MOC correctly [8.3.2.2, Fig. 10.3.13]. Note too that two papers at the Avoiding Dangerous Climate Change conference (Challenor et al., Schlesinger et al. - see Schellenhuber et al., 2006 Avoiding Dangerous Climate Change, Cambridge University Press) suggest the probability of abrupt slowdown / shutdown is higher than originally thought. [Meric Srokosz (Reviewer’s comment ID #: 250-5)]	Rejected. Text reflects AR4 models. See chapter 5 regarding lack of observational evidence currently. The overplotting has not obscured anything and the results have been carefully analyzed.
TS-937	A	43:28	:45	The significance of an MOC slowdown in models should be given (i.e., does a 60% change impact simulated atmospheric climate). Since Bryden et al. claim a 30-50% slowdown in the MOC, is there any observational evidence for observed atmospheric changes. The emphasis on the MOC throughout the ocean section needs such a “reality check”. [Govt. of United States of America (Reviewer’s comment ID #: 2023-966)]	See TS-936 regarding observational claims.
TS-938	A	43:31		Observed freshwater input values: from melting sea ice 0.014 Sv (Lindsay and Zhang 2005), from Greenland 0.007 Sv (Rignot and Karangaratnam 2006), and from Eurasian rivers 0.005 Sv (Peterson et al. 2002). Even without precipitation over the oceans and Canadian river runoff, this is together about a quarter of what is widely considered a rough critical magnitude (0.1 Sv) for an MOC shutdown. [European Commission (Reviewer’s comment ID #: 2008-8)]	Reject. 0.1Sv is not a threshold. It is unclear if the observed melt rates are variability.
TS-939	A	43:31		Currently observed freshwater input values together about a quarter of what is widely considered a rough critical magnitude (0.1 Sv) for a MOC shutdown from melting sea ice. The inputs amount 0.014 Sv (Lindsay and Zhang 2005), from Greenland 0.007 Sv (Rignot and Karangaratnam 2006), and from Eurasian rivers 0.005 Sv (Peterson et al. 2002). In order to include the most important freshening terms in the text amend it this way: "due to	See response above TS-938

				the combined effects of an increase of high latitude temperatures and high latitude precipitation, river runoff and meltwater input, which..." [Govt. of Germany (Reviewer's comment ID #: 2011-91)]	
TS-940	A	43:31		Amend: "due to the combined effects of an increase of high latitude temperatures and high latitude precipitation, river runoff and meltwater input, which..." Otherwise you are forgetting the most important freshening terms. Currently observed freshwater input values (which chapter 10 unfortunately fails to discuss): from melting sea ice 0.014 Sv (Lindsay and Zhang 2005), from Greenland 0.007 Sv (Rignot and Karangaratnam 2006), and from Eurasian rivers 0.005 Sv (Peterson et al. 2002) - even without precipitation over the oceans and Canadian river runoff, this is together about a quarter of what is widely considered a rough critical magnitude (0.1 Sv) for an MOC shutdown. [Stefan Rahmstorf (Reviewer's comment ID #: 206-37)]	See response above TS-938
TS-941	A	43:32	43:40	The statement that no models suggest an abrupt MOC shutdown in the 21st century appears twice in this paragraph. The entire paragraph can be edited for clarification. [Franklin SCHWING (Reviewer's comment ID #: 230-26)]	Accepted – text changed
TS-942	A	43:34	43:40	Please be specific here. Of the models used in this IPCC report (such as the 11/13 shown in Fig. TS-26), how many explicitly included melting of Greenland in their MOC response? And did they get the melting rate correct? Otherwise these statements are confusing at least and perhaps even incorrect. [Terrence Joyce (Reviewer's comment ID #: 122-8)]	Partly accepted: sentence noting absence of Greenland melting will be added, but it will be noted that models including this show similar results
TS-943	A	43:36	43:40	For proper English, change "the MOC will reduce" to "The magnitude of the MOC will diminish" or something similar. In addition to saying that the change in the MOC is not likely to be rapid, it might be interesting to say whether changes in atmospheric circulation could cause temperature changes in particular locations (even if up on the Greenland Ice Sheet) to change abruptly. [Michael MacCracken (Reviewer's comment ID #: 152-158)]	First part accepted. There is no information to answer the question posed in the second part of the comment.
TS-944	A	43:37	43:40	Sentence repeated '...abrupt shutdown during the 21st century' [Reto Knutti (Reviewer's comment ID #: 133-37)]	Text edited
TS-945	A	43:37		Change to "unlikely" in light of the previous comment. [European Commission (Reviewer's comment ID #: 2008-9)]	Rejected – while the text has been clarified, the authors assessment based on all the available model simulations is that a large abrupt transition of the MOC during the 21st century is very unlikely
TS-946	A	43:37		From our point of view the expert judgement "very unlikely that the MOC will undergo an abrupt collapse" is not justified as it suggest more confidence and less uncertainty than is currently evident. Please change to "unlikely". [Govt. of Germany (Reviewer's comment ID #: 2011-92)]	See TS-945
TS-947	A	43:37		"very unlikely that the MOC will undergo an abrupt collapse" - change to "unlikely", otherwise you suggest more confidence and less uncertainty than we currently have on this. [Stefan Rahmstorf (Reviewer's comment ID #: 206-36)]	See TS-945

TS-948	A	43:40	43:40	Delete the sentence "No models..." as it repeats line 32. [David Parker (Reviewer's comment ID #: 195-98)]	Text edited
TS-949	A	43:40		It is noted that the sentence "No models suggest an abrupt MOC shutdown during the 21st Century." is already included in line 32. [Govt. of Austria (Reviewer's comment ID #: 2002-39)]	Text edited
TS-950	A	43:47	43:51	As clearer phrasing for the start of the first sentence, I would suggest "Changes in the intensities of extratropical storms are expected in the future, ..." In the second sentence, should this not be saying that a larger fraction of the storms will become intense (or more intense). In the third sentence, should this be referring to the winter or cold season. [Michael MacCracken (Reviewer's comment ID #: 152-159)]	Text edited
TS-951	A	43:51	43:51	Add at end "Once again, there is no observational evidence to support these projections" [VINCENT GRAY (Reviewer's comment ID #: 88-2027)]	Rejected – see Chapter 8
TS-952	A	43:53	43:54	COMMENT: Replace the phrase of "do not suggest significant increases" with "suggest a global decrease" REASON: keep consistency with Chapter 10.3.6.3. [Govt. of Japan (Reviewer's comment ID #: 2014-29)]	Accepted
TS-953	A	43:53	43:53	Models are tending to predict that there will be fewer tropical cyclones globally in the future. [Ruth McDonald (Reviewer's comment ID #: 173-34)]	Accepted
TS-954	A	43:53	44:2	The results presented in chapter 10, sub-section 10.3.6.3 (pages 33-35) give a rather more complicated image of model projections. In particular, it appears that the confidence level on the maximum wind speed change is lower than on the precipitation change (see my comments n°1-2 and 4). The fact to choose only one result is not convincing and the corresponding model has likely its own limitation (ocean-atmosphere coupling ? scale interaction between large scale and mesoscale ?; impossible for the reader to know it since there is no reference). [Govt. of France (Reviewer's comment ID #: 2010-128)]	Text has been edited
TS-955	A	43:53	44:2	The results presented in chapter 10, sub-section 10.3.6.3 (pages 33-35) give a rather more complicated image of model projections. In particular, it appears that the confidence level on the maximum wind speed change is much lower than on the precipitation change. The fact to choose only one result is not convincing and the corresponding model has likely its own limitation (ocean-atmosphere coupling ? scale interaction between large scale and mesoscale ?; impossible for the reader to know it since there is no reference). [Serge PLANTON (Reviewer's comment ID #: 199-7)]	See TS-954
TS-956	A	43:55	43:57	For clarity, I would suggest saying at the start "One simulation with a very high resolution model and able ...", then change "strongest" to "very strong" and on line 57 change "though" to "while" [Michael MacCracken (Reviewer's comment ID #: 152-160)]	Text changed – emphasis kept on overall assessment rather than single studies
TS-957	A	44:1	44:2	The problem so far is that the simulations with the models have generally been for quite	Rejected – Ch10 covers simulations for

				limited situations, such as for a uniform increase in temperature, etc. The real challenge is that doing the full problem is really tough--and so one might say these model simulations have been quite schematic or simplified. [Michael MacCracken (Reviewer's comment ID #: 152-161)]	much more detailed temperature changes and resolutions down to 20 km
TS-958	A	44:2		It is proposed to delete theoretical because this specific model does not seem to differ in its nature from other models addressed in the TS. [Govt. of Austria (Reviewer's comment ID #: 2002-40)]	Accepted
TS-959	A	44:10	44:16	It would also be correct (and useful, I think) to say that it is consistent with modeling evidence to date that ENSO is robust in the context of climate changes of the magnitude projected for the 21st century, and that changes in its amplitude and frequency will be relatively modest, with no GCMs with realistic ENSO variability predicting a transition to a state in which the system gets stuck in an extreme El Nino or La Nina state. [Isaac Held (Reviewer's comment ID #: 105-62)]	Text modified.
TS-960	A	44:10	:16	The modeled increase in ENSO response patterns is not consistent with increased hurricane intensity as one feature associated with ENSO is increased wind shear over the Atlantic, which tends to decrease hurricane intensity. This apparent inconsistency needs to be addressed. [Govt. of United States of America (Reviewer's comment ID #: 2023-967)]	Not an inconsistency. Model results are self-consistent so to the extent this occurs, it will have been part of the evaluation. Text edited.
TS-961	A	44:20	44:20	Is this statement that winter changes have been moderate correct--there was significant diminishment during the 2005-6 winter. [Michael MacCracken (Reviewer's comment ID #: 152-162)]	The statement is about projected winter sea ice – no change.
TS-962	A	44:21	44:22	I would change "where" to "by" and delete "in the climate system accelerate the melting of sea ice" as duplicative. [Michael MacCracken (Reviewer's comment ID #: 152-163)]	Accepted
TS-963	A	44:22	44:25	This statement is inconsistent with TS page 43, lines 28-45 that suggest a reduction of MOC with anthropogenic warming. A reduction of the MOC requires reduced poleward transport of mass and heat at the surface and the opposite of what is claimed in the sentence. [William Kininmonth (Reviewer's comment ID #: 128-70)]	Rejected. The high latitude heat transport increases in the 21st climate in the Atlantic. Text ok. See chapter 10.3.
TS-964	A	44:25	44:26	This sentence seems a bit confusing. I would change "reduce" to "will be reduced" and evolves" to "evolve"--but I am not at all sure what is meant by saying it will evolve on the same time scale as global warming (a very obscure phrasing). It also needs to be said that some models project total summer loss much earlier, and I think a Naval Postgraduate School analysis suggested it might only be a few decades, so this wording is quite inadequate. [Michael MacCracken (Reviewer's comment ID #: 152-164)]	Noted – but no change, text consistent with chapter and with models available for this assessment.
TS-965	A	44:27	44:27	Need to change "Sea ice is also projected to reduce" to "Sea ice cover is also projected to decrease" [Michael MacCracken (Reviewer's comment ID #: 152-165)]	Text edited
TS-966	A	44:28	44:28	Add at end. "Again there is no observational evidence to support these projections, and the scenarios A2 and A1F1 are so ridiculous that they, at least can be disregarded"	rejected – see Chapter 8. No basis given for disregarding A2 or A1F1

				[VINCENT GRAY (Reviewer's comment ID #: 88-2028)]	
TS-967	A	44:32	44:32	Replace "As" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-2029)]	Rejected – present language justified by statements on committed warming
TS-968	A	44:32	44:34	The explanation is inadequate. With global warming glaciers and ice caps lose mass because of a rise in the altitude of the freezing level that both allows for increased melting and, at least over lower elevations, ensures an increase in the ratio of rainfall to snowfall. [William Kininmonth (Reviewer's comment ID #: 128-71)]	Rejected. Ice mass balance involves potential changes in melting and precipitation as stated
TS-969	A	44:35	44:35	"The statement that thawing of the upper layer of permafrost is projected to be as much as 90% does not make sense (doesn't use terminology that permafrost scientists would use), is misleading and based on results from models with some rather important limitations (see comments on Ch. 10). A more correct statement (which is a better interpretation of the results of the cited studies) would be that increases in thaw depth are projected to occur in response to warming over the next century. " [Govt. of Canada (Reviewer's comment ID #: 2004-138)]	ACCEPTED: wording regarding permafrost changed here and in chapter
TS-970	A	44:35	44:35	Add at end "which is so ridiculous it can be ignored" [VINCENT GRAY (Reviewer's comment ID #: 88-2030)]	Rejected – no basis given for this view
TS-971	A	44:35	44:35	The statement that thawing of the upper layer (how defined?) of permafrost is projected to be as much as 90% does not make sense (does not use terminology that permafrost scientists would use) and is not defined, is misleading and based on results from models with some rather important limitations (see comments on Ch. 10 - comment #58 - 69). A more correct statement (which is a better interpretation of the results of the cited studies) would be that increases in thaw depth are projected to occur in response to warming over the next century. [Sharon Smith (Reviewer's comment ID #: 244-85)]	Statement deleted.
TS-972	A	44:39	44:41	It should also be said that if the melt water can runoff, then it only takes the heat of fusion and not the heat of fusion plus vaporization (i.e., heat of sublimation) for an ice sheet to lose mass--and since the heat of fusion is much less than the heat of vaporization, much more melting can occur. [Michael MacCracken (Reviewer's comment ID #: 152-166)]	Noted – but we don't have space for this level of tutorial here; has been taken into account
TS-973	A	44:41	44:41	Add section reference to end of paragraph: [10.3]. [Melinda Marquis (Reviewer's comment ID #: 162-131)]	Accepted
TS-974	A	44:43	44:43	Add at beginning "If the projections are to be believed" [VINCENT GRAY (Reviewer's comment ID #: 88-2031)]	rejected no basis given for this view.
TS-975	A	44:43	44:43	For consistency with previous paragraphs, change "Antarctic *will* gain mass" to "Antarctic *is projected to* gain mass." [Melinda Marquis (Reviewer's comment ID #: 162-132)]	Accepted
TS-976	A	44:43	44:48	Insert at the beginning of the sentence: "Although Antarctica appears to have lost mass during the last decade, during the next century, Antarctica will gain mass..." [Govt. of United States of America (Reviewer's comment ID #: 2023-968)]	Rejected – see other comments – the statement has to be linked to model projections and comparison with recent

					observations can not be oversimplified as suggested
TS-977	A	44:43	44:48	The report needs the TS to reconcile the statement that Antarctica will gain mass in the next century, with the discussion on page 24 which says that Antarctica lost mass over the last decade. This paragraph appears to be an attempt to explain why the recent observations don't imply that [Govt. of United States of America (Reviewer's comment ID #: 2023-969)]	Accepted. Dynamical response will be mentioned..
TS-978	A	44:43	44:48	the contribution from Antarctica could be either positive or negative in the next century. But it is so vague—and the average reader does not know what you mean by recent dynamical imbalance. [Govt. of United States of America (Reviewer's comment ID #: 2023-970)]	Taken into account. Text has been edited
TS-979	A	44:43	44:48	Many scientists who do not follow these matters as closely as the authors of chapters 4 and 10—not to mention the public—are under the impression that the recent positive contribution from Antarctica would imply that the future contribution may be positive—or at least that we simply do not know whether the positive or negative factors are strongest. In writing this passage, the authors need to recognize this presumption that most readers have. To say that, in spite of the recent positive contribution, the future contribution will be negative—rather than uncertain—requires a persuasive explanation that simply is not in this summary right now. In an ideal world, such an analysis might be in Chapters 4 and 10—but this is really an issue that cuts across both chapters and hence it may be up to the TS authors to work this out. [Govt. of United States of America (Reviewer's comment ID #: 2023-971)]	Taken into account.
TS-980	A	44:43		This appears to contradict an earlier bullet saying that Antarctica is losing mass. Please explain the mismatch between model results and observations. [European Commission (Reviewer's comment ID #: 2008-7)]	Text edited
TS-981	A	44:43		"Antarctica will gain mass..." - another bullet says that it is already losing mass. This is a contradiction. Also the paleoclimatic evidence points to smaller Antarctic ice sheet in warmer climates. All we can say is that models which do not adequately simulate ice sheet dynamics suggest it might gain mass, but this is highly unlikely in view of the observational data which contradict those models. [Stefan Rahmstorf (Reviewer's comment ID #: 206-42)]	Text edited
TS-982	A	44:48	44:48	Add section reference to end of paragraph: [10.6]. [Melinda Marquis (Reviewer's comment ID #: 162-133)]	Accepted
TS-983	A	44:50	44:52	Would it be possible to also give numbers for other SRES scenarios? [Govt. of Finland (Reviewer's comment ID #: 2009-4)]	Accepted: Ch10 will have a new figure with the ranges of the 6 marker scenarios. They will be carried fwd to the TS and SPM.
TS-984	A	44:50	44:52	This highlight repeats what is on page 40, lines 27-31. As indicated in my comment there, the estimates there are much too low, with the rate from now til 2050 having to decrease by 25% or so for this to be the case--and there is virtually no chance of this to occur as the	Addressed.

				warming continues. [Michael MacCracken (Reviewer’s comment ID #: 152-167)]	
TS-985	A	44:50	44:52	I'm somewhat uneasy with the discussion here. Sea Level Rise (SLR) is likely to be one of the most severe potential climate change impacts on society, yet there is no attempt here to provide a truly comprehensive assessment of the potential changes and their true uncertainties. The discussion here centers on one particular scenario and a certain set of model simulations. Projected rates are provided based on these estimates, yet it seems quite plausible that these are significant underestimates. The physics of basal lubrication and the importance of ice dynamics (as highlighted by Rignot and Kanagaratnam, 2006) are poorly understood and not fully accounted for in current climate model simulations. Until we can be relatively confident that these effects are well represented, there is a substantial probability that we may be underestimating the dynamic nature of the ice sheets, and the potential for significant increases in ice streaming and calving. It would seem quite possible that the models significantly underestimate the potential acceleration of ablation for both major ice sheets. This issue needs to be handled with far more circumspection than at present, for it has potentially greater societal ramifications than any other issue dealt with in AR4. [Michael Mann (Reviewer’s comment ID #: 156-58)]	Taken into account.
TS-986	A	44:50	44:52	Please explain why IPCC expects sea level rise to decelerate. This passage seems to suggest a rate of 2.5 mm/yr in the next 50 years and 2.9 mm/yr in the next century. But on page 27 the TS says that sea level is currently rising 3.1 mm/yr. Much of this IPCC report provides reasons for why we might expect the sea to rise more rapidly in the future—including text that suggests that there has been some recent acceleration. Simply providing a projection without analysis is not enough—especially when the projection shows the opposite of what one would otherwise expect. [Govt. of United States of America (Reviewer’s comment ID #: 2023-972)]	Taken into account.
TS-987	A	44:50		The TS needs a clear statement on future sea level rise, specifying an uncertainty range which takes the full information into account (not just models - also the new observations on ice sheet decay, and paleoclimatic information), and does not just discuss single scenarios like A1B. In my view evidence since the TAR clearly points to the risk of higher sea level rise than given in the TAR - sea level is currently (1993-2005 satellite altimeter data shown in Chapter 5) rising faster than any scenario shown in the TAR, the Antarctic ice sheet appears to be losing mass overall according to the GRACE satellite data (this was not anticipated in the TAR), and the ice loss at the fringes of Greenland and Antarctica is more rapid than expected. Models do not capture these things, and chapter 10 finds that current models underestimate the observed sea level rise 1961-2005 by 40% - that is a major finding which is well hidden so far in the SOD. The data clearly point to greater sea level rise than projected in the TAR, and this needs to be brought out clearly in the SPM, the TS as well as in Chapter 10! [Stefan Rahmstorf (Reviewer’s comment ID #: 206-43)]	Taken into account.
TS-988	A	44:51	44:52	Earlier in the TS (page 40, lines 27-31), the projected sea level rise (under A1B scenario) by	Noted – consistency in style will be

				the year 2100 is cited as a range (0.14 - 0.43 m). Here (TS, page 44, lines 51-52), the projected sea level rise is cited as the median value of the range cited on page 40. Of course, these are not inconsistent, but just different. Is this OK? [Melinda Marquis (Reviewer's comment ID #: 162-135)]	improved
TS-989	A	44:52	44:52	Add section reference to end of paragraph: [10.6]. [Melinda Marquis (Reviewer's comment ID #: 162-134)]	Accepted
TS-990	A	45:4	45:14	Some clarification would be welcome whether or not coupling between climate change and the carbon cycle have been taken into account in the model results highlighted in the TS before this chapter. [Govt. of Austria (Reviewer's comment ID #: 2002-41)]	Accepted – this will be clarified
TS-991	A	45:6	45:7	Amend sentence to "These simulations all exhibit higher atmospheric CO2 increases and stronger climate change than their uncoupled counterparts." [David Parker (Reviewer's comment ID #: 195-99)]	Text revised in different ways
TS-992	A	45:9	45:9	Insert after "scenario" (which is too absurd to be credible)" [VINCENT GRAY (Reviewer's comment ID #: 88-2032)]	rejected – no basis given for this view
TS-993	A	45:12	45:13	"Alternatively it reduces the total emissions consistent with a given CO2 stabilization level, ..." Though technically this phrase is correct, I think it is confusing. I suggest re-wording it, e.g., "This positive feedback means that a lower level of GHG emissions will lead to greater warming than is predicted by models that don't account for the carbon cycle and the positive feedback caused by increased warming." [Melinda Marquis (Reviewer's comment ID #: 162-136)]	Suggested text is not what is intended – text edited for clarity
TS-994	A	45:12		Replace "higher SRES" by SRES A2. As it stand it suggest higher (than A2) SRES scenario, which is not true. [Pierre Friedlingstein (Reviewer's comment ID #: 77-39)]	Accepted
TS-995	A	45:16	45:16	Insert after "concentrations" "is calculated to" [VINCENT GRAY (Reviewer's comment ID #: 88-2033)]	Unnecessary
TS-996	A	45:16	45:16	Add at end "but this is yet to be confirmed by observations" [VINCENT GRAY (Reviewer's comment ID #: 88-2034)]	Rejected – factually incorrect
TS-997	A	45:19	45:19	Replace "will" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2035)]	Rejected – factually incorrect – this is basic chemistry
TS-998	A	45:27	45:27	Replace "variations" by "fall" [VINCENT GRAY (Reviewer's comment ID #: 88-2036)]	Rejected – the implications of the fall in CH ₄ growth rate are dealt with elsewhere. This sentence points to implications of the high variability.
TS-999	A	45:27	45:29	Whereas it is stated that there are large uncertainties in future projections for methane, the summary appears to be quite categorical in identifying rice agriculture and wetlands are future potential sources of methane emissions. This is despite the fact that rice-paddy has been extensively studied especially in the rice-producing countries and the methane emission estimates from this source do not show any potential of alarming increase in the	Rejected. Rice agriculture is one of several contributors, stated to be poorly estimated at present.

				future. [Govt. of India (Reviewer's comment ID #: 2013-10)]	
TS-1000	A	45:33	45:33	Delete "other". [David Parker (Reviewer's comment ID #: 195-100)]	Accepted
TS-1001	A	45:36	45:36	After "troposphere." add "There will also be converse feedback processes whereby the deposition of reactive gases such as ozone and reactive nitrogen affect the biosphere and hence the carbon cycle." [William Collins (Reviewer's comment ID #: 45-38)]	Insufficient basis for such a statement in the chapter – such secondary feedback effects are believed to be small.
TS-1002	A	45:36	45:36	For clarity, consider adding "concentration" after "ozone," and adding "the concentration of products of ozone's reaction with" after "hence": "... alter stratospheric ozone concentration and hence the concentration of products of ozone's reaction with solar ultraviolet in the troposphere." [Melinda Marquis (Reviewer's comment ID #: 162-137)]	First part accepted. Second part would be incorrect since it is transmission that is referred to, not local concentrations..
TS-1003	A	45:40	45:40	Change "Estimate" to "Estimates". [David Parker (Reviewer's comment ID #: 195-101)]	Accepted
TS-1004	A	45:48	45:48	Change "however," to "but". [David Parker (Reviewer's comment ID #: 195-102)]	Text changed
TS-1005	A	45:53	45:53	Replace "both" with its antecedent. [Melinda Marquis (Reviewer's comment ID #: 162-138)]	Copy-edit.
TS-1006	A	45:54	45:55	The effects of black carbon on climate is still very uncertain. It should not put the effects of black carbon and greenhouse gases at the same level of current scientific understanding. There is no sufficient evidence to conclude "... warming ... dominates over the sulphate cooling." Suggest to delete this sentence or delete "black carbon" in this sentence. [Govt. of China (Reviewer's comment ID #: 2006-21)]	Text edited substantially and concern addressed.
TS-1007	A	46:5	46:12	More emphasis should be given to the following sentence "Thus, environmental strategies aimed at stabilization or climate change commitment below a prescribed threshold would require consideration not only of greenhouse gas emissions, particularly of CO2, but also of measures that may be implemented to improve air quality." This is highly policy relevant and therefore should be emphasised in the TS and in the SPM. [Philippe Tulkens (Reviewer's comment ID #: 271-7)]	Apart from indicating the connection little more can be said here. No change.
TS-1008	A	46:7	46:10	The statement that "The hypothetical removal from the atmosphere of the entire current burden of anthropogenic aerosol particles would produce a rapid increase of about 0.8C within a decade or two in the globally averaged temperature." is of concern. This is actually somewhat misleading as the aerosols would actually reduce to near zero if their production ceased. It is not necessary to "remove" them, as they have a short life time. Further, this is probably one of the most important points in the chapter in that it makes very clear that we are already committed to a large and rapid climate change over coming decades as any attempt to reduce the burning of fossil fuels will lead to reduced greenhouse forcing but inevitably lead to a large over-compensatory aerosol forcing. [Govt. of Australia (Reviewer's comment ID #: 2001-112)]	Agreed that this is potentially an important statement – but there is insufficient material in chapters to elaborate and the effect of specific decreases in coal burning are in the scope of WG3 rather than WG1.

TS-1009	A	46:8	44:8	On this phrase of "rapid increase"--while this is a pulse change, will the response really be so rapid to the full value? Does it not take time to get the ocean warmer. [Michael MacCracken (Reviewer's comment ID #: 152-168)]	Text does not say that 0.8C is the full rise. Rest is correct - see Figure 7.6.1.
TS-1010	A	46:9	46:9	The text "Changes in aerosols would also be expected to influence precipitation" may be modified to "Changes in aerosols are likely to influence precipitation". [Govt. of India (Reviewer's comment ID #: 2013-11)]	Agreed
TS-1011	A	46:9	46:12	This line may be deleted as it attempts to prescribe policy action and modified as per the suggestion given in the next comment [Govt. of India (Reviewer's comment ID #: 2013-12)]	Text edited to remove any sense of being prescriptive.
TS-1012	A	46:14	46:17	Climate change induced air quality degradation is a highly uncertain issue and it does not warrant an exclusive paragraph in the summary. It is suggested that these lines be deleted. Alternatively, a scientific statement bringing out our current understanding of the linkages between various factors including physical processes relating air quality degradation and climate change may be added as a replacement for the lines 9-14 and 14-17. [Govt. of India (Reviewer's comment ID #: 2013-13)]	. Text edited along the lines suggested.
TS-1013	A	46:20	46:20	I think it important that the report needs to define what "regional" means--for IPCC this generally means continental scale--and yet this chapter really does get to finer scales--so subcontinental, etc. And it also needs to say that in general, everywhere will experience changes, and an initial estimate is the latitudinal average, and then the departure from this on the subcontinental scale. We need to get away from the notion that indications that there are uncertainties in regional projections does not mean that there will be no change--only that the local departure from the subcontinental estimates is not yet clear. [Michael MacCracken (Reviewer's comment ID #: 152-169)]	Rejected. "Regional" here is to be interpreted as explained in Chapter 11 on regional climate change and the usage in this report is consistent with the TAR.
TS-1014	A	46:26	46:26	Replace "robust" by "tentative" [VINCENT GRAY (Reviewer's comment ID #: 88-2037)]	rejected – no basis given for this view
TS-1015	A	46:33	46:33	Replace "would substantially exceed" with "is expected to exceed substantially the." [Melinda Marquis (Reviewer's comment ID #: 162-139)]	Text edited
TS-1016	A	46:41	46:42	This statement as written could be used provocatively by skeptics as an illogical flaw in the report. Most people will not comprehend how ALL regions will warm at a rate greater than the global mean. Perhaps a clarifying statement is needed. [Franklin SCHWING (Reviewer's comment ID #: 230-28)]	Taken into account. Comment appears to refer to previous paragraph
TS-1017	A	46:47	46:47	I would suggest changing "blocking" to 'atmospheric blocking" as blocking alone might not be a familiar term to all readers. [Michael MacCracken (Reviewer's comment ID #: 152-170)]	Accepted
TS-1018	A	46:47		Rather than single out ENSO and the NAO, perhaps say something more general like modes of climate variability such as ENSO and the NAO (that have regionally heterogeneous patterns). [Franklin SCHWING (Reviewer's comment ID #: 230-27)]	Rejected. NAO and ENSO are given as examples in present text.
TS-1019	A	46:54	46:54	Replace "expected" by "projected" [VINCENT GRAY (Reviewer's comment ID #: 88-2038)]	Accepted – text changed

TS-1020	A	46:54	46:54	Replace 'expected' by 'projected on the basis of modeling'. "Expected' is a subjective term and not quantified in the introduction. [William Kininmonth (Reviewer's comment ID #: 128-72)]	See TS-1019
TS-1021	A	46:56	47:30	Regional results for runoff from Milly et al. (2005) would be relevant here. Runoff (nearly equivalent to atmospheric water flux divergence) is distinct from precipitation. [P.C.D. Milly (Reviewer's comment ID #: 179-31)]	Insufficient material in chapter
TS-1022	A	47:0	53:	The section on Robust Findings and Key Uncertainties is particularly helpful. More use could also be made of this discussion by placing it in a prominent position at the start of the TS or in the SPM. It would also be of assistance at the end of each dot point in the TS to cite where those specific findings are contained in the body of the report. In addition each point could be included in the chapter to which it relates to further increase the utility of these points. [Govt. of Australia (Reviewer's comment ID #: 2001-113)]	We suspect the reviewer(s) found this section useful because it was all in one place. As a separate section it is also signalled in the table of Contents. On balance we feel it better to keep the existing structure.
TS-1023	A	47:8	47:13	It would be helpful here (and in the SPM) to be saying something about the major monsoons so much of the region depends upon. [Michael MacCracken (Reviewer's comment ID #: 152-171)]	Text edited where information is available.
TS-1024	A	47:12	47:12	Why is the word "return" before the word "frequency"? [Melinda Marquis (Reviewer's comment ID #: 162-140)]	Standard term
TS-1025	A	47:16	47:17	It would help here to mention that it is not just the northern mountains of North America being affected, but the mountains of western North America--including their southern reaches. Mention should also be made in this point to the intensification of rain events. [Michael MacCracken (Reviewer's comment ID #: 152-172)]	Text edited
TS-1026	A	47:19	47:22	This is a bit confusing as elsewhere it is said that the tropics generally warm less than the global average--presumably because a greater share of the trapped energy is going to evaporation. The phrasing here almost makes it seem as if everywhere is being said to warm more than the global average, which is of course not possible. I imagine this is because the land areas warm more than the oceans, but it might help overall to be a bit more careful in one's explanations--saying an area warms more than other land areas, or something. [Michael MacCracken (Reviewer's comment ID #: 152-173)]	Accepted – text clarified
TS-1027	A	47:29	47:30	I would suggest phrasing it as "an "increase in annual precipitation in the Arctic (does this also not happen in the Antarctic--indeed, greater snowfall on Antarctica is a critical assumption in keeping sea level rise so low--too low in my view. This item should also melting back of glaciers, permafrost, and sea ice, etc.--and I would say the Greenland Ice Sheet. [Michael MacCracken (Reviewer's comment ID #: 152-174)]	Text has been edited. The confidence in Antarctic projected increases of precipitation are not as confident. Thus, while consideration of physical processes might suggest this will be the case, the evidence to support a robust statement in this regard is not yet available.
TS-1028	A	47:32	47:33	First, I think this item needs to differentiate between what can happen to low-lying islands and mountainous islands--so low-lying ones are most exposed to rising sea level, beach erosion, etc.; mountainous islands are likely to have an intensification of rainfall as the most	Text edited for clarity

				important problem. I also do not understand this phrase "enhanced sea level rise"--for low lying islands, any sea level rise is going to be problematic; in areas with enhanced sea level rise (but is this a fluctuation or permanent), impacts will just come a little bit sooner, but all will face serious problem by end of century. [Michael MacCracken (Reviewer's comment ID #: 152-175)]	
TS-1029	A	47:35	47:35	Replace "greater confidence in" by "better" [VINCENT GRAY (Reviewer's comment ID #: 88-2039)]	Would not make sense
TS-1030	A	47:35	47:45	Do you want to add increased precipitation in the tropics (increased tropical ppt maxima), as is cited earlier in TS, page 43, lines 9-10? [Melinda Marquis (Reviewer's comment ID #: 162-141)]	Text edited for consistency with earlier para
TS-1031	A	47:35	47:45	Regional results for runoff from Milly et al. (2005) would be relevant here. Runoff (nearly equivalent to atmospheric water flux divergence) is distinct from precipitation. [P.C.D. Milly (Reviewer's comment ID #: 179-32)]	Insufficient material in chapter
TS-1032	A	47:41	47:41	Add "many of" before "those regions" [Govt. of Finland (Reviewer's comment ID #: 2009-5)]	Text has been edited
TS-1033	A	47:41	47:41	Replace 'drier' with 'more arid'. The increase in annual rainfall implies a wetter rain season but an increase in evapotranspiration due to warmer temperatures may lead to a prolonged period of soil moisture deficit. [William Kininmonth (Reviewer's comment ID #: 128-73)]	Text has been edited
TS-1034	A	47:43	47:43	Add "high latitudes" after "convergence zones" [Govt. of Finland (Reviewer's comment ID #: 2009-6)]	Text has been edited
TS-1035	A	47:50	47:50	Replace "Robust" with "Tentative" [VINCENT GRAY (Reviewer's comment ID #: 88-2040)]	Rejected – no basis given for this view
TS-1036	A	47:50	53:56	Among the "Robust findings", there are ones with qualifiers like "it is very likely" etc., but also ones without such qualifiers (e.g. "the amount of ice ... is decreasing"). Does this always mean that the ones without qualifiers are statements with 100% confidence? Suggest to check. [Govt. of Hungary (Reviewer's comment ID #: 2012-22)]	Some statements summarize observations without needing a likelihood qualifier – but all cases will be checked.
TS-1037	A	47:50	53:56	I found the use of the IPCC lexicon in this section very inconsistent--it only appearing in a few of the points, and in many cases seeming quite forced (e.g., in the first point on page 47, line 55, it is really absurd to include the phrase "It is virtually certain that"--as it is certain a warming influence is created; it might make sense if the lexicon phrase were moved to next to 650,000 years--which I assume is the issue, but generally it can be dropped (especially considering a number of other later phrases that would need to have such a phrase added if one were to use the lexicon in every finding). So, I generally recommend dropping the lexicon phrase where possible (and I will suggest a number of those places). [Michael MacCracken (Reviewer's comment ID #: 152-176)]	Noted. It is accepted that some statements do not need a likelihood qualifier.
TS-1038	A	47:50		The section is not homogeneously written: some paragraphs use the standard terms of uncertainty and others not. I think that they should be written in same way.	See TS-1036, TS-1037 – terminology and confidence levels will be made

				[Aristita Busuioc (Reviewer's comment ID #: 35-13)]	consistent with the chapters as far as possible
TS-1039	A	47:50		Robust findings and key uncertainty section is very good [Piers Forster (Reviewer's comment ID #: 73-27)]	Thank you.
TS-1040	A	47:50		Section TS.6: possibly include references back to source sections within TS. [David Parker (Reviewer's comment ID #: 195-108)]	Thank you this will be done
TS-1041	A	47:55	47:56	"virtually certain" means likelihood >99%. It is wrong to use it here. It could be "very likely" or "likely" because the present levels of LLGHGS is compared within the last 650,000 years here. Please compare this sentence with line 17-20 of TS-6. [Govt. of China (Reviewer's comment ID #: 2006-22)]	Text edited
TS-1042	A	47:55	47:55	Replace "virtually certain" by "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-2041)]	Text edited
TS-1043	A	47:55	47:55	Drop "It is virtually certain that"--or move its position next to aspect of phrase that is not certain. [Michael MacCracken (Reviewer's comment ID #: 152-177)]	Accepted
TS-1044	A	47:55	47:56	"positive radiative forcing (warming effect)" could be replaced simply by "warming effect". [Adrian Simmons (Reviewer's comment ID #: 242-14)]	Rejected – radiative forcing a central concept in this report
TS-1045	A	48:2	48:3	Same problem as above. "virtually certain" is not right. Suggest to use "very likely" or "likely". [Govt. of China (Reviewer's comment ID #: 2006-23)]	Text edited
TS-1046	A	48:2	48:2	Replave "virtually certain" by "probable" [VINCENT GRAY (Reviewer's comment ID #: 88-2042)]	Text edited
TS-1047	A	48:2	48:2	Drop "It is virtually certain"--not needed, especially given that in items on lines 5-6, 11-12, 14-15, etc. do not have any qualifying phrase. [Michael MacCracken (Reviewer's comment ID #: 152-178)]	Accepted.
TS-1048	A	48:2	48:3	Is this supposed to say human activities...over the last 250 years? Not, say, 150 years? [Franklin SCHWING (Reviewer's comment ID #: 230-29)]	Yes sentence reads as intended
TS-1049	A	48:6	48:6	Replace "is" by "may be" [VINCENT GRAY (Reviewer's comment ID #: 88-2043)]	Rejected – see chapter 6 for exhaustive explanation of this
TS-1050	A	48:8	48:8	Drop "It is very likely that"--this has to have happened if the CO2 concentration went up. If there is to be a qualifier, and I don't think it is needed, apply it to the numerical value, so say very likely 42% plus or minus 7%. [Michael MacCracken (Reviewer's comment ID #: 152-179)]	Text edited with new emphasis.
TS-1051	A	48:8	48:8	Do you want to add "since pre-industrial times" after "atmosphere" or to add "anthropogenic" before "CO2"? [Melinda Marquis (Reviewer's comment ID #: 162-142)]	Text edited with new emphasis.
TS-1052	A	48:8	48:8	The 42% is difficult to reconcile with Table TS-1, except by dividing the ocean-atmosphere flux by the difference between the land-atmosphere flux and the emissions before. [David Parker (Reviewer's comment ID #: 195-103)]	Text edited with new emphasis.

TS-1053	A	48:11	48:12	I was surprised there was not some lexicon qualifier here--likely or very likely. [Michael MacCracken (Reviewer's comment ID #: 152-180)]	Likelihood added
TS-1054	A	48:17	34:18	add a quantitative description of the exerted warming influence on climate or add a preposition like "clear" warming influence [Govt. of Germany (Reviewer's comment ID #: 2011-93)]	Accepted – text edited
TS-1055	A	48:17	48:18	Delete "very likely"--is there really any question of this compared to other statements without qualification? Even "virtually certain" seems too weak to me. [Michael MacCracken (Reviewer's comment ID #: 152-181)]	Rejected – the likelihood statements is important here.
TS-1056	A	48:30	48:31	As phrased, this statement applies quite widely--we have not directly measured the CO2 effect over this timescale either--we have indirectly figured this out. This needs to be revised to something like "Multi-century reconstructions of changes in solar irradiance remain inadequate to pin down the Sun's effect on radiative forcing" [Michael MacCracken (Reviewer's comment ID #: 152-182)]	Text edited
TS-1057	A	48:35		The lack of mention of surface, tropospheric, and stratospheric temperature trends is surprising, especially given the focus on this topic earlier in the TS and in Ch. 3. [Dian Seidel (Reviewer's comment ID #: 231-2)]	Rejected. Believe text reflects chapter.
TS-1058	A	48:35		The lack of mention of surface, tropospheric, and stratospheric temperature trends is surprising, especially given the focus on this topic earlier in the TS and in Ch. 3. Given the major new works on upper tropospheric trends, one would expect this to be a major, robust finding. [Govt. of United States of America (Reviewer's comment ID #: 2023-973)]	See TS-1057
TS-1059	A	48:38	48:38	Replace 'temperatures' with 'surface temperatures' [William Kininmonth (Reviewer's comment ID #: 128-74)]	Accepted
TS-1060	A	48:41	48:41	Insert ' after several decades with little trend' after 'the mid-1970s'. [William Kininmonth (Reviewer's comment ID #: 128-75)]	Unnecessary
TS-1061	A	48:42	48:42	For clarity, change "rate of the ocean" to "rate of warming of the ocean" [Michael MacCracken (Reviewer's comment ID #: 152-183)]	Text edited
TS-1062	A	48:46	48:47	Is the decrease in precipitation in the tropics since the 1970s consistent with the projected increase in tropical precipitation maxima (TS, page 43, lines 9-10)? [Melinda Marquis (Reviewer's comment ID #: 162-143)]	Issue is too detailed for RFKU section – see chapter text.
TS-1063	A	48:52	48:53	Evidence also suggests that at least one of the satellite reconstructions is unreliable, since they disagree by ~0.1 K in trend. The statement should be broadened to say that all records of upper-air temperatures have serious uncertainties related to homogeneity problems. [Melissa Free (Reviewer's comment ID #: 76-13)]	Partly accepted Text edited.
TS-1064	A	48:56	48:56	Insert 'following the inclusion of satellite observations' following 'after 1979'. [William Kininmonth (Reviewer's comment ID #: 128-76)]	Suggestion would not be entirely accurate
TS-1065	A	48:56		"after 1979" should read "from 1979 onwards" or "after 1978". The observing system was improved late in 1978, in preparation for the FGGE year of 1979. [Adrian Simmons (Reviewer's comment ID #: 242-15)]	Unnecessary

TS-1066	A	49:2	49:2	Insert after "disagree" : "surface measurements find no evidence of warming before 1997, suggesting that the surface record is influenced by urban factors. They also disagree" [VINCENT GRAY (Reviewer's comment ID #: 88-2057)]	Rejected – factually incorrect
TS-1067	A	49:2	49:2	Change to read "disagree on changes in total and low-level cloud extent over the ocean" [Michael MacCracken (Reviewer's comment ID #: 152-184)]	Rejected. Chapter 3 discusses issues of comparability between surface and satellite measurements and uses the broader language as in the TS.
TS-1068	A	49:3	49:2	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2058)]	Rejected – no basis given
TS-1069	A	49:7	49:7	Replace "is decreasing" by "fluctuates with fluctuating temperatures" [VINCENT GRAY (Reviewer's comment ID #: 88-2059)]	Rejected – long term behaviour is identified and explained
TS-1070	A	49:7	49:9	I agree that this is a robust statement, and for this reason would think it would require that IPCC project that the rate of sea level rise will accelerate from its present rate and allow a safety margin of a quite high rate of rise--but no, IPCC has gone the other way. Totally baffling. [Michael MacCracken (Reviewer's comment ID #: 152-185)]	Noted – but ice mass loss taken into account in sea level projections already
TS-1071	A	49:8	49:8	Replace "very likely" by "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-2044)]	(Presumably page 48?) Text edited for other reasons.
TS-1072	A	49:8	49:8	Qualify the last sentence to read: "The rate of mass loss from glaciers and the coastal margins of the Greenland ice sheet is increasing". [William Kininmonth (Reviewer's comment ID #: 128-77)]	Unnecessary addition
TS-1073	A	49:9	49:9	accelerating (rather than increases) [Roger Barry (Reviewer's comment ID #: 13-6)]	Disagree that this statement could be made at present
TS-1074	A	49:9	49:9	Insert after "is" "thought by some to be" [VINCENT GRAY (Reviewer's comment ID #: 88-2060)]	Rejected – all observational evidence assessed here
TS-1075	A	49:11	49:11	N.H. snow cover [Roger Barry (Reviewer's comment ID #: 13-7)]	Accepted
TS-1076	A	49:11	49:11	Insert at beginning, "Some". What about black carbon? [VINCENT GRAY (Reviewer's comment ID #: 88-2045)]	Black carbon is irrelevant to this statement
TS-1077	A	49:11	49:11	Add at end "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2061)]	Rejected – all observational evidence assessed here.
TS-1078	A	49:12	49:12	After "decreased" insert "over" [VINCENT GRAY (Reviewer's comment ID #: 88-2062)]	Accepted
TS-1079	A	49:14	49:14	Insert after "are", "thought to be" [VINCENT GRAY (Reviewer's comment ID #: 88-2046)]	Rejected – all observational evidence assessed here.
TS-1080	A	49:14	49:14	Change to "Since at least 1978" as these changes, I believe, go back further in time, even though records are not as extensive. [Michael MacCracken (Reviewer's comment ID #: 152-186)]	The robust statement depends on the satellite data – no change
TS-	A	49:17	49:17	Replace "very likely" by "possibly"	Rejected – no basis given

1081				[VINCENT GRAY (Reviewer's comment ID #: 88-2047)]	
TS-1082	A	49:20	49:20	again permafrost temperatures increases by up to 3C [Roger Barry (Reviewer's comment ID #: 13-8)]	Text edited
TS-1083	A	49:20	49:20	"The statement regarding the warming of permafrost regions should indicate that it is the shallow permafrost temperatures (upper 20 m) that have increased by up to 3°C and that this is largely the case for colder permafrost. The increase in permafrost temperatures has not be uniform and in some areas the increases have been small. It would be better to say that increases in shallow permafrost temperature have been observed since the 1980s throughout the permafrost zone of teh northern hemisphere." [Govt. of Canada (Reviewer's comment ID #: 2004-139)]	Accepted - text edited.
TS-1084	A	49:20	49:20	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2048)]	Rejected – no basis given
TS-1085	A	49:20	49:20	Change "warmed" to "have warmed" [Michael MacCracken (Reviewer's comment ID #: 152-187)]	Text edited.
TS-1086	A	49:20	49:20	The statement regarding the warming of permafrost regions should indicate that it is the shallow permafrost temperatures (upper 20 m, top of permafrost) that have increased by up to 3°C and that this is largely the case for colder permafrost. The increase in permafrost temperatures has not be uniform and in some areas the increases have been small to negligible. The statement should reflect the variability and not emphasize the extreme case. It would be better to say that increases in shallow permafrost temperature have been observed since the 1980s throughout the permafrost zone of the northern hemisphere. [Sharon Smith (Reviewer's comment ID #: 244-86)]	See TS-1083. Text edited in line with comment.
TS-1087	A	49:20	:21	The 3C statement is based on specific regions, this statement now extends it globally. Better to insert at start: In specific regions permafrost temperature have warmed etc. As for use of the word thinning, better to rephrase and say "less deep" [Govt. of United States of America (Reviewer's comment ID #: 2023-974)]	See TS-1083. Text edited.
TS-1088	A	49:23	49:33	"Another key uncertainty that could be added is that there are limited long-term records of permafrost temperature and some rather important spatial gaps in the monitoring networks." [Govt. of Canada (Reviewer's comment ID #: 2004-140)]	Noted – but will omit this because of space limitations.
TS-1089	A	49:23	49:33	Another key uncertainty that could be added is that there are limited long-term records of permafrost temperature and some rather important spatial gaps in the monitoring networks. [Sharon Smith (Reviewer's comment ID #: 244-87)]	See TS-1088
TS-1090	A	49:24	49:24	no global compliation [Roger Barry (Reviewer's comment ID #: 13-9)]	Accepted
TS-1091	A	49:24	49:24	Insert "global" before "in-situ." [Melinda Marquis (Reviewer's comment ID #: 162-144)]	See TS-1090
TS-	A	49:27	49:27	This statement is really unsatisfactory. There is, for example, no such single thickness--	Text edited.

1092				what one might observe is the global average of the change in sea ice thickness. One might say that there are insufficient observations to generate an estimate a change in sea ice thickness and volume, or something similar, but as phrased, this sets an impossible goal--no one ever observes "global sea ice thickness"--they observe it in places, and there are many such measurements. There is even a new satellite measuring sea ice free-board, so we should soon have such a data set--just not long enough. In addition, of course, in observing reduction in sea ice, we have some indications of where sea ice thickness has gone to zero, and how would that be accounted for? [Michael MacCracken (Reviewer's comment ID #: 152-188)]	
TS-1093	A	49:29	49:29	Better phrasing would be "Uncertainties in estimates of the loss of glacier mass arise from limitations in the global inventory of glaciers, ..." [Michael MacCracken (Reviewer's comment ID #: 152-189)]	Accepted
TS-1094	A	49:30	49:30	Delete "It is virtually certain that"--it makes no sense to use lexicon here if not used everywhere else, and it really generates an awkward phrasing to have it. [Michael MacCracken (Reviewer's comment ID #: 152-190)]	Accepted
TS-1095	A	49:37	49:37	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2049)]	Rejected – no basis given
TS-1096	A	49:37	49:37	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2063)]	Rejected – no basis given
TS-1097	A	49:38	49:38	Insert after "temperatures" "as measured solely by the surface record have risen since 1978" [VINCENT GRAY (Reviewer's comment ID #: 88-2050)]	Factually incorrect – see Chapter 5
TS-1098	A	49:38	49:39	Delete from "rise" in line 38 to end, in line 39. These figures are different for the different records and one example is unfair. [VINCENT GRAY (Reviewer's comment ID #: 88-2051)]	(Presumably page 48) The statement is true for different records – rejected.
TS-1099	A	49:38	49:38	Delete "It is virtually certain that" [VINCENT GRAY (Reviewer's comment ID #: 88-2064)]	Rejected – no basis given
TS-1100	A	49:38	49:38	Replace "increased" by "fluctuated" [VINCENT GRAY (Reviewer's comment ID #: 88-2065)]	Rejected – no basis given
TS-1101	A	49:38	49:38	Add at end "with a peak in 1980 and a value in 2005 of comparable magnitude" [VINCENT GRAY (Reviewer's comment ID #: 88-2066)]	Suggested text would be misleading – the later peak is higher than the earlier one.
TS-1102	A	49:38	49:38	Replace 'oceans' by 'ocean surface layer'. The data is to a depth of 700 m only. [William Kininmonth (Reviewer's comment ID #: 128-78)]	Data is to 3000 m, "ocean surface layer" would be very misleading. – no change.
TS-1103	A	49:40	49:40	Delete "It is very likely that" [VINCENT GRAY (Reviewer's comment ID #: 88-2067)]	Rejected – no basis given
TS-1104	A	49:40	49:40	If getting rid of lexicon, would seem could drop "It is very likely that" as this statement does not really draw any definitive conclusion, just restates some undisputed observations. [Michael MacCracken (Reviewer's comment ID #: 152-191)]	Accepted

TS-1105	A	49:41	49:41	Use "sub-polar" to replace "polar". [Govt. of China (Reviewer's comment ID #: 2006-24)]	Accepted
TS-1106	A	49:42	49:42	"increased salinity in the mid-latitudes and freshening on the equator" does not match Figure TS-20 or the text of Section TS.3.3.2: see also comment on page TS 26, line 51. [David Parker (Reviewer's comment ID #: 195-104)]	Text edited and is now consistent with the chapter
TS-1107	A	49:42		why freshening at the equator if precip has decreased as low latitudes(48:42) [David Rind (Reviewer's comment ID #: 214-14)]	Text corrected
TS-1108	A	49:42		Why freshening at the equator if precip has decreased as low latitudes? [Govt. of United States of America (Reviewer's comment ID #: 2023-975)]	See TS-1107
TS-1109	A	49:44	49:44	Delete whole sentence. "Warming " has not occurred before 1997 in two tropospheric records, so how could "warming" and "extremes" be related? [VINCENT GRAY (Reviewer's comment ID #: 88-2052)]	(Presumably page 48?) Rejected – factually incorrect.
TS-1110	A	49:44	49:44	Delete "It is virtually certain that" [VINCENT GRAY (Reviewer's comment ID #: 88-2068)]	Accepted
TS-1111	A	49:44	49:44	Delete "It is very likely that" [VINCENT GRAY (Reviewer's comment ID #: 88-2069)]	Rejected – the qualifier is significant here.
TS-1112	A	49:44	49:44	Delete "It is virtually certain that"--there is really no doubt of there having been a rise--just perhaps in its amount (and cause). [Michael MacCracken (Reviewer's comment ID #: 152-192)]	Accepted
TS-1113	A	49:45	49:45	Insert after "century" "as judged by tide gauges" [VINCENT GRAY (Reviewer's comment ID #: 88-2070)]	Unnecessary – methods are not elaborated in this section.
TS-1114	A	49:46	49:46	Insert after "2003" "after a change to satellite measurements" [VINCENT GRAY (Reviewer's comment ID #: 88-2071)]	Unnecessary – methods are not elaborated in this section.
TS-1115	A	49:46	49:47	Is it not correct to add "ice sheets" to this list given satellite data regarding Greenland? [Michael MacCracken (Reviewer's comment ID #: 152-193)]	Rejected – the ice sheet contribution is the smallest and has the largest uncertainty
TS-1116	A	49:52	49:52	Replace "much less complete" by "incomplete" [VINCENT GRAY (Reviewer's comment ID #: 88-2053)]	(Presumably page 48?) Unnecessary change.
TS-1117	A	49:52	49:52	Delete "spatially than surface records" [VINCENT GRAY (Reviewer's comment ID #: 88-2054)]	(Presumably page 48?) Unnecessary change.
TS-1118	A	49:52	49:53	Replace "a number of radiosondes" with "surface" [VINCENT GRAY (Reviewer's comment ID #: 88-2055)]	(Presumably page 48?) Rejected – suggested text would be highly misleading
TS-1119	A	49:53	49:53	Insert after "unreliable" "because of the influence of urban factors and poor calibration," [VINCENT GRAY (Reviewer's comment ID #: 88-2056)]	(Presumably page 48?) Rejected – no basis given
TS-1120	A	50:4	50:5	This item should add that there is, however, near consistency over the last decade--and at a higher rate. For the longer period, the data suggest, it seems, a "missing source"--so this is not reservoirs as their water content has been increasing: groundwater or deforestation are suggested as possibilities--and should we not think at least some contribution from this term	Text edited

				will continue. More likely, it seems to me, is that the missing term is from the Greenland or Antarctic ice sheets, and we just do not have measurements to show this--and only with satellite measurements do we have a real sense of what is going on. That IPCC seems to be continuing to place such credence in our past understanding seems a bit troubling given the missing terms--should IPCC not be drawing its best estimates from the recent observational history? [Michael MacCracken (Reviewer's comment ID #: 152-194)]	
TS-1121	A	50:4	50:4	Page TS-27 line 15 states that the discrepancy may be insignificant. [David Parker (Reviewer's comment ID #: 195-105)]	Accepted – text edited.
TS-1122	A	50:5	50:5	Add at end "so it is probably influenced by land subsidence near cities" [VINCENT GRAY (Reviewer's comment ID #: 88-2072)]	Rejected – no basis given
TS-1123	A	50:9	50:9	Del;ete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2073)]	Rejected – no basis given
TS-1124	A	50:10	50:11	I would suggest making it explicit that during the Eemian evidence indicates that about half of the Greenland Ice Sheet disappeared. What is intriguing is that the strong warming was in the Northern Hemisphere, yet the Antarctic Ice Sheet must also have lost mass for sea level to be higher. If strong heating of Antarctica did not cause this, might it have been the sea level rise generated by the loss of half of Greenland's Ice Sheet that did, and so we should be including the effect of sea level rise from global thermal expansion and from the melting of Greenland in projecting what is likely to happen in Antarctica (and not just that more snow may fall as it is warmer). Given all of this, it would be helpful to have a bit more paleoclimatic description included here. [Michael MacCracken (Reviewer's comment ID #: 152-195)]	Text edited for consistency with chapter.
TS-1125	A	50:10	50:10	"3 to 4 degrees C" needs to be reconciled with "2-4° C" at page TS-33, line 18 where it also needs to be clarified whether the warmth is relative to the present day rather than the previous glacial.. The caption of Figure TS-24 should also make this clear. [David Parker (Reviewer's comment ID #: 195-106)]	Temperatures removed here
TS-1126	A	50:10	50:11	In the SPM and underlying chapter it says that the Arctic was 2-4°C warmer than at present, not 3-4°C. [Andy Reisinger (Reviewer's comment ID #: 210-67)]	See TS-1125
TS-1127	A	50:13	50:14	Other than when there were strong and rapid outflows of glacial meltwater, is there any indication of the THC changing abruptly? The models suggest that we face gradual changes in the ocean circulation and not abrupt ones. This does not mean that the atmospheric circulation might not flip in an abrupt way into some mode that gives an abrupt change in many particular places, but adding them all up, it seems very unlikely (given the vast energy content of the oceans, particularly in the tropics) that an abrupt change in the world climate lies ahead. So, I think that this statement should make clear that the likely cause of the abrupt changes in the past was a driver that is very unlikely in the past--and it Greenland Ice Sheet creates a large enough pond to abruptly change the global ocean circulation, we'll have so much sea level rise that the weather shift will be small potatoes.	Noted – but no specific change to text suggested

				[Michael MacCracken (Reviewer’s comment ID #: 152-196)]	
TS-1128	A	50:16	50:17	What is meant by 'a natural interglacial climate cycle'? If predictions related to the earth's orbital parameters are implied then this should be stated, otherwise the sentence is confusing and potentially misleading. [William Kininmonth (Reviewer’s comment ID #: 128-79)]	Text edited
TS-1129	A	50:16	50:	Replace "There is no evidence for" by "Available evidence gives no indication of", and add "imply" after "or" [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-79)]	Text edited
TS-1130	A	50:21	50:23	I would suggest changing the last phrasing of the sentence to: "the warmest 50-year period in at least the past 1000 years, and possibly as far back as several thousand years when the Earth's orbital parameters were quite different." [Michael MacCracken (Reviewer’s comment ID #: 152-197)]	Text must remain consistent with chapter. No change.
TS-1131	A	50:23	50:23	Add at end "because of the warming effects associated with human habitation" [VINCENT GRAY (Reviewer’s comment ID #: 88-2074)]	Rejected – no basis given
TS-1132	A	50:25	50:26	The sentence confuses paleoclimate reconstructions with model projections. Delete the clause 'under a wide range of climate forcings' to make it clear that paleoclimate reconstructions are being referred to. [William Kininmonth (Reviewer’s comment ID #: 128-80)]	Accepted – model simulations text removed.
TS-1133	A	50:25	50:25	The statement needs to give the spatial scale of droughts--perhaps say "Subcontinental-scale droughts"--the present phrasing seems to imply that the whole of North America has suffered from long droughts, and this is not correct. [Michael MacCracken (Reviewer’s comment ID #: 152-198)]	Text has been edited.
TS-1134	A	50:27	50:27	add a new item, insert sentences at page 32 line 50-52, this is a quite important information. [Govt. of Germany (Reviewer’s comment ID #: 2011-248)]	Noted – but this aspect not regarded as sufficiently clear or robust for this section
TS-1135	A	50:32	50:33	The related statement on line 13 used "very likely" to even define their cause--saying "not well understood" here seems in direct conflict with the phrasing above--something has to give. I think the concluding sentence here is also too strong. While ice cores (e.g., from Greenland) indicate that the local temperature can change there very abruptly (and presumably also elsewhere as the hemispheric atmospheric circulation is linked, it is not at all clear that this means the global climate change can change by so large an amount so abruptly--there is a tremendous amount of heat in the oceans, and one cannot simply have this disappear (and ocean sediment cores really do not generally have the resolution to confirm an abrupt change, so maybe it is not the models that are the problem, but the data sets that we have--at least both aspects should be indicated as limiting our understanding. [Michael MacCracken (Reviewer’s comment ID #: 152-199)]	The statement is about the ability of models to simulate observations of abrupt change – independently of what the oceans might be doing. No change.
TS-1136	A	50:35	50:36	The sentence is not entirely true. Ice sheets rapidly disintegrate when surface temperature of the ice sheet exceeds freezing for substantial periods of summer (recognising that daily solar insolation over the poles at midsummer is as much as over the tropics) and summer melt exceeds winter snow accumulation. What is not known is the cause for elevation of	Rejected – if we knew the mechanisms we would be able to show simulations of them. But they are clearly absent in the literature.

				surface temperatures (ie, the concerted lift in the altitude of the atmospheric freezing level) in the past, such as occurred over the North American and Northern European ice sheets at the end of the last glacial maximum. The rates of change are certainly not well known. [William Kininmonth (Reviewer's comment ID #: 128-81)]	
TS-1137	A	50:35	50:36	While this is the case (though "well known" should be changed to "well established" as it is not as if we just need to publicize some hidden understanding), it should also be indicated here that there is quite good paleoclimatic evidence that ice sheets can disintegrate quite rapidly (e.g., end of the last glacial, entry into the Eemian, etc.). [Michael MacCracken (Reviewer's comment ID #: 152-200)]	See TS-1136
TS-1138	A	50:41	50:43	This statement seems a bit overstated to me--at the least indicate that it is the ones that use different proxies that differ (and this may because they are representative of different climatic measures--such as summer temperature change, annual temperature change, land only, etc.). Phrasing should also be changed to indicate that this problem is not unbounded--they are not all that far off, and none contradict the notion that humans are now having a significant impact on the global climate. [Michael MacCracken (Reviewer's comment ID #: 152-201)]	Text edited
TS-1139	A	50:41	50:43	This statement is very weak and needs work. I would say "Available millennial-length northern hemisphere temperature reconstructions have different amplitudes of temperature change (though all are within the reported uncertainties of the TAR report). The relationship between these differences and choices of proxy data or statistical calibration has not been definitively resolved. [Eric Steig (Reviewer's comment ID #: 252-27)]	Not quite true to say within TAR uncertainties. No change.
TS-1336	B	50:41	50:41	The key uncertainties for millennial climate are inaccurate proxies, over-confident reliance on inaccurate proxies, interdependence of studies through common proxies so that many studies stand or fall together and inaccurate statistical methods. Key potential proxy problems are bristlecones, inconsistent site chronologies together with biased choices (Yamal vs Polar Urals), use of uncalibrated or poorly calibrated proxies (Moberg), use of precipitation proxies for temperature reconstruction. Overfitting, inadequate model testing and use of calibration period residuals all lead to unrealistically narrow confidence intervals. [Stephen McIntyre (Reviewer's comment ID #: 309-118)]	Believe current text is appropriate for summary. See chapter for details.
TS-1140	A	50:46	50:46	Consider re-wording "of role of," e.g., "the influences exerted by." [Melinda Marquis (Reviewer's comment ID #: 162-145)]	Accepted
TS-1141	A	50:50	50:50	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2075)]	Rejected – no basis given
TS-1142	A	50:51	50:51	Replave "It is highly likely (>95%) that" by "The" [VINCENT GRAY (Reviewer's comment ID #: 88-2076)]	Rejected – the likelihood statement is a key product of the attribution studies.
TS-1143	A	50:51	50:51	Insert after "warming" , "of the surface as measured from weather stations and ships" [VINCENT GRAY (Reviewer's comment ID #: 88-2077)]	Rejected – the warming considered is broader
TS-	A	50:51	50:52	Replace "cannot be explainedforcing" by "was significantly influenced by	Rejected – no basis given

1144				socioeconomic factors related to human habitation" [VINCENT GRAY (Reviewer's comment ID #: 88-2078)]	
TS-1145	A	50:51	50:53	This statement is completely untrue. It ignores the findings of Trenberth and Stepaniak (2004) and the potential for increased poleward heat transport by the atmospheric circulation that produces a consistent heat, mass and momentum signature with the observed changes since the middle 1970s. The consistent response of tropical tropospheric temperatures to tropical SST changes associated with ENSO is well documented. McPhaden and Zhang (2002) have identified reduced upwelling and entrainment of cold subsurface water across the thermocline since the mid-1970s as an explanation for increased ocean surface layer heat content. [William Kininmonth (Reviewer's comment ID #: 128-82)]	Rejected – Attribution uses models with heat transport processes. The cited study is not inconsistent with RF being the cause of warming
TS-1146	A	50:51	50:53	In the first sentence, simply delete "It is highly likely (>95%) that" as not needed given what is done elsewhere--in any case, the lexicon says "very likely". There is really no significant disagreement with this--especially as it does not in making the statement "most of the warming is human-induced"--it rather uselessly instead is not quantitative. For the second sentence, I would rephrase to be more explicit: "It is very likely that greenhouse gas forcing has been the dominant cause of the warming as opposed to an increase in solar radiation or diminution in the cooling effects of volcanic aerosols." [Michael MacCracken (Reviewer's comment ID #: 152-202)]	See TS-1142
TS-1147	A	50:52	50:52	Replace ``external radiative forcing" by ``anthropogenic radiative forcing", since readers may think that ``external" means something outside the Earth (like change in solar radiation). [Michael Danilin (Reviewer's comment ID #: 55-14)]	Text edited for other reasons
TS-1148	A	50:52	50:52	clarify which external radiative forcing is meant [Govt. of Germany (Reviewer's comment ID #: 2011-94)]	Text edited
TS-1149	A	50:52	50:53	Delete from "It is highly likely" to end on line 53 [VINCENT GRAY (Reviewer's comment ID #: 88-2079)]	rejected – no basis given
TS-1150	A	50:52		again, GHG are not external forcing. [David Rind (Reviewer's comment ID #: 214-15)]	Rejected – see glossary definitions and consistent usage throughout report
TS-1151	A	50:55	50:56	Delete, as there is no supporting evidence [VINCENT GRAY (Reviewer's comment ID #: 88-2080)]	Rejected – no basis given
TS-1152	A	50:55	50:56	This statement is qualitative more, some) and should not be included under the heading of 'Robust findings'. [William Kininmonth (Reviewer's comment ID #: 128-83)]	Text edited – but direct comparative statements will be retained.
TS-1153	A	50:56	50:56	For clarity, I would suggest changing "with some warming" to "if not somewhat" [Michael MacCracken (Reviewer's comment ID #: 152-203)]	Text edited for other reasons
TS-1154	A	50:56	50:56	Consider re-wording, e.g., " ... years, had some warming not been offset by the cooling caused by natural and other anthropogenic factors, such as sulfate aerosols." [Melinda Marquis (Reviewer's comment ID #: 162-146)]	Text edited

TS-1155	A	51:2	51:2	Replace "likely" by "unlikely" [VINCENT GRAY (Reviewer's comment ID #: 88-2081)]	Rejected – no basis given
TS-1156	A	51:2	51:2	This statement gives little credence to the findings of McPhaden and Zhang (2002) that the upper ocean has warmed since the mid-1970s due to reduced upwelling and less entrainment of colder subsurface water across the thermocline. Additionally, it is highly implausible that slightly increased back radiation, that is absorbed at the surface, will be mixed downward against the naturally upwelling over most of the oceans arising from the MOC. This is not a robust finding. [William Kininmonth (Reviewer's comment ID #: 128-84)]	All potential causes of warming have been assessed
TS-1157	A	51:2	51:2	Delete "It is likely that"--there is no other suggested possibility of the right magnitude. This qualification is just not justified, especially given other statements (like the next one). [Michael MacCracken (Reviewer's comment ID #: 152-204)]	Rejected – likelihood is an output of attribution studies
TS-1158	A	51:2	51:2	Surely one could add "very" before "likely" here ? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-80)]	Accepted Text edited
TS-1159	A	51:4		A 'substantial' fraction is vague; it should be noted that a more precise determination is limited by our lack of knowledge of the past radiative forcing variations associated with both volcanoes and solar irradiance variations. [Govt. of United States of America (Reviewer's comment ID #: 2023-976)]	The point made is covered below under key uncertainties.
TS-1160	A	51:5	51:5	Replace "variability" by "changes" [VINCENT GRAY (Reviewer's comment ID #: 88-2082)]	Unnecessary
TS-1161	A	51:8		Make more explicit. What phenomena are being referred to here? [Richard Soulen (Reviewer's comment ID #: 248-31)]	Elaboration would be too detailed – see chapter section cited.
TS-1162	A	51:9	51:9	Add at end "of urban bias" [VINCENT GRAY (Reviewer's comment ID #: 88-2083)]	rejected – see chapter 3.
TS-1163	A	51:14	51:15	The statement should be more specific about key uncertainties. Suggest the statement be changed to read: " The range of attribution statements is limited by the absence of formal detection and attribution studies, or their very limited number, for some important phenomena (eg, ENSO, NAO, etc and some types of extreme events)". [William Kininmonth (Reviewer's comment ID #: 128-85)]	Suggested statement not justified – no change.
TS-1164	A	51:21	51:21	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2084)]	Rejected – no basis given
TS-1165	A	51:22	51:22	Delete "there is considerable confidence that" [VINCENT GRAY (Reviewer's comment ID #: 88-2085)]	Rejected – no basis given
TS-1166	A	51:22	51:22	Insert after "provide "some" [VINCENT GRAY (Reviewer's comment ID #: 88-2086)]	Unnecessary
TS-1167	A	51:22	51:25	'considerable confidence' is an unquantified term and should be replaced by terminology of Box TS 1.1. A suggested alternative is: "It is about as likely as not likely that models provide useful projections". The broadscale circulations of the atmosphere and oceans are based on accepted equations of motion but these are modified by	Standard terms can not be used because they would not apply to all aspects of simulation – text clarified.

				parameterisations for sub-grid scale phenomena for which there is considerable uncertainty and which differ between model, thus the statement is misleading. Suggest delete 'their foundation on accepted physical principles and'. [William Kininmonth (Reviewer's comment ID #: 128-86)]	
TS-1168	A	51:27	51:27	Replace "Confidence in models has increased due to" with "Models show" [VINCENT GRAY (Reviewer's comment ID #: 88-2087)]	Rejected – no basis given
TS-1169	A	51:38	51:38	An omission in the list of key uncertainties is the specification of important parameterisations that affect the energy flow through the climate system, especially the role of deep moist convection and the work done against atmospheric stratification by the buoyancy forces associated with deep tropical convection. [William Kininmonth (Reviewer's comment ID #: 128-87)]	The existing list of key uncertainties gives the implications of the uncertainties mentioned by the reviewer and this has more relevance in the TS than the suggested level of detail.
TS-1170	A	51:39	51:40	An example of model metrics that could be used is the poleward transport of heat by the atmospheric circulation as calculated by Trenberth and Caron (2001). Suggest adding a clause at the end of the sentence to read "... yet to be developed, for example, the zonal average poleward energy transport by the atmospheric circulation needed to achieve top of the atmosphere global radiation balance". [William Kininmonth (Reviewer's comment ID #: 128-88)]	Noted but the statement is intended to be much more general than the suggested text.
TS-1171	A	51:42	51:42	One of the sets of results (see caption for figure TS-26) used results from models (presumably 13) that had a drift of less than something like 0.2 C/century, so I presume viewed as pretty small. Yet this item says "most" models have a problem. It would help to be more specific--how big a problem and does this problem have any significant influence on their estimates of global warming, or is it just an issue for the modeling groups and not for the mainline results? [Michael MacCracken (Reviewer's comment ID #: 152-205)]	Statement has been clarified to indicate that the term has to be corrected. Figure TS-26 is not relevant to the identification of climate drift in the deep ocean.
TS-1172	A	51:44	51:45	I think it would be useful here to indicate that there is, however, some compensation across mechanisms as not all of the feedbacks are independent. While differences in resolution of mountain ranges are surely one systematic cause, most of the process-related differences likely arise, I believe, due to cloud-related processes, and then these differences create other ones. It might be added that most of the importance of this is likely to limit the accuracy of projecting how changes on subcontinental scales differ from larger scale average changes. Given all of this, I think a more nuanced statement is needed here, as this statement will be really abstracted and used to condemn all findings unless it is more carefully said. [Michael MacCracken (Reviewer's comment ID #: 152-206)]	text edited to be consistent with Chapter 8
TS-1173	A	51:48		how about ENSOs? [David Rind (Reviewer's comment ID #: 214-16)]	Believe that text is consistent with chapters.
TS-1174	A	51:48		How about ENSOs? [Govt. of United States of America (Reviewer's comment ID #: 2023-977)]	See TS-1173
TS-1175	A	51:50	51:50	Need to change "which is" to "that are" [Michael MacCracken (Reviewer's comment ID #: 152-207)]	Accepted

TS-1176	A	51:50	51:50	Change "which is linked to" to "and lead to". [David Parker (Reviewer's comment ID #: 195-107)]	See TS-1175
TS-1177	A	51:55	51:55	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2088)]	Rejected – no basis given
TS-1178	A	52:1	52:1	Replace "will continue to" with "may possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-2089)]	Rejected – no basis given
TS-1179	A	52:1	52:2	The first sentence needs to make clear that the Greenland Ice Sheet will be contributing to SL rise during the 21st century. There is virtually no chance that it will accumulate ice through the century--it is already in rapid retreat around some of the edges, and the Eemian makes clear warming leads to loss of ice. So, this needs to be said. The second sentence also needs some bounds established--or more explanation. There are limits to how geographically different the actual rise in sea level can be--and this global aspect needs to be distinguished from what happens along coastlines that move up and down. The second statement alone, is just inadequate. [Michael MacCracken (Reviewer's comment ID #: 152-208)]	Rejected – GIS contribution is not projected to be as large as others – see Chapter 10 – so does not deserve special mention.
TS-1180	A	52:4	52:4	Replace "would" by "may" [VINCENT GRAY (Reviewer's comment ID #: 88-2090)]	Rejected – no basis given
TS-1181	A	52:4	52:7	The first sentence is a nonsense statement because 'stabilisation of climate' implies energy equilibrium. Thus if the system is in equilibrium there is no source of additional energy to warm the oceans, raise sea-level and melt ice. Elimination of the Greenland ice sheet requires, primarily, a raising of the freezing level by an average of 1.5 km in summer (a warming of surface temperature by 10C) in order to bring plateau surface temperature above freezing and to take advantage of summer insolation to melt the ice sheet. These sentences are based on the false premise of 'commitment to future change' and should be deleted. A more realistic statement would refer to the continuing melting of lowland ice over the coastal margins of greenland and the surging of glaciers that each reflect an ongoing contribution to future sea level rise. [William Kininmonth (Reviewer's comment ID #: 128-89)]	Accepted – stabilisation of RF was intended
TS-1182	A	52:4	52:7	Saying that it would take 1000 years or more to eliminate the Greenland ice sheet may well be true, but an awful lot of deterioration can happen much more rapidly, and this statement basically ignores that possibility. I would urge a statement something like: "Virtually all emissions scenarios will lead to temperatures that are ultimately likely to lead to significant deterioration of the Greenland ice sheet, and scenarios resulting in a few degree warming are likely to lead to the melting of roughly half of Greenland over a period as short as a few centuries." [Michael MacCracken (Reviewer's comment ID #: 152-209)]	Rejected – no basis for providing such time scales exists in the assessment in Chapter 10
TS-1183	A	52:4	52:7	I think it would be better not to link this statement to SRES scenarios since SRES are only century-scale scenarios. It would be more robust to state the actual global average temperature threshold (3.1±1.6°C) for elimination together with the relevant time scale (1000 years or more). What might be useful though, if you want to link it to concentrations,	Rejected – the point is that under plausible emission scenarios the warming level required for destabilization of GIS is reached this

				is to give the estimated CO2-equivalent stabilisation concentration that would result in such an average warming level. But SRES isn't really relevant for a time scale of 1000 years. [Andy Reisinger (Reviewer's comment ID #: 210-68)]	century
TS-1184	A	52:6	52:6	Insert after "scenarios "is so extreme that itcould" [VINCENT GRAY (Reviewer's comment ID #: 88-2091)]	Rejected – no basis given
TS-1185	A	52:6	52:6	Delete "is likely to" [VINCENT GRAY (Reviewer's comment ID #: 88-2092)]	Rejected – no basis given
TS-1186	A	52:7	52:7	Replace "more" by "so" ? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-81)]	Accepted
TS-1187	A	52:11	52:13	Regarding specific editing, on line 11, I would urge changing "all" to "key" as it will be a long time before all processes are included; I would also encourage changing "may" to "could". More generally, this seems to be a very strong statement, suggesting that no models exist to do this--it seems to me more appropriate to suggest that models do not yet include all of the processes that could lead to large and rapid dynamical change ... It seems to me also a bit strange to be including this if it is solely about "discharge of ice into the ocean" as one can have relatively rapid deterioration in place as well that would put the water into the ocean instead of discharging everything as ice into the ocean. With regard to uncertainties in this area generally, I was surprised not to see something about limits on our knowledge of paleo-ice history. [Michael MacCracken (Reviewer's comment ID #: 152-210)]	First part accepted. Paleo ice history would belong in previous section
TS-1188	A	52:16	52:16	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2093)]	Rejected – no basis given
TS-1189	A	52:16	52:24	COMMENTS: add the more description of "equilibrium climate secsitivity" REASON: ECS have been calculated using atmosphere models coupled with non-dynamic "slab" ocean, i.e., models omitting all ocean dynamics. The numbers of ECS are higher than actual temperature change. Explanation on ECS is necessary for easy understanding. RECOMMEND: add the following sentence, "It should be noted, however, that the equilibrium climate sensitivity which assumes 50m-depth ocean will give higher temperature rise than what will actually happen." [Govt. of Japan (Reviewer's comment ID #: 2014-30)]	Noted – but the assessment of clim sensitivity takes account of the distinction between slab ocean model runs and the glossary definition of ECS. No change.
TS-1190	A	52:16	52:24	COMMENT: suggest describe the "transient climate response" REASON: TCR is evaluating in the more actual condition than ECS. Therefore, TCR is useful information about more real estimation of future temperature changes. RECOMMEND: add this phrase with "On the other hand, the range of transient climate responses among models is smaller than the range in the equilibrium climate sensitivity. The 90% confidence interval of TCR is estimated to be 1.2–2.4 °C,and the median for TCR is 1.8°C. TCR can better simulate what will actually happen than ECS. [9.4, 10.5, Box 10.2] [Govt. of Japan (Reviewer's comment ID #: 2014-31)]	noted – but inclusion of TCR expected to be confusing in this context – left to chapter and text where the difference with ECS can be covered. No change.
TS-	A	52:18	52:18	Add at end "but no proper statistical anlysis"	Rejected – factually incorrect

1191				[VINCENT GRAY (Reviewer's comment ID #: 88-2094)]	
TS-1192	A	52:20	52:20	Replace "a good" by "some:" [VINCENT GRAY (Reviewer's comment ID #: 88-2095)]	Rejected – factually incorrect
TS-1193	A	52:23	52:23	Replace "strongly supports" by "suggests" [VINCENT GRAY (Reviewer's comment ID #: 88-2096)]	Rejected – no basis given
TS-1194	A	52:23	52:24	Modeling and observation studies support an increase in atmospheric water vapour and amplification of the surface temperature increase in the high troposphere. The increase in atmospheric water vapour is likely associated with regions of atmospheric ascent and cloudiness, the latter reducing the effectiveness of water vapour as a positive feedback. The amplification of warming in the high tropical troposphere is a negative feedback as it effectively increases the emission to space in the water vapour bands and in the margins of the CO ₂ spectra where emission emanates in the high troposphere; these offsets some of the impact of the increased CO ₂ concentration. Suggest that the sentence be modified to read: "New observational and modelling evidence strongly supports increased water vapour and upper tropospheric lapse rate amplification as being important feedback mechanisms, although their signs and relative magnitudes are yet to be fully evaluated". [William Kininmonth (Reviewer's comment ID #: 128-90)]	Rejected – such tropospheric dynamics has been considered in the assessment.
TS-1195	A	52:27	52:27	Change opening of sentence to read: "Cloud specification and feedbacks are a". [William Kininmonth (Reviewer's comment ID #: 128-91)]	Text edited
TS-1196	A	52:28	52:28	Please clarify: Low clouds are the largest contributor to what? To cloud feedbacks? Or to inter-model differences? [Melinda Marquis (Reviewer's comment ID #: 162-147)]	Text edited
TS-1197	A	52:30	52:31	The cryospheric bullet seems strange here for several reasons. First, I'm not sure whether the melting of Greenland would have a big effect on climate sensitivity. But fact is that no GCM in AR4 has a dynamic model of Greenland, so that certainly does not contribute to the spread of model responses. Suggest avoiding Greenland here and something like 'The magnitude of the long term cryospheric and oceanic feedbacks remains uncertain.' Don't relate this to the spread of the models, since sensitivity calculations in GCMs are with slab models and don't even have a dynamic ocean or ice sheets. [Reto Knutti (Reviewer's comment ID #: 133-38)]	Text moved
TS-1198	A	52:35	52:35	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2097)]	Rejected – no basis given
TS-1199	A	52:35	52:35	I was surprised that this section did not even mention monsoons--they are so important to so many people on Earth they deserve a statement. [Michael MacCracken (Reviewer's comment ID #: 152-211)]	Monsoon change is region specific and can not be summarized accurately in this section – see other parts of the report.
TS-1200	A	52:36	52:36	The numbers suggest that this is the inter-scenario range of all-model-mean warming, rather than the range of individual models as indicated by the text. [Govt. of Finland (Reviewer's comment ID #: 2009-7)]	Text edited
TS-1201	A	52:36	52:37	The warming range of 0.64-0.7 for 2011-30 is certainly wrong and is much wider (see Fig. 10.3.2) This comes from the Exec. Summary of chapter 10, and I have no idea how it got in	Text edited

				there. [Reto Knutti (Reviewer's comment ID #: 133-39)]	
TS-1202	A	52:36	52:37	This is a very strong statement, and really deserves a lot more prominence. What surprises me a bit is that it is said elsewhere that changes in sulfate aerosol levels can have a large and rapid influence on the climate, and I thought there were significant differences in aerosol amounts, so I am surprised the range is so small and would urge really reconfirming this statement. [Michael MacCracken (Reviewer's comment ID #: 152-212)]	Noted – the statement is for SRES scenarios though
TS-1203	A	52:42	52:42	Delete "robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2098)]	Rejected – no basis given
TS-1204	A	52:42	52:44	I would think that this statement should make mention of potential changes in monsoons (does this apply to them--are they the "large-scale 'patterns'" being referred to, and if so, monsoons should be mentioned explicitly? I would also encourage mention that tropical cyclones are likely to become more intense. [Michael MacCracken (Reviewer's comment ID #: 152-213)]	See TS-1199
TS-1205	A	52:42	52:44	Change into: Changes in precipitation now show some robust large-scale patterns. Precipitation increases at high latitude, where the multi-model mean increase is larger than the inter-model standard deviations. With respect to the sign of the precipitation changes more than 75% of the models agree in many regions at low and mid-latitudes. It appears that generally the precipitation increases in tropical precipitation maxima, and decreases in low precipitation areas, but the magnitude of these precipitation changes is highly uncertain. [Govt. of Netherlands (Reviewer's comment ID #: 2016-32)]	Not clear why the extra detail is being suggested – too long for balance in this section.
TS-1206	A	52:42		tropical precip forecast to increase, opposite to what has been happening. [David Rind (Reviewer's comment ID #: 214-17)]	The statement is about tropical precip MAXIMA and not linked to observations. Fig TS-10 (new numbering) shows that there is insufficient observational data to do so
TS-1207	A	52:42		Tropical precip forecast to increase, opposite to what has been happening... This points to a serious problem with "robust findings" about projections that make no sense when compared with current trends – THIS NEEDS TO BE HIGHLIGHTED, and possibly the robustness reduced(?) [Govt. of United States of America (Reviewer's comment ID #: 2023-978)]	See TS-1206
TS-1208	A	52:46	52:46	Replace "As" by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-2099)]	Rejected – no basis given
TS-1209	A	52:46	52:49	In the first sentence, need to mention that ice sheets also generally lose mass. In the second sentence, change "ice reduces" to "ice extent decreases". I am baffled by the last sentence--how does permafrost change relate to the preceding sentence on sea ice--at least it looks to read that way. [Michael MacCracken (Reviewer's comment ID #: 152-214)]	Text edited
TS-	A	52:47	52:49	"It is not fair to classify this statement regarding a substantial reduction of permafrost in the	text edited

1210				upper layers of the soil as a robust finding. As mentioned in previous comments and those related to Chapter 10, this statement makes little sense and is based on the results of models that have some important limitations and therefore the conclusions drawn are questionable. A more appropriate statement would be that increases in thaw depth in the permafrost regions is predicted to occur in response to warming over the next century. The statement presented here and others related to it in the SPM and TS are misleading and imply that there will be a loss of permafrost over large areas which is not the case (see comments on Ch 10). It is also important to note that permafrost degradation will not have consequences everywhere and that the areas of concern are those where soils are ice-rich. "	
				[Govt. of Canada (Reviewer's comment ID #: 2004-141)]	
TS-1211	A	52:47	52:49	Cut this into two sentences. The accelerated reduction of sea ice and the reduction of permafrost are not connected. [Govt. of Finland (Reviewer's comment ID #: 2009-8)]	Text edited
TS-1212	A	52:47	52:49	It is not fair to classify this statement regarding a substantial reduction of permafrost in the upper layers of the soil as a robust finding. As mentioned in previous comments and those related to Chapter 10 (#58-69), this statement makes little sense and is based on the results of models that have some important limitations and therefore the conclusions drawn are questionable (Conclusion based mainly on a paper that appears to have had minimal peer review - see comment #59) . A more appropriate statement would be that increases in thaw depth in the permafrost regions are predicted to occur in response to warming over the next century. The statement presented here and others related to it in the SPM and TS are misleading and imply that there will be a complete loss of permafrost over large areas which is not the case (see comments on Ch 10). It is also important to note that permafrost degradation will not have consequences everywhere and that the areas of concern are those where soils are ice-rich. In fact there may be areas where larger increases in thaw depth may be experienced because earth-materials are ice-poor (have low latent heat requirements) and/or are materials with high thermal conductivity (such as bedrock of the Canadian Shield) but the consequences are negligible. [Sharon Smith (Reviewer's comment ID #: 244-88)]	See TS-1210
TS-1213	A	52:51	52:53	The last part of this should be re-worded - No model shows a collapse of the MOC by the year 2100 but an abrupt slowdown cannot be ruled out. (See earlier comments on TS.) [Meric Srokosz (Reviewer's comment ID #: 250-6)]	Text edited – language very carefully considered
TS-1214	A	52:51	:53	If not 2100 then when? [Govt. of United States of America (Reviewer's comment ID #: 2023-979)]	Text edited
TS-1215	A	52:52		Add after "No model shows a collapse" "if meltwater runoff from Greenland is not included". [Govt. of Germany (Reviewer's comment ID #: 2011-95)]	See TS-1213
TS-1216	A	52:52		"No model shows a collapse... if meltwater runoff from Greenland is not included". That is a crucial caveat.	See TS-1213

				[Stefan Rahmstorf (Reviewer's comment ID #: 206-38)]	
TS-1217	A	53:4	53:7	The robust finding from C4MIp is that the positive feedback leads to an additional increase in atmospheric CO ₂ (of 20 to 220 ppm by 2100) under the SRES A2 scenario. See Chapter 7 exec summary [Pierre Friedlingstein (Reviewer's comment ID #: 77-40)]	Noted but this section does not present numeric results.
TS-1218	A	53:4	53:5	add that the reduced carbon uptake capacity is a result of climate change, and not directly a result of rising CO ₂ (as the current wording implies) [Chris Jones (Reviewer's comment ID #: 120-7)]	Text edited
TS-1219	A	53:4	53:7	in addition to affecting emissions for stabilisation, mention that carbon cycle feedbacks amplify CO ₂ rises for a given set of emissions and could increase warming by a further 1 degree or so. [Chris Jones (Reviewer's comment ID #: 120-8)]	Text edited
TS-1220	A	53:5	53:6	Consider re-wording, e.g., "This feedback means a greater reduction of emissions is required to achieve a given atmospheric CO ₂ stabilization level." [Melinda Marquis (Reviewer's comment ID #: 162-148)]	Text edited
TS-1221	A	53:6	53:6	add after "level" "and could lead to as much as 1.2°C" the insertion is taken from page 45, line 11 [Govt. of Germany (Reviewer's comment ID #: 2011-249)]	Noted but this section does not present numeric results.
TS-1222	A	53:11	53:12	Re-word - The likelihood of a large abrupt change of the MOC in the 21st century and beyond cannot yet be assessed reliably. [Meric Srokosz (Reviewer's comment ID #: 250-7)]	Accepted - See TS-1213
TS-1223	A	53:20	53:22	The sentence currently implies that if all GCMs simulated ENSO equally well in the present-day, then there would be no variation in the climate change response. There is no evidence that this is the case. I suggest replacing "due to" with "possibly partly associated with". [Keith Williams (Reviewer's comment ID #: 290-2)]	Accepted
TS-1224	A	53:21	53:21	Should change "due to" to "due in large part to" as these are not the only factors. [Michael MacCracken (Reviewer's comment ID #: 152-215)]	See TS-1223
TS-1225	A	53:28	53:28	Replace THC with MOC? Is use of terminology of THC and MOC consistent throughout? [Meric Srokosz (Reviewer's comment ID #: 250-8)]	Accepted
TS-1226	A	53:30	53:30	add that there is strong confidence that the feedback is positive - the uncertainty is in the strength but not the sign. [Chris Jones (Reviewer's comment ID #: 120-9)]	Sign is dealt with above under robust findings
TS-1227	A	53:34	53:34	Del;ete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-2100)]	Rejected – no basis given
TS-1228	A	53:35	53:35	Replace "will very likkely" with "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2101)]	Rejected – no basis given
TS-1229	A	53:35		See previous comment. [Franklin SCHWING (Reviewer's comment ID #: 230-30)]	Seems irrelevant

TS-1230	A	53:37	53:37	Replace "will very likkely" with "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2102)]	Rejected – no basis given
TS-1231	A	53:37	53:43	It would be nice here, if not elsewhere, to say something about changes in monsoons. Also, it would be useful to mention projected changes in North America--at least for the west and southwest where so many people live. [Michael MacCracken (Reviewer's comment ID #: 152-216)]	Monsoon change is region specific and can not be summarized accurately in this section – see other parts of the report.
TS-1232	A	53:37	:46	Surprised by the level of confidence expressed here – i.e. very likely or 90-99%. This is the same level of confidence assigned to the fact that most of the warming of the last 50 years is mostly attributable to human activities. Are we really objectively as confident about regional precipitation projections of the future as we are about attribution of recent warming? [Govt. of United States of America (Reviewer's comment ID #: 2023-980)]	text revised
TS-1233	A	53:45	53:45	Replace "will very likkely" with "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2103)]	text revised
TS-1234	A	55:0	55:	Figure TS-1 Comment: This Figure is grossly misleading for two reasons. It stitches together different data sets derived from different sources using different methods, and thus wrongly implies the data sets are directly comparable. And, importantly, it ignores the severe limitations of the ice core data. Either delete Figure TS-1 and its caption or, preferably, (a) amend its caption to state the limitations of ice core data and (b) add another figure that shows the limitations of ice core data with additional text explaining what it shows. For example, the additional figure could be Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO2 changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389 This suggested additional Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric carbon dioxide concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO2) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO2 concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C3 plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO2 concentration (16–18). Identification of a CO2-sensitive gene involved in	See response to comments TS-118, TS-147 and TS-152 and similar comments by this reviewer

				<p>stomatal development in <i>Arabidopsis thaliana</i> demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO₂ changes at different time scales (14, 17–25).”</p> <p>It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firm in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO₂ and the recent rates of change to atmospheric concentration of CO₂ have repeatedly occurred in recent millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO₂ concentration and this permits the determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-49)]</p>	
TS-1235	A	55:0	55:	<p>Explanation for symbols used in graphic TS-1 is missing [Govt. of Germany (Reviewer’s comment ID #: 2011-224)]</p>	Figure redrawn and caption changed
TS-1236	A	55:0		<p>Fig. TS1: Axis labels are messed up. [Reto Knutti (Reviewer’s comment ID #: 133-18)]</p>	Figure redrawn and caption changed
TS-1237	A	55:0		<p>My PDF does not correctly show the units in the right Y axis of Figure TS-1 [Govt. of Spain (Reviewer’s comment ID #: 2019-145)]</p>	Figure redrawn and caption changed
TS-1238	A	55:1	55:1	<p>"Radiative Forcing" should be in Watts per meter squared in all figures [VINCENT GRAY (Reviewer’s comment ID #: 88-2105)]</p>	Not in lowest panel - Figure redrawn and caption changed
TS-1239	A	55:2	55:3	<p>Make the same changes in Figure TS-1 as in Figure SPM-1 (see above) and use ppmv and ppbv instead of ppm and ppb, respectively. [Michael Danilin (Reviewer’s comment ID #: 55-6)]</p>	Figure redrawn and caption changed. Ppmv and ppbv are incorrect.
TS-	A	55:3		<p>Figure TS-1: The large vertical line around 1600 in the fourth panel (presumably showing</p>	Figure redrawn and caption changed –

1240				forcing) seems totally out of context with the panels above, which is supposedly their sum. Are you sure this does not count some change in solar or volcanic forcing? Otherwise, that very unusual jump needs to be explained in the caption. [Michael MacCracken (Reviewer’s comment ID #: 152-217)]	negative spike in rate of change explained.
TS-1241	A	55:4	55:4	"Radiative Forcing" of carbon dioxide should increase logarithmically. The diagram seems to show almost linear. [VINCENT GRAY (Reviewer’s comment ID #: 88-2106)]	It is non-linear as required
TS-1242	A	56:0	56:	Figure TS-2 Comment: This Figure is grossly misleading for two reasons. It stitches together different data sets derived from different sources using different methods, and thus wrongly implies the data sets are directly comparable. And, importantly, it ignores the severe limitations of the ice core data. Simply, Figure TS-2 amounts to a set of lies. Either delete Figure TS-1 and its caption or, preferably, (a) amend its caption to state the limitations of ice core data and (b) add another figure that shows the limitations of ice core data with additional text explaining what it shows. For example, the additional figure could be Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO2 changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389 This suggested additional Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric carbon dioxide concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO2) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data This is a brief quote from the paper (see the paper for references): “The conventional iced-based concept of relatively stabilized CO2 concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C3 plants are often characterized by a plastic phenotype capable of consistent adjustment of numbers of leaf stomata in response to changes in ambient CO2 concentration (16–18). Identification of a CO2-sensitive gene involved in stomatal development in Arabidopsis thaliana demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO2 changes at different time scales (14, 17–25).” It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firm in the	See response to comments TS-118, TS-147 and TS-152 and similar comments by this reviewer

				<p>decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO₂ and the recent rates of change to atmospheric concentration of CO₂ have repeatedly occurred in recent millennia. The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO₂ concentration and this permits the determination of past atmospheric CO₂ concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO₂ concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO₂ concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouenberget al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-52)]</p>	
TS-1243	A	57:5	57:5	<p>This diagram is misleading as it does not show inaccuracies. There is no significant increase in the rate of change since 1988</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-2107)]</p>	Information on errors has been added to the caption.
TS-1244	A	57:5	57:6	<p>Change "global atmospheric CO₂ concentrations" to "the global atmospheric CO₂ concentration"</p> <p>[Michael MacCracken (Reviewer’s comment ID #: 152-218)]</p>	Accepted
TS-1245	A	58:1	58:3	<p>Dots have different colors in a. (white) and b. (red and black), without explanation given.</p> <p>[Govt. of Netherlands (Reviewer’s comment ID #: 2016-33)]</p>	Figure redrawn
TS-1246	A	58:1	58:3	<p>Figure TS-4: Its seems strange that the number of major international aerosols campaign studies and ground based monitoring sites differ in number and location between winter and summer.</p> <p>[Govt. of Netherlands (Reviewer’s comment ID #: 2016-34)]</p>	Caption revised to make clear the locations are for two different types of aerosol measurement.
TS-1247	A	58:6	58:7	<p>The assignment of the red and black dots used in graphic TS-4 is missing</p> <p>[Govt. of Germany (Reviewer’s comment ID #: 2011-225)]</p>	See TS-1246
TS-1248	A	59:0	59:	<p>The terms "cooling" and "warming" due to the radiative forcing should be added to the graphic</p> <p>[Govt. of Germany (Reviewer’s comment ID #: 2011-226)]</p>	This is covered adequately in the text – no change.
TS-1249	A	59:0		<p>SPM-2 and TS-5 figure needs more explanation in caption describing how forcing is defined, relative to what, and how natural and anthropogenic differentiated. Fig 3 from</p>	Disagree – reader expected to read accompanying text and refer to glossary

				IPCC 2001 was much more defined more carefully and accurately. [Govt. of United States of America (Reviewer's comment ID #: 2023-981)]	as cited by footnote definitions of radiative forcing. Not necessary to repeat all definitions in captions.
TS-1250	A	59:1	59:3	It is notable (surprising?) that the level of scientific understanding for pre-satellite-era solar forcing which is based on proxies and models has jumped from "Very Low" in the TAR, to "Medium" in the AR4 figure. This should either be explained and highlighted here, or corrected including in this Figure which appears 3 times. In addition, this contradicts Chapter 2, page 6, lines 27-28! [Haroon Kheshgi (Reviewer's comment ID #: 125-28)]	Accepted. Oversight – should have been Low. Corrected
TS-1251	A	59:2	59:3	I suggest not to show the tiny effect of contrail cirrus in Figure TS-5, since it is about two orders of magnitude smaller than other forcing showing in this Figure. [Michael Danilin (Reviewer's comment ID #: 55-11)]	There is considerable interest in this term and the fact that it is small is significant
TS-1252	A	59:3		Fig. TS-5: Radiative forcing figure: Need to add the timeframe for this Figure. RF since 1750? Also, would be nice to have the actual RF values in the Figure caption. [Govt. of Canada (Reviewer's comment ID #: 2004-195)]	See definition of radiative forcing in SPM footnote and in glossary.
TS-1253	A	59:3		Figure TS-5: As noted in other comments, I think the right column needs to be dropped in that it is not at all clear what these terms mean on a comparative basis, and the error bars shown on the main part of the chart suffice to give an indication of the importance of the remaining uncertainties--for example, while we may not understand contrail effects, they are so small that this really does not matter for this assessment, and so the "low" scientific understanding does not really matter. [Michael MacCracken (Reviewer's comment ID #: 152-219)]	Disagree – chapter 2 explains carefully the provenance of the terms.
TS-1254	A	59:5	59:5	Typo: should be "1-sigma" instead of "1-s". [Michael Danilin (Reviewer's comment ID #: 55-10)]	Text edited
TS-1255	A	59:5	59:5	This diagram should include ALL greenhouse gases and forcing agents. The omissions of water vapour and clouds with the excuse that they are "feedbacks" is done only to conceal their very large uncertainties. Also the uncertainties should be TWO standard deviations, not ONE. They should all be doubled [VINCENT GRAY (Reviewer's comment ID #: 88-2108)]	Water vapor change is a feedback and not a primary driver of climate change as explained in the text. Uncertainty ranges are now 90% confidence intervals.
TS-1256	A	59:5	59:5	Insert a missing parenthesis. [David Parker (Reviewer's comment ID #: 195-109)]	Text edited
TS-1257	A	59:8	59:10	As far as I know, several of the forcings in this table are affected by different time-scales, so, I think that the phrase in the caption "No CO2 timescale is given...single lifetime" must be reworded or explained further. I see no difference between CO2 and solar variability in this regard. [Govt. of Spain (Reviewer's comment ID #: 2019-146)]	Timescale information now only covered in text.
TS-1258	A	60:1	60:3	Figure TS-6: 10 and 1000 year GWPs may also contain policy relevant information, suggest to add these. [Govt. of Netherlands (Reviewer's comment ID #: 2016-35)]	Figure dropped for reasons of length
TS-	A	60:2	60:2	There is an inconsistency between CO2 emissions in Figures TS-6 (and Figure 2.28) and	Figure dropped for reasons of length

1259				the following text on page TS-7, line 3. Namely, if indeed the fossil fuel emissions rose from 6.5 to 7.2 Gt(C)/yr from 1999 to 2005, then the CO ₂ emissions from fossil fuel should be ~6.6 Gt(C)/yr in 2000 or $6.6 \times 42/12 = 23.1$ Gt(CO ₂)/yr. On the other hand, the CO ₂ emission rate shown in Figures TS-6 and 2.28 is equal to 27.6 Gt(CO ₂)/yr in 2000. These values should be reconciled. [Michael Danilin (Reviewer's comment ID #: 55-8)]	
TS-1260	A	61:1	61:3	Figure TS-7: For readability increase size both top left and right figures to full page width. [Govt. of Netherlands (Reviewer's comment ID #: 2016-36)]	Such issues will be considered in the layout step.
TS-1261	A	61:3		Figure TS-7: The values indicated during the war years should likely have a larger indication of uncertainty as quite large adjustments have typically been made and spatial coverage has been altered. That the war years are the most unusual period over the instrumental record and are also when we think the data are most suspect (so have made the largest adjustments) should be leading to some caution in using this period as a break point for trend analysis. That somehow nature knew to change its results right after WWII is really quite suspicious, especially given that when one looks at the precipitation during these years (see Figure TS-11) one does not see a significant anomaly. [Michael MacCracken (Reviewer's comment ID #: 152-220)]	Not supported by the literature
TS-1262	A	61:4	61:4	This is NOT a "linear" trend. No responsible statistician should draw a straight line through such irregular data. It is not evidence for a "trend" The separate parts of the record need to be considered on their own. It is surely obvious that the entire sequence could not be considered to have a single "cause". The pretence that the satellite record is compatible with the surface data is absurd. [VINCENT GRAY (Reviewer's comment ID #: 88-2109)]	Text made clear that the data did not follow a linear trend – this has been clarified in a redrawn figure.
TS-1263	A	62:3	62:3	Correct spelling of El Chichón to include accent on "o." -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-7)]	Copy -editing
TS-1264	A	62:3		Figure TS-8: It is strange that the range of estimates is greatest for the recent period when we have the most data. Is this really the case, or is this something that is a result of calibrating the curves together over some other period? [Michael MacCracken (Reviewer's comment ID #: 152-221)]	No specific change suggested
TS-1265	A	62:5	62:7	The identifiers A, B, C, and D should be added to the caption [Govt. of Germany (Reviewer's comment ID #: 2011-227)]	Accepted
TS-1266	A	62:5	62:5	This is a trick to try and cover up the very real difference between the surface record and the satellite and radiosonde records of the lower troposphere. The radiosonde record should be displayed separately, as it shows no change in temperature between 1958 and 2004, in direct conflict with the surface record. The satellite record shows no change from 1979 to 1997, also The uncertainties in the surface record are much greater than with the others because there is no indication of the bias that has been found by the recent statistical studies [VINCENT GRAY (Reviewer's comment ID #: 88-2110)]	The radiosonde record, as this reviewer notes elsewhere, is the least reliable. Other parts of the comment inconsistent with recent studies and literature.
TS-1267	A	63:5	63:8	Change to read: "Top: the structure of the Southern Annular Mode (SAM) ... Bottom: Seasonal values of the SAM index calculated from station data.	Figure dropped for reasons of length

				[Govt. of Netherlands (Reviewer's comment ID #: 2016-37)]	
TS-1268	A	63:5	63:8	Suggest to define SAM to reduce the risk of misunderstanding when figure is used in isolation. [Govt. of Netherlands (Reviewer's comment ID #: 2016-38)]	Figure dropped for reasons of length
TS-1269	A	63:5	63:6	Change sentence to: "Top: The structure of the SAM indicated by regression of seasonal 850hPa geopotential height anomalies on the SAM time series." [David Parker (Reviewer's comment ID #: 195-110)]	Figure dropped for reasons of length
TS-1270	A	64:0		The figure caption makes reference to T2 and T12. These terms need at least a little context, as far as saying, "These are IR channels on satellite X, instrument Y," and possibly specify wavelengths. [Govt. of United States of America (Reviewer's comment ID #: 2023-982)]	Caption has been revised to use avoid these terms along the lines suggested.
TS-1271	A	64:1	64:3	Figure TS-10: Suggest to define T2-T12 to reduce the risk of misunderstanding when figure is used in isolation. [Govt. of Netherlands (Reviewer's comment ID #: 2016-39)]	See TS-1270
TS-1272	A	64:3		Figure TS-10: It is not clear what "T2-T12" means. This should be explained. It would also be useful to add pointers to when major El Nino events occurred so that the major jumps are indicated as related to those events and not just random variability. [Michael MacCracken (Reviewer's comment ID #: 152-222)]	See TS-1270
TS-1273	A	65:0		The upper panels of this figure need to be larger. [Govt. of United States of America (Reviewer's comment ID #: 2023-983)]	Layout issue
TS-1274	A	65:3		Figure TS-11: The bottom graph is really rather strange and potentially misleading. It is apparently the anomaly in mm, which would mean that it is dominated by fluctuations in areas where there is heavy rain. And this focuses only on land areas (and areas where there are data), and there is really no error bar given, and this needs to be done as measurements of convective rains are notoriously variable and so it is not at all clear from this diagram if this is real or noise. It might help if the baseline value around which anomalies are taken is given--so how much precipitation is occurring over land? It would also be nice to have affiliated with this a graph of the variations in aerosol amounts over land over this period--it almost looks as if the amount of sulfate loading would have a high correlation. With respect to the top two diagrams, why have one with a rate of per century and one with a rate of per decade--how can I then compare the graphics? In addition, why are the top graphs in per cent and the bottom one in absolute amount--how are they to be related? [Michael MacCracken (Reviewer's comment ID #: 152-223)]	The different estimates provide the best indication of uncertainty. The reasons for change are not to be simplified and linked to just aerosols, as changes in atmospheric circulation matter more. The comment is correct that the top figures in % make sense and allow local areas to be shown, but the bottom figure is dominated by tropics. Other figures are in chapter 3.
TS-1275	A	65:8	65:8	Replace "1961-90" with "1961-1990" just to be consistent with the rest of the years that appear in the text [Govt. of Spain (Reviewer's comment ID #: 2019-84)]	Accepted
TS-1276	A	66:0		The lower panel seems to be missing from this figure. [Govt. of United States of America (Reviewer's comment ID #: 2023-984)]	Caption and figure corrected
TS-1277	A	66:3		Fig TS-12: "Upper panel - assume the white areas are areas with no data? Should be specified in the Figure caption. Lower panel is missing."	Caption and figure corrected

				[Govt. of Canada (Reviewer's comment ID #: 2004-143)]	
TS-1278	A	66:5	66:11	Caption of Figure TS-12. In line 5, change "for" to "in"; in line 7, insert "the contribution from" before "very wet days"; in lines 8-12 either delete the text or ensure that the "lower" panel referred to is included in the Figure. [David Parker (Reviewer's comment ID #: 195-111)]	Caption and figure corrected. Second suggestion would be incorrect.
TS-1279	A	66:7	66:7	Replace "1961-90" with "1961-1990" [Govt. of Spain (Reviewer's comment ID #: 2019-85)]	Accepted
TS-1280	A	66:8	66:11	The figure TS 12 caption describes a lower panel which does not exist on this figure (contrary to figure 3.8.2). [Govt. of France (Reviewer's comment ID #: 2010-129)]	Caption and figure corrected
TS-1281	A	66:8	66:11	The lower curve mentioned in the caption seems to be missing. [Michael MacCracken (Reviewer's comment ID #: 152-224)]	Caption and figure corrected
TS-1282	A	66:8		There seems to be a missing lower panel in this figure (TS-12). A lower panel is discussed which shows regions where changes in heavy precipitation were documented. But this lower figure is missing from my version. [Terrence Joyce (Reviewer's comment ID #: 122-9)]	Caption and figure corrected
TS-1283	A	71:0		Figure TS-17: Is latent heat energy (from melting of ice) included? [David Parker (Reviewer's comment ID #: 195-112)]	Yes. Caption revised to be more explicit about the heat terms included in each bar.
TS-1284	A	71:3		Fig TS-17: "Apologies for poor understanding of physics here, but...line 8 of text for TS-17 says: ""Positive energy content change means an increase in stored energy (or heat in the oceans)."" My question is: is stored energy measured some other way (other than heat) in other parts of the climate system? The sentence suggests that storing energy as heat only applies to the oceans." [Govt. of Canada (Reviewer's comment ID #: 2004-142)]	Noted – but we think the text is clear.
TS-1285	A	71:3		Figure TS-17: Does the change in continental heat content include the change in energy due to the 3 C warming of permafrost mentioned in the text; if so, this should be made clear? Is there any account for the warming of the ice sheets that I believe has been occurring? [Michael MacCracken (Reviewer's comment ID #: 152-225)]	No. This estimate is derived from borehole data. See TS-1283, caption now makes this clear
TS-1286	A	71:6	71:6	This diagram is deliberately misleading as it does not take into account the fluctuations in ocean heat content. The current figure is approximately the same as it was in 1980. Previous measurements only started in 1955 [VINCENT GRAY (Reviewer's comment ID #: 88-2111)]	Rejected – uncertainties are indicated and the period chosen are determined by available studies of ocean heat content.
TS-1287	A	72:8	72:8	Again, no responsible statistician would draw a straight line through such irregular, fluctuating data. Any "trend" is meaningless. It seems highly probable that ocean heat content fluctuates over periods greater than the current measurements [VINCENT GRAY (Reviewer's comment ID #: 88-2112)]	A linear trend was not drawn through the data. Caption has been revised and reference to trend removed.

TS-1288	A	73:1	73:3	Figure TS-19: Explain units and pattern. [Govt. of Netherlands (Reviewer's comment ID #: 2016-40)]	Figure removed for reasons of length
TS-1289	A	73:5	73:5	Correct "Inventory of anthropogenic carbon (mol/m2) for the year 1994" for "Inventory of anthropogenic carbon uptake by oceans (mol/m2) for the year 1994." The legend is confusing without this clarification. [Govt. of Hungary (Reviewer's comment ID #: 2012-23)]	Figure removed for reasons of length
TS-1290	A	73:5	73:5	The first sentence of the caption of Figure TS-19 should mention that 1994 was the date of a series of ocean cruises--otherwise the question might arise of why such old data are being used. [Michael MacCracken (Reviewer's comment ID #: 152-226)]	Figure removed for reasons of length
TS-1291	A	74:1	74:3	Figure TS-20: Explain salinity change unit. [Govt. of Netherlands (Reviewer's comment ID #: 2016-41)]	Accepted
TS-1292	A	74:5	74:5	Another "linear trend" How irregular were these data? [VINCENT GRAY (Reviewer's comment ID #: 88-2113)]	Trend is only used as a statistical summary here.
TS-1293	A	75:0		Fig. TS21: Please indicate the quantity on the vertical axis, not just the unit. I think this should be made mandatory for each figure in the report, such that when a figure is reproduced in a talk, it is clear what is shown even if there is no caption. [Reto Knutti (Reviewer's comment ID #: 133-23)]	Accept, revised
TS-1294	A	75:1	75:3	Figure TS-21: Suggest to add information on uncertainty [Govt. of Netherlands (Reviewer's comment ID #: 2016-42)]	Rejected, the figure already has errorbars, but the figure is going to be improved so that they are much more visible.
TS-1295	A	75:5	75:7	Fig. TS-21. What is the reference level (0)? Any particular reason why 1940 - 45 is considered the base case (or 0)? [Govt. of Hungary (Reviewer's comment ID #: 2012-24)]	Taken into account: The reference level will be given in the caption.
TS-1296	A	76:3	76:3	Yet another unjustified "linear trend" [VINCENT GRAY (Reviewer's comment ID #: 88-2114)]	Trend is only used as a statistical summary here.
TS-1297	A	76:3		Figure TS-22 (bottom): I think it is a bit misleading to show the spatial pattern in the bottom figure without then saying in the caption that the regional pattern is not necessarily indicative of a long-term trend--that is, one would not expect the ocean levels off the west coast of North America to just keep dropping and dropping (in fact, I would guess that the pattern looks the way it does because of the 1997-98 El Nino, and that these variability effects have not yet averaged out--this only occurring when one adds up things for the whole ocean. Thus, I am not at all sure what the purpose is of including the bottom panel. perhaps it would be useful to give a sense of rise in each major ocean basin, but even that might not provide enough averaging. [Michael MacCracken (Reviewer's comment ID #: 152-227)]	Taken into account. Caption revised and reference to longer term patterns in Chapter 5.
TS-1298	A	77:0		Figure TS-23. Symbolson maps appear not to match the caption, e.g. red symbols indicating thermometers appear on the AD 1000 map. [David Parker (Reviewer's comment ID #: 195-113)]	Caption corrected and figure simplified.

TS-1299	A	77:7	77:7	The increased temperature after 1910 is obviously due to the proximity of the instruments to human habitation, not to greenhouse gas increases. [VINCENT GRAY (Reviewer's comment ID #: 88-2117)]	Rejected – no basis given
TS-1300	A	79:5	79:5	Another misleading diagram. Where it has been possible to correct the surface figures by "homogeneity adjustmen' (USA and China)t the supposed "warming" all but disappers [VINCENT GRAY (Reviewer's comment ID #: 88-2118)]	See Ch 3. Urban stations have been removed.
TS-1301	A	80:4	60:4	I do not believe this diagram because I do not think it is possible to simulate "natural" forcing since 1900 without taking into account urban influences and even the "recovery from the little ice age" which was postulated in the 1990 IPCC Report. In particular I do not believe that the temperature rise of 0.4°C between 1910 and 1942 can be explained exclusively by changes in the sun and volcanoes.. At least this diagan proves that models based exclusively on increases in greenhouse gases cannot possibly be used to forecast future climate. [VINCENT GRAY (Reviewer's comment ID #: 88-2119)]	Urban forcing would not be a natural forcing? Logic here seems confused. No specific change suggested
TS-1302	A	80:11	80:12	This statement in the caption needs a bit more expansion--do the models mentioned have natural external forcings or is this the trend when there is absolutely no external forcing, natural or human? [Michael MacCracken (Reviewer's comment ID #: 152-228)]	usage of anthropogenic and natural forcings is consisent throughout the report – details are in chapters 2 and 9.
TS-1303	A	81:5	81:5	"Trends" again!, I wonder how regular these were? [VINCENT GRAY (Reviewer's comment ID #: 88-2120)]	And again trends are only used as a statistical summary of the data
TS-1304	A	82:5	82:5	Here we go again. The poor hemispheric coverage of the "proxies" is ignored and the obvious influence of proximity to human habitation for the readings after 1900 is ignored [VINCENT GRAY (Reviewer's comment ID #: 88-2121)]	Figure removed for reasons of length
TS-1305	A	82:12	82:13	Delete sentence "The region of overlapping..." as it appears also in lines 16-17. [David Parker (Reviewer's comment ID #: 195-114)]	Figure removed for reasons of length
TS-1306	A	83:0		Fig. TS29: Is aligning everything at 1990 the best way to present this figure? Or would it be better to align the mean 1990 to 2000? Also, although the projections seem to be aligned at 1990, the vertical scale is not zero there, i.e. the anomalies are probably given relative to some other period, which is not given in the caption. Also, on the vertical please indicate the quantity, not just the unit, e.g. global surface warming (°C). [Reto Knutti (Reviewer's comment ID #: 133-22)]	Alignment at 1990 retained as easier to explain and fairly arbitrary. Data sources and baselines now explained in caption. Axis labels will be improved in final form.
TS-1307	A	83:3		Figure TS-29: It needs to be made clear that the purple curve (representing FAR) was for simulations without sulfate aerosol effects, so would be expected to be higher) and the green curve (representing the SAR) was a situation where aerosol loading was held constant over time (at a relatively high level) and so this is sort of a lower bound. Making these aspects clear in the caption should help readers understand that the reasons the IPCC projections have changed, and hopefully one can say the newer results are for the case with time-varying aerosols. [Michael MacCracken (Reviewer's comment ID #: 152-229)]	These and several other factors underly the differences shown between different assessment reports and the level of detail required to explain them can only be included in the chapter – see Chapter 1 as ctied.
TS-	A	83:3		Figure TS-29: I am unclear why the commitment simulation has such large variability.	Caption clarified to note that these are

1308				Were there a different number of simulations averaged to get this curve? While it is nice to show actual results, are not the variations in all of the runs a result of running only a limited size ensemble--or are there other reasons for the variations? [Michael MacCracken (Reviewer's comment ID #: 152-230)]	model ensemble means but the details can only be given in the chapters – see Chapter 10 as cited.
TS-1309	A	83:5	83:5	The models are for the lower troposphere, and you insist on comparing them with the unreliable surface record. But the troposphere measurements suggest negligible warming. [VINCENT GRAY (Reviewer's comment ID #: 88-2122)]	Models simulations are for surface temperatures. Rest of comment factually incorrect
TS-1310	A	83:12	83:12	Caption TS29: To be consistent with 10.7, this should be termed constant composition commitment, not forcing commitment. [Reto Knutti (Reviewer's comment ID #: 133-21)]	Accepted. Chapter 10 now using “committed warming” as an alternative term to the Wigley definitions and this used consistently in the TS and SPM.
TS-1311	A	84:5	84:5	Further evidence that scenario A2 is stupid. But who believes any of them?. None of them agree with what actually happens in the climate [VINCENT GRAY (Reviewer's comment ID #: 88-2123)]	Rejected – no basis given
TS-1312	A	84:7	84:7	Caption TS30: The terminology of commitment here is consistent with Box. TS 5.2 but inconsistent with the terminology in 10.7. In 10.7 climate change commitment is used as the overarching term for constant composition commitment, constant emission commitment, and zero emission commitment. Using different definitions will only confuse the reader and I see no reason to introduce a new terminology in the TS and not to stick with the definitions used in 10.7 [Reto Knutti (Reviewer's comment ID #: 133-24)]	Text will be revised and made consistent
TS-1313	A	85:0		The top left panel does not show emissions but cumulative emissions. The TS team requested a figure showing emissions and that was provided by me (Reto Knutti) in Feb. 2006. Please decide what you want, and make the figure consistent with the caption. [Reto Knutti (Reviewer's comment ID #: 133-26)]	Accepted: Panel replaced to match caption
TS-1314	A	85:0		Figure TS-31. I'm somewhat troubled by this figure because it seems to exclude ice sheet contributions. It would be helpful to clarify in the caption the specific assumptions and exclusions, and also perhaps to state the implicit climate sensitivities of the models used in these runs. It should also be stated whether the changes are relative to 1990, or to 1900; it's not clear because of the large scale of the x-axis. [Andy Reisinger (Reviewer's comment ID #: 210-69)]	Climate sensitivities are listed in table of Ch8. Caption is clear that we are only showing thermal expansion. Given that the associated text deals with ice sheet contributions to sea level separately and notes the lack of quantitative projections we feel the context should be clear.
TS-1315	A	85:5	85:5	Caption TS31: The terminology of commitment here is consistent with Box. TS 5.2 but inconsistent with the terminology in 10.7. In 10.7 climate change commitment is used as the overarching term for constant composition commitment, constant emission commitment, and zero emission commitment. Using different definitions will only confuse the reader and I see no reason to introduce a new terminology in the TS and not to stick with the definitions used in 10.7 [Reto Knutti (Reviewer's comment ID #: 133-25)]	Text will be revised and made consistent
TS-	A	86:1	86:7	I question the assignment of likelihood to the calculations since this implies the assessment	Rejected: Level of detail that belongs

1316				that each estimate is a complete estimate of uncertainty, and this is stated on page 48 (chapter 10) not proven. Specifically I refer to the caveat on lines 36-38 of that page which states "The potential for missing or inadequately parameterised processes to broaden the simulated range of future changes in not clear, however, it is an important caveat on the results discussed below." Suggest removing the likelihood indicators, and including a discussion of the assumptions needed to take this step. [Haroon Kheshgi (Reviewer's comment ID #: 125-48)]	into Ch10 but not into the TS.
TS-1317	A	86:5	86:7	This figure caption is not understandable. For example what is meant by individual? I expect that this means an individual study that was chosen for inclusion. Suggest that the basis for the calculations, be stated in caption. For example, one color could be used for each of the sets of data from the panels in Figure 10.2.1. I am not sure what is meant by the green curves? [Haroon Kheshgi (Reviewer's comment ID #: 125-35)]	Accepted: <i>individual</i> deleted
TS-1318	A	86:5	86:5	In the caption to Figure TS-32, what is meant by "individual"? I may be dense, but I think the jargon here needs to be better explained to understand what these curves are and why they are shaped as they are--at least, for me, this is quite unclear. [Michael MacCracken (Reviewer's comment ID #: 152-231)]	Accepted: <i>individual</i> deleted
TS-1319	A	87:0		It was emphasized many times that WG1 is not supposed to and not qualified to discuss the likelihood of scenarios and not qualified to make a selection of scenarios. Showing only the six illustrative SRES marker scenarios and ignoring the other 29 or so completely in the whole report is a judgment of preference of some scenarios against others, which I thought we are not qualified to make. I don't see any reason not to show the whole range of scenarios, and I think providing the full information is less likely to draw criticism that only providing part of the picture. [Reto Knutti (Reviewer's comment ID #: 133-27)]	Rejected – the illustrative marker scenarios were carefully chosen by the SRES authors to be representative.
TS-1320	A	87:3		Figure TS-33: This figure appears to be using units for emissions that are not used elsewhere in the report. It would really help here to have a double scale showing cumulative GHG emissions in units of TtCeq rather than TtCO2eq, which is a term the UNFCCC community uses, but not generally IPCC WG I. [Michael MacCracken (Reviewer's comment ID #: 152-232)]	Accepted - Axis will be changed
TS-1321	A	87:5	87:5	You can forget the absurd A2 and A1F1, but the others are't much either. Still we might only get 2 degrees. [VINCENT GRAY (Reviewer's comment ID #: 88-2124)]	Rejected – no basis given
TS-1322	A	87:7	87:7	eleven AOGCMs' should be '19 AOGCMs' [Sarah Raper (Reviewer's comment ID #: 208-3)]	Figure has been substantially revised and this part of the caption dropped. Details are in chapter 10 as cited.
TS-1323	A	88:0		Caption TS34: Please make it more clear that the two parts of the figure come from very different studies and models. Add AOGCM to description of the right part, and 'models of different complexity, partly constrained by observations' to the left part. The idea to combine the two figures is interesting, but there is a danger that the data shown will be	. Figure has been revised to use consistent time periods. Caption states explicitly that the PDFs are for “different” studies and readers will be

				interpreted as coming from the same models, whereas the two have very different origins, and are not even necessarily consistent with each other, because the AOGCMs span a more narrow range of projections than the observationally constrained ensembles with simpler models. Also, the time periods need to be consistent in the figure. Changing the right part to a single decade is easier since the maps are based on new data, while the PDFs are from various papers. I realize that a decade of averaging is normally a bit short for the map, but since this is a multi model average over twenty or so models and probably sixty or so ensembles, the noise is averaged out anyway, so a decade should be fine. [Reto Knutti (Reviewer's comment ID #: 133-28)]	aware that many figures in the TS synthesize material in this way so this should not be confusing.
TS-1324	A	88:5	88:5	It is not happening this way. But who cares? [VINCENT GRAY (Reviewer's comment ID #: 88-2125)]	Rejected – no basis given
TS-1325	A	89:9	89:9	"...average observed sea-ice concentration limit. Model results are based on..." [David Parker (Reviewer's comment ID #: 195-115)]	Figure dropped for reasons of length
TS-1326	A	90:1	90:4	Why are ocean regions not considered as regional panels? I would suggest the inclusion of a panel for the tropical North Atlantic, given its prominence in discussions of Atlantic tropical cyclone genesis. [Michael Mann (Reviewer's comment ID #: 156-54)]	Rejected. If we did this would need to do all ocean basins and supporting d&a research has not yet been done for all basins.
TS-1327	A	90:5	90:5	Again, if you substitute the records that have been "homogeneity adjusted" for the USA and for China most of the "warming" disappears. The same would happen if you carry out the same process for the other continents [VINCENT GRAY (Reviewer's comment ID #: 88-2126)]	See Ch3 – urban effects have been removed.
TS-1328	A	90:6		In fact the observed period is 1906-2005. [Daithi Stone (Reviewer's comment ID #: 256-55)]	Caption corrected
TS-1329	A	90:9	90:10	"the average 5th-95th" would be more accurate than "the 5th-95th" [Daithi Stone (Reviewer's comment ID #: 256-56)]	Caption clarified
TS-1330	A	91:0		Fig. TS37: Why is the bottom part in black and white? Couldn't all the panels be in the same color scheme and also the same map projection? [Reto Knutti (Reviewer's comment ID #: 133-29)]	Figure redrawn and this issue addressed.
TS-1331	A	92:0		Comment on Figure 1. This figure is somewhat misleading. The earth's orbit is shown as too elliptical, and the sun is shown as being too near the center of the ellipse rather than at a focus unless the earth's orbit is intended to be on a plane with respect to the plane of the paper. If this is the case a coordinate system might help illustrate the idea that the earth's orbit is at an angle to the plane of the paper. In addition the figure shows that the eccentricity of the orbit is changed without changing the locations of the perihelion or the apohelion. Also the figure does not show either the precession of the perihelion of the earth's orbit or the time variation of the tilt of the earth's orbit with respect to the plane of Jupiter's orbit. [Wilmer Anderson (Reviewer's comment ID #: 5-66)]	Noted. Indeed this was meant as a perspective view of the orbit from diagonally above - the figure has now been modified to make this clear. The figure only shows relative position of Earth and sun - no changes relative to "absolute space" or Jupiter's orbit, as these do not affect climate.
TS-1332	A	92:2	92:3	The inner orbit (dashed line) in the figure should cross the outer (solid line) orbit to be correct. Fix.	Rejected. Not justification given. Comment by Marie-France Loutre to the

				[Haroon Kheshgi (Reviewer's comment ID #: 125-30)]	previous draft said just the opposite, i.e., only the minor axis of the ellipse changes, while the major axis remains fixed, so that's how we prepared the figure.
TS-1333	A	93:3	93:4	Pretty short caption. [Govt. of Germany (Reviewer's comment ID #: 2011-228)]	Figure is an integral part of the box it illustrates and dealt with in the accompanying text.